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Transactions of the American Medical Association

American Medical Association,
Joseph Meredith Toner

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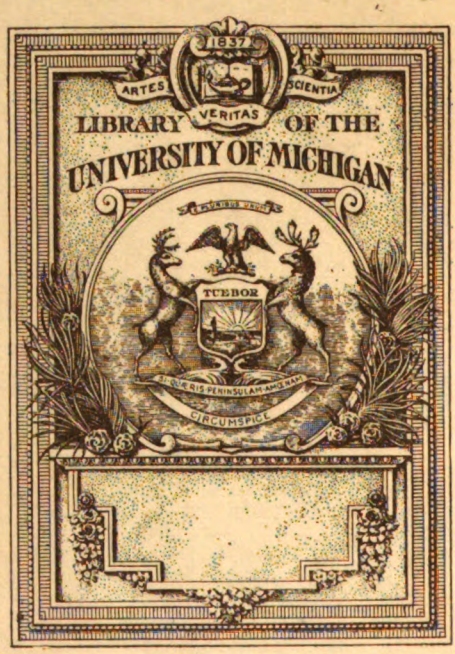

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THE

TRANSACTIONS

OF THE

AMERICAN

MEDICAL ASSOCIATION.

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INSTITUTED 1847.

VOL. VI.

PHILADELPHIA:
PRINTED FOR THE ASSOCIATION,
BY T. K. AND P. G. COLLINS.
1853.

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By a Resolution passed at the Session of 1851, the Committee of Publication were instructed to print conspicuously, at the beginning of the volume of the *Transactions*, the following disclaimer:—

"The American Medical Association, although formally accepting and publishing the Reports of the various Standing Committees, holds itself wholly irresponsible for the opinions, theories, or criticisms therein contained, except when otherwise decided by special resolution."

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MINUTES

OF THE

SIXTH ANNUAL MEETING

OF THE

AMERICAN MEDICAL ASSOCIATION,

HELD IN THE CITY OF NEW YORK, MAY, 1853.

New York, May 3, 1853.

THE Association met in the "Bleecker St. Presbyterian Church," at 11½ o'clock A.M.; the President, Dr. BEVERLEY R. WELLFORD, in the chair.

Dr. F. CAMPBELL STEWART, Chairman of the Committee of Arrangements and Reception, congratulated the Association upon the recurrence of its anniversary. "Seven years," said he, "have elapsed since the preliminary meeting of the Convention which recommended the organization of this National Congress, assembled here; and as New Yorkers we indulge in a feeling of proud satisfaction at the triumphant success which has attended so important a movement originating in our State."

He spoke of the arduous labours of the Association, the mighty objects aimed at, the "achievements already accomplished," and the "guarantee afforded of the eventual fulfilment of the desires of those whose aspirations for the advancement and perfection of medicine led them to propose the formation of this Association." And concluded by saying:—

"Your advent has been anxiously looked for by us, and your colleagues here have been desirous to manifest their appreciation of the cause in which you are engaged, and their estimation of the favour conferred by the Association in selecting this metropolis as the place for holding the present session," and, "in the name of the united profession of New York, tender you a sincere, a hearty, and a cordial welcome to our city."

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Dr. S. then read a list of the accredited delegates; those present answering as their names were called. He announced that a majority of those who had registered their names were in attendance.

The President then declared the meeting organized, welcomed the delegates, and stated the order of business.

The following list comprises the names of all the delegates and members present, as reported at various stages of the meeting.

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Maine Medical Association,	T. G. Stockbridge, John Benson, Alonzo Garcelon.
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Centre District Medical Society,	Luther M. Knight, William P. Stone, Nathan Sanborne, G. W. Garland.
Medical Department of Dartmouth College,	Dixi Crosby, Albert Smith.
Grafton District Med. Society,	H. B. Brown,
Permanent Member,	Josiah Crosby.

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Southern District Med. Society,	LYMAN BARTLETT.
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MISSOURI.

Medical Faculty of St. Louis University,	CHARLES A. POPE, JOHN B. JOHNSON.
Medical Association of the State of Missouri,	E. S. LEMOINE, JNO. B. JOHNSON, JAS. R. WASHINGTON, CHAS. A. POPE, C. W. HEMPSTEAD.
St. Louis Medical Society,	JAS. R. WASHINGTON, C. A. POPE, J. B. JOHNSON, C. W. HEMPSTEAD, E. S. LEMOINE.

ARKANSAS.

By Invitation,	DR. BORLAND.

MICHIGAN.

Peninsular Med. and Scientific Society of Michigan,	A. L. LELAND, ISAAC PADDOCK, HENRY TAYLOR, J. ANDREWS.
Lapeer Co. Medical Society,	FRED. K. BAILEY,
Permanent Member,	ZINA PITCHER.

IOWA.

Medical Department of the University of the State of Iowa,	J. C. HUGHES.

WISCONSIN.

State Med. Society of Wisconsin,	C. B. CHAPMAN, H. VAN DUSEN.

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CALIFORNIA.

By Invitation, STEPHEN R. HARRIS.

U. S. ARMY AND NAVY.

B. F. BACHE, U. S. N.
NINIAN PINKNEY, U. S. N.
JOHN M. CUYLER, U. S. A.
RICHARD H. COOLIDGE, U. S. A.
JOSIAH SIMPSON, U. S. A.

FOREIGN.

American Med. Society of Paris, { R. R. McILVAINE, N. J. PITTMAN.
Beyroot, Syria, ROBT. R. KALLEY.
Canada West, EDWARD M. HODDER.

[Thirty States and Territories, the District of Columbia, the Army and Navy of the United States, and the "American Medical Society in Paris" were represented in the Association. There was one member by invitation from Syria, and one from Canada West. The whole number of members in attendance was 573, and the whole number of institutions represented, 170.]

The Vice-Presidents and Ex-Presidents of the Association were requested to take seats on the platform with the President.

The President announced that in accordance with custom, a recess of fifteen minutes would be taken in order to allow the Delegates from each State represented in the Association to select one of their number to constitute a Committee of Nominations, to designate suitable officers for the ensuing year.

On motion, the Association then went into a recess to enable the Delegates to appoint the Nominating Committee.

At the expiration of the recess, the President announced the Nominating Committee as follows:—

ISAAC LINCOLN, Maine,
JOSIAH CROSBY, New Hampshire,
E. S. CARR, Vermont,
A. L. PEIRSON, Massachusetts,
HIRAM ALLEN, Rhode Island,
ARCHIBALD WELCH, Connecticut,
JOSEPH M. SMITH, New York,
LEWIS CONDICT, New Jersey,
FRANCIS WEST, Pennsylvania,
H. F. ASKEW, Delaware,
JOEL HOPKINS, Maryland,
THOMAS MILLER, D. C.,

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THOMAS P. ATKINSON, Virginia,
J. G. TULL, North Carolina,
HENRY R. FROST, South Carolina,
H. F. CAMPBELL, Georgia,
J. A. ENGLISH, Alabama,
J. B. FLINT, Kentucky,
R. L. HOWARD, Ohio,
JOSEPH SOMERS, Indiana,
CHARLES A. POPE, Missouri,
HENRY TAYLOR, Michigan,
J. C. HUGHS, Iowa,
N. S. DAVIS, Illinois,
J. B. LINSLEY, Tennessee.

Dr. ATLEE, of Pa., moved, that immediately after the Nominating Committee shall have reported, the President be requested to deliver his address, but withdrew his motion to allow Dr. Condie, of Pa., to present the Reports of the Committee of Publication, and of the Treasurer.

Dr. POPE, of Missouri, invited the Association to hold its next meeting in St. Louis.

Dr. CONDIE, of Pa., said he was instructed to invite the Association to hold their next meeting in Philadelphia.

Dr. HAYS, of Pennsylvania, expressed his regret that he must appear to act in opposition to the expressed wishes of his delegation, but he conceived that the Association were *pledged*, so far as a body like this could be pledged, by the acts of a former meeting, to assemble next year in St. Louis, and he therefore moved that the Committee of Nomination be instructed to report St. Louis as the place for the next annual meeting.

In making this motion, Dr. Hays stated, that he wished to be understood as yielding to a postponement merely for a year of the invitation to meet in Philadelphia, and trusted that his disinterestedness now would be remembered next year by his western friends as constituting an additional claim to a favourable consideration of the invitation to meet in Philadelphia in 1855.

Dr. Hays's motion was carried by acclamation.

Dr. CONDIE, of Pa., read the Report of the Committee of Publication, and of the Treasurer. The reports were accepted and ordered to be printed, and the following resolutions appended to the Report of the Committee of Publication were taken up separately for consideration:—

1. *Resolved*, That the assessment for the present year shall be Five Dollars.

2. *Resolved*, That the Committee of Publication be authorized to decide upon the terms at which the volume of *Transactions* for the present year shall be furnished to the members of the Association

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and others, and that it shall be their duty to inform the members of the terms agreed upon, by a circular.

3. *Resolved*, That the Committee be further authorized to take such measures in relation to the disposal of copies of the *Transactions* remaining after all such members are supplied as shall comply with the terms set forth in the circular of the Committee, as they may deem expedient.

Dr. CONDIE urged the adoption of the resolutions; they were then separately put and adopted.

Dr. CONDIE offered the following, which was adopted:—

Resolved, That the Committee of Publication have power to furnish the Chairmen of Committees on Epidemics with extra copies of their reports, respectively, at the expense of the Association—the said extra copies not to exceed one hundred.

The Secretary read communications inviting the Association to visit Messrs. J. C. and D. Hyatt's Collection of Anatomical and Pathological Models; the Surgical Instrument establishment of Mr. George Tiemann; from Dr. Draper to visit the Medical College of the University of the City of New York, and from the Chairman of the Committee of Governors of the New York Hospital to visit the Bloomingdale Insane Asylum.

Dr. STEWART presented a report from the Committee of Arrangements, recommending that the following gentlemen be admitted as members by invitation:—

Dr. MARSHALL HALL, of London.
T. G. MOWER, U. S. Army.
JOSIAH SIMPSON, U. S. Army.
B. F. BACHE, U. S. Navy.
NINIAN PINKNEY, U. S. Navy.
JAMES BROWNELL, U. S. Navy.
THOMAS K. LEONARD, of Florida.
GEORGE W. BETTON, of Florida.
Hon. H. E. BARTLETT, M. D., of the New York State Senate.
Dr. STEPHEN R. HARRIS, of California.
EDWARD M. HODDER, of Canada West.

And Drs. MCILVAINE, of Ohio, and N. J. PITTMAN, as representatives from the "American Medical Society in Paris."

The report was accepted and adopted, and, on motion of Dr. COX, of Maryland, it was "*Resolved*, That Dr. MARSHALL HALL, of London, be invited to take a seat in this body; and farther, that a

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committee of three be appointed, whose duty it shall be to convey this invitation to Dr. Hall and introduce him to the Association.

The President appointed Dr. Cox, of Maryland, Dr. JOSEPH M. SMITH, of New York, and Dr. DOVE, of Virginia, that committee.

On motion of Dr. ATLEE, of Pennsylvania, the President then delivered his address, embracing a review of the origin, progress, and benefits achieved by the Association.

Dr. HAYS, of Pennsylvania, moved the following, which was seconded by Dr. MARCH, of New York, and adopted unanimously: *Resolved*, That the thanks of this Association be presented to Dr. BEVERLEY R. WELLFORD, our President, for his elegant, appropriate, and eloquent address, and that he be requested to furnish a copy for publication in the *Transactions* of the Association.

The Secretary read the following resolution, passed by the Medical Society of Virginia, at a meeting held in Richmond on the 7th of April, 1853; and communicated by Dr. W. D. Atkinson, the Secretary.

"*Resolved*, That this Society recommend to the American Medical Association at its ensuing meeting, to consider the propriety of appointing some distinguished and well qualified chemist (not a practitioner of medicine), whose duty it shall be to analyze the prominent nostrums of the day, as far as may be practicable, and to publish the result of such analysis monthly in the most extensively circulated newspaper of each State in the Union represented in the Association."

The Secretary also read a letter from the President of the "American Medical Society in Paris," giving an account of the present condition and future prospects of the Society, and announcing the appointment of Drs. PITTMAN, WALTON, and MCILVAINE, to attend the meeting of the Association.

Dr. CONDIE inquired whether he should be at liberty to send a set of the *Transactions* of the Association to the "American Medical Society in Paris."

Dr. ATLEE, of Pa., moved that the Chairman of the Committee of Publication be authorized to send a complete set of the *Transactions* of the Association to the American Medical Society in Paris.

Dr. HAYS, of Pa., moved to amend by referring the matter to the Committee of Publication.

After a brief discussion, Dr. HAYS withdrew his amendment, and Dr. ATLEE'S resolution prevailed.

The Secretary stated that he had received a communication from

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Dr. RAMSEY, of Georgia, inclosing documents, which, on motion of Dr. LEVING, of South Carolina, were laid on the table.

Dr. JOSEPH M. SMITH, Chairman of the Committee on Nominations, reported the following officers for the ensuing year:—

PRESIDENT,
JONATHAN KNIGHT, *Connecticut.*

VICE-PRESIDENTS,
USHER PARSONS, *Rhode Island,* LEWIS CONDICT, *New Jersey,*
HENRY R. FROST, *South Carolina,* R. L. HOWARD, *Ohio.*

SECRETARIES,
EDWARD L. BEADLE, *New York,* EDWIN S. LEMOINE, *Missouri.*

TREASURER,
D. FRANCIS CONDIE, *Pennsylvania.*

The Committee reported St. Louis, Missouri, as the place for the meeting of the Association in 1854.

The report was accepted and unanimously adopted, and the gentlemen nominated declared to be the officers of the Association for the ensuing year.

On motion, Dr. GOOCH, of Va., Drs. WATSON, of N. Y., and ATLEE, of Pa., were appointed a committee to conduct Dr. JONATHAN KNIGHT, the President elect, and the other officers, to their seats.

Dr. KNIGHT, on taking the Chair, returned thanks for the honour conferred on him.

Dr. ATLEE, of Pa., offered the following, which was adopted unanimously by a rising vote:—

Resolved, That the thanks of this Association be presented to Dr. BEVERLEY R. WELLFORD, our late President, for the dignified, courteous, and efficient manner in which he has performed the duties of his office.

Dr. STEWART, of N. Y., moved a vote of thanks to the retiring Secretary, Dr. P. C. GOOCH, for the efficient manner in which he had conducted the business of this Association; which was unanimously adopted.

Dr. HOPKINS, of Maryland, moved that no member be allowed to speak more than ten minutes on any subject at one time, nor more than twice on the same subject.

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Dr. CONDIE proposed to amend the resolution of Dr. Hopkins, by prohibiting any member from speaking more than twice on the same subject, unless by permission, but without restriction as to time.

Dr. COX, of Md., moved to amend, by substituting fifteen minutes for ten.

The amendments were lost, and the original resolution adopted.

On motion, the reading of the minutes of the last meeting of the Association was dispensed with.

Dr. GOOCH, of Va., moved that when the Association adjourn, it adjourn to meet at ten o'clock A. M., to-morrow.

Dr. STEWART, on the part of the Committee of Arrangements, suggested that the Association meet each day from 9 to 12 A. M., and from 1 to 4 P. M.

An amendment to Dr. Gooch's resolution was offered, fixing the hour at 9 o'clock. The amendment was accepted by Dr. Gooch, and the resolution then adopted.

On motion, the Association adjourned.

Wednesday, May 4, Morning Session.

The Association met at 9 A. M.; the President, Dr. KNIGHT, in the chair.

The Secretary read the minutes, which were approved.

Dr. COX, of Maryland, moved for a reconsideration of the vote approving the minutes, in order to allow of a correction in the Special Report of the Committee of Arrangements, inviting Drs. Pinkney and Bache, U. S. N., to seats, as members by invitation. Dr. Cox remarked, that these gentlemen had a right to sit as delegates from the Army and Navy. Dr. Pinkney has twice represented the naval medical corps in the Convention as a delegate, and without any question. As the gentleman was so admitted at Richmond, he would move to have the record altered.

Dr. STEWART, Chairman of the Committee of Arrangements, explained and read the second article of the Constitution, which relates to the appointment of delegates.

Dr. WATSON, of N. Y., stated they had always been received as delegates.

The motion to reconsider was put and lost.

Dr. COX moved that Drs. Pinkney and Bache be received as regular delegates.

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Dr. PINKNEY, U. S. N., asked and obtained leave to be heard, and claimed his right to a place as a delegate, and as a permanent member, having been formerly received as such, and having signed the Constitution.

Dr. F. C. STEWART offered the following as an amendment to Dr. Cox's motion:—

Resolved, As the sense of this Association, that, under its present Constitution, delegates can be received from the U. S. Army and Navy Medical Staffs, when appointed by the Chiefs of the Army and Navy Medical Bureaus.

The amendment was adopted.

Dr. STEWART, from the Committee of Arrangements, reported that several members had registered their names since the report of yesterday.

On motion of Dr. COX, of Maryland, Dr. Borland, of Arkansas, was received as a member by invitation.

Dr. C. D. MEIGS, of Philadelphia, presented a report on "Acute and Chronic Diseases of the Neck of the Uterus," with a request that it should be referred to the Committee on Publication without reading.

The report was accepted and referred.

Dr. COVENTRY, of New York, moved for a suspension of the order of business, to take up the subject of the proposed amendments to the Constitution.

On motion of Dr. CASH, of New York, the resolution was laid on the table, and the regular order of business proceeded with.

On motion of Dr. ATLEE, of Pa., the Secretary proceeded to call in their order the special committees.

The following Reports were received:—

From Dr. G. EMERSON, of Philadelphia, Pa.—On the "Agency of the Refrigeration produced through upward radiation of Heat, as an exciting Cause of Disease," an abstract of which report was read, and the report itself referred to the Committee of Publication.

From Dr. H. F. CAMPBELL, of Augusta, Ga.—On Typhoid Fever. Dr. Campbell gave a verbal abstract of the report, which latter was accepted and referred to the Committee of Publication.

From Dr. W. L. SUTTON, of Georgetown, Ky.—On the Epidemics of Tennessee and Kentucky. Dr. Sutton not being present, the abstract of the report was read by the Secretary. On motion, the report was accepted and referred to the Committee of Publication.

The Secretary read a letter from Dr. GEORGE MENDENHALL, of

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Cincinnati, Ohio—Chairman of the Committee on the Epidemics of Ohio, Indiana, and Michigan—stating that he had appointed as his colleagues, Dr. Z. Pitcher, of Detroit, Michigan; Dr. N. Johnson, of Cambridge City, Indiana; Dr. Tilden, of Sandusky, Ohio, and Dr. J. A. Allen, of Michigan—that a "considerable amount of valuable materials had been collected, which he hoped to use more profitably after observations that he hoped to procure by the next meeting of the Association."

Dr. CONDIE, of Pa., Chairman of Committee on Causes of Tubercular Disease, stated that, in consequence of his duties as Treasurer, and Chairman of Committee of Publication, he had not been able to complete his report. A large amount of material had been collected—in fact, the report was almost ready; but he had had such onerous duties to perform during the year, that it was utterly impossible for him to find time to put it in proper shape.

Dr. F. PEYRE PORCHER, of Charleston, S. C., Chairman of the Committee on the Toxicological and Medicinal Properties of our Cryptogamic Plants, had prepared his report, and it would be presented if the Association requested it; but Dr. Porcher believed that it would be much more valuable if he was allowed to continue his observations for another year.

Dr. WORTHINGTON HOOKER, of Conn., Chairman of Committee on Epidemics of New England and New York, asked that the Committee be continued.

Dr. WELLFORD announced that Dr. HAXALL, of Va., Chairman of Committee on Epidemics in Virginia and North Carolina, had resigned, and that Dr. J. F. Peebles, of Virginia, had been appointed to fill his place.

Dr. WELLFORD stated that he had also received the resignation, this morning, of Dr. E. H. Barton, of New Orleans, as Chairman of the Committee on the Epidemics of Mississippi, Louisiana, Texas, and Arkansas.

Dr. CAIN stated that Dr. Bolling, of Ala., Chairman of Committee on Epidemics in South Carolina, Georgia, Florida, and Alabama, had resigned.

Dr. JOHNSON, of Mo., stated that Dr. Reyburn, Chairman of Committee on Epidemics of Missouri, Illinois, Iowa, and Wisconsin, had requested that his Committee be continued.

On motion of Dr. ATLEE, it was

Resolved, That the delegates from the different States be requested

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to designate some gentleman from the States to which they belong, to co-operate with the Scientific Committees.

Dr. Z. PITCHER, of Mich., stated that he was ready to present the Report of the Committee on Medical Education, of which he was Chairman.

On motion, Dr. Pitcher was requested to read the report at length. To the report were annexed the following resolutions:—

Resolved, That the Association reaffirm its formerly expressed opinions on the value and importance of general education to the student and practitioner of medicine, and that it would gladly enlarge its rule on this subject, so as to include the Humanities of the schools, and the Natural Sciences.

Resolved, That in the opinion of this Association, a familiar knowledge of the elements of medical science should precede clinical instruction.

Resolved, That in order to accomplish the latter, the Hospitals, when they shall be elevated to the rank of schools of practice, and the intelligent private preceptor, are the most efficient instrumentalities to be employed.

On motion of Dr. ATLEE, of Pa., the report was accepted, and the accompanying resolutions being separately considered, were adopted.

Dr. JOSEPH M. SMITH, Chairman of the Committee on Volunteer Communications, presented the following Report:—

The Committee on Volunteer Communications, appointed at the last meeting of the American Medical Association, beg leave to report that they have received fifteen essays, twelve of which were accompanied with sealed packets, containing the names of their authors. These communications have been critically examined; and the Committee have awarded the prizes, each of $100, to the two bearing the following titles:—

1. "The Cell; its Physiology, Pathology, and Philosophy, as deduced from Original Investigations, to which is added its History and Criticism."

"Natura in minimis maxima est."

2. "The Surgical Treatment of Certain Fibrous Tumours of the Uterus, heretofore considered beyond the Resources of Art."

"Palmam qui meruit ferat."

In regard to the remaining essays, of which the names of the authors are concealed, the Committee have the pleasure to state that the following are deemed worthy of being honourably mentioned:—

1. "On the Erysipelatous Fever of the United States."
2. "Chloroform."

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3. "On Subacute Functional Disorder of the Urinary Bladder of Females."

These essays present many interesting facts and observations relating to the subjects of which they treat; and their publication would, in the judgment of the Committee, be acceptable to the profession, and creditable to their authors.

Of the three papers communicated with the names of their authors, two of them relate to points of pathology and practice, so novel and interesting, that the Committee would respectfully ask permission for their authors to submit abstracts of them to the Association. The first is by Dr. Alden March, "On Coxalgia, or Hip Disease;" and the second, by Dr. Gurdon Buck, "On the Surgical Treatment of Morbid Growths within the Larynx, illustrated by an Original Case, with Statistical Observations, elucidating their Nature and Forms." The remaining paper of this class does not, in the opinion of the Committee, possess sufficient originality to entitle it to the special attention of the Association.

All which is respectfully submitted.

JOSEPH M. SMITH,
JOHN A. SWETT,
GURDON BUCK,
ALFRED C. POST,
WILLIAM PACKER.

The report was accepted, and the request of the Committee granted.

On breaking the seals of the accompanying packets it was found that the author of the first, was Waldo J. Burnett, M. D., Boston, Mass., and of the second, Washington L. Atlee, M. D., Philadelphia.

Dr. MARCH then gave a verbal abstract of his paper on Disease of the Hip-Joint.

Dr. L. A. SMITH, of N. J., moved that Dr. March be requested to read his paper at length, during the recess of to-morrow, in the College of Physicians and Surgeons in Crosby Street.

Adopted.

Dr. A. B. PALMER, of Chicago, moved the following resolutions:—

Resolved, That this Association earnestly recommend to the local Societies in different portions of our country, to appoint Committees, whose duties it shall be to record the prevalence of epidemics, or other diseases, and the general state of health in their respective localities, and to transmit said reports to the Committees of the Society on Epidemics, through the State Societies where they exist.

Resolved, That the Secretaries be instructed to secure a wide publicity to the above resolutions, by such means as they may deem proper.

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The resolutions were adopted.

Dr. ATLEE, of Pa., called the attention of the Association to resolutions passed at the last annual meeting of the Association, for purchasing a suitable stone for the Washington Monument.

Dr. THOS. W. BLATCHFORD, of N. Y., offered the following:—

Resolved, That the suggestions in the President's Address, touching the licensing power, be referred to a committee of five, of which Dr. Wellford shall be the Chairman, to prepare some plan whereby the subject may be brought fairly before the profession, and, if deemed advisable, that the Legislatures of the several States be memorialized to carry out the recommendations of this Association; the committee to report at the next meeting of the Association.

Dr. ALEX. Y. P. GARNETT, of Washington, D. C., proposed the following as an amendment:—

Resolved, That a committee of five be appointed by the President of this Association, to prepare a Memorial to the legislative bodies of the several States of the Union, praying that a law be passed prohibiting the Faculty of any medical institution which may at present exist, or which may hereafter be established within the limits of said States, from conferring the degree of Doctor of Medicine upon any candidate for graduation who has not previously graduated at some literary institution, or who, upon examination by a competent Board, is not found to possess a good English and Classical education.

Resolved, That regarding the present term of preparatory study adopted by the Medical Colleges of the United States, too limited to enable students of medicine to acquire such a competent knowledge of the profession as should entitle them to receive the degree of Doctor of Medicine, a recommendation be incorporated in said Memorial that some legislative action be also had upon this subject.

Resolved, That it be the duty of the President and Secretary of this Association, to transmit, through the Executive heads of each State, a copy of said Memorial to their respective Legislative Assemblies, and that a Circular be addressed to the members of the Medical Profession, resident at, or convenient to, the seats of Government of the several States, requesting them to use every practicable exertion consistent with the honour, dignity, and good repute of the profession, to procure the passage of a law in accordance with the foregoing resolutions.

On motion of Dr. CASH, of N. Y., the resolution and amendment were laid on the table, and the Association adjourned to 1½ o'clock P. M.

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Afternoon Session.

At 1½ o'clock P. M., the Association was called to order by the President.

The Secretary read a letter from Dr. J. H. GRISCOM, on behalf of the Board of Governors and Medical Staff of the New York Hospital, for the Association to visit that Institution.

Dr. F. C. STEWART, from the Committee of Arrangements, reported the names of several delegates who had registered their names since the last report.

The Committee recommend that the following gentlemen be admitted to seats in the Association, as members by invitation: Dr. ROBERT REID KALLEY, of Beyroot, Syria, formerly of Glasgow, Scotland; Dr. JAS. G. COOPER, of Washington Territory, U. S., there being no accredited delegate from that Territory, and Dr. HEZEKIAH WILLIAMS, of Southern Illinois. Report accepted, and the recommendation adopted.

The following resolutions, from Dr. CHAS. A. LEE, a permanent member, not present, were read by the Secretary:—

Resolved, As the sense of this Association, that those Medical Colleges which give two courses of Lectures annually, each of which counts as a separate course, have virtually violated and forfeited their charters, which do not contemplate but one annual session (thus making two Colleges out of one).

Resolved, That the practice in question is calculated to lower the standard of attainment in the profession, and subjects those who countenance it to the imputation of acting from mercenary motives.

Resolved, That no delegates shall hereafter be received by this Association from any Medical Schools, which give two courses of Lectures annually, each of which counts towards a degree.

On motion of Dr. ATLEE, of Pa., the resolutions were laid on the table.

Dr. STEWART read the following resolutions, offered by Dr. STEPHEN W. WILLIAMS, of Mass, a permanent member, not present:—

Resolved, That the thanks of this Association be presented to Dr. WINSLOW LEWIS, of Boston, a member of the Massachusetts Legislature, for the bill which he has presented and endeavored to sustain, providing that "no druggist, apothecary, or person engaged in manufacturing medicines, or compounds to be administered as medicines (except such as are published in standard works of chemistry, materia medica, or in pharmacopœiæ), shall offer the same for sale in any

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way, until he has filed a complete recipe in English, sworn to before a legal authority constituted for such purpose."

Resolved, That a Committee be appointed by this Association for the purpose of petitioning Congress and the State Legislatures to enact regulations and laws similar to the above.

The resolutions were adopted, and Dr. JAS. MOULTRIE, of South Carolina, Dr. STEPHEN W. WILLIAMS, of Mass., and Dr. WORTHINGTON HOOKER, of Conn., appointed the committee.

The following resolution from Dr. WILLIAMS was also read.

As we are constantly called upon to deplore the ravages of death among the illustrious and worthy members of our profession throughout the United States,

Resolved, That a Standing Committee be appointed by this Association to procure memorials of the eminent and worthy dead among the distinguished physicians of our country, and present them to this Association for publication in their *Transactions*.

Dr. COX, of Md., moved an amendment, not confining the memoirs to distinguished members of the Association.

On motion of Dr. JAMES E. MORGAN, of Washington, D. C., the resolutions and amendment were laid on the table.

Dr. G. BUCK, of New York, read his paper "On the Surgical Treatment of Morbid Growths within the Larynx," illustrating a case which had been under his care by means of the morbid specimen and a wax model.

On motion, the paper was referred to the Committee of Publication.

Dr. J. K. MITCHELL, of Pa., after some remarks, offered the following:—

Whereas, The claim of Naval Medical officers to a defined rank, assimilated with the grades of officers of the line of the Navy, has not yet been decided by Congress, therefore

Resolved, That the President of this meeting appoint a Committee of three, which is hereby instructed to communicate to Congress, through the presiding officer of each House, at the commencement of its next session, an expression of the interest felt by the American Medical Association of the United States, for their professional brethren employed in the Navy, as set forth in resolutions unanimously adopted at several previous sessions of this body.

Dr. JACKSON, of Philadelphia, seconded the resolution, with remarks.

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The President stated that the bill had passed one branch of our National Legislature, but was lost in the other House.

Dr. PINKNEY, U. S. N., made a statement of the action had in the National Legislature.

The resolution was put and adopted.

Dr. STEVENS, of New York, moved that the President be Chairman of the Committee.

Adopted, and the President named as his associates, Dr. MITCHELL, of Pa., and Dr. A. H. STEVENS, of New York.

Dr. CHARLES HOOKER, of Conn., offered the following, which was adopted:—

Resolved, That the delegates from the several States be requested to appoint committees, who shall aid the Committee of Publication in procuring subscribers and in distributing the annual *Transactions* of the Association.

Dr. CONDIE moved that the Committee on Nominations be authorized to nominate the several Standing Committees as far as may be necessary, and report the said Committees to-morrow, if possible. Adopted.

Dr. BOLTON, of Virginia, presented the following resolution:—

Resolved, That the medical department of the University of Michigan, by its co-operation with the efforts of this body to elevate the standard of medical education, especially in regard to its preliminary studies, and an extended term of instruction, has entitled itself to our warm commendation and hearty support.

A discussion ensued, in which Drs. W. HOOKER, PALMER, COX, and BOLTON took part, and finally the resolution was withdrawn.

A letter was read by the President from Dr. CARNOCHAN, of New York, proposing to present before the Association a patient from whom he had removed the entire lower jaw; permission was granted, but on motion of Dr. F. S. BURROWES, of Pennsylvania, the permission was reconsidered, and the exhibition allowed during recess.

Dr. MAURAN, of Rhode Island, presented a report which was read by the Secretary from Dr. SIMONS, of South Carolina, Chairman of a Committee appointed to memorialize Congress to "enact a law making it obligatory upon all vessels carrying emigrants to have a regular Surgeon on board."

The report stated that the proper measures were taken by the Committee, and the matter was presented to the Senate of the United States, in May, 1852, and referred to the Committee on Commerce; no farther action was had by the Senate, but the Committee has re-

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ceived every assurance that the measure will have a proper consideration at the next session of Congress; they therefore ask that the Committee be considered as reporting progress, and asked to be continued.

Report accepted and Committee continued.

The Secretary read another report from Dr. SIMMONS, Chairman of a Committee to memorialize Congress in accordance with a resolution of Dr. SUTTON, of Georgetown, Kentucky, to have the medical statistics of the United States Census printed separately, for the use of the medical profession.

The Committee had discharged its duty so far as it was able, but without accomplishing the object desired. The Committee concluded its report by "urging upon the members of the Association, the great importance of using their best influence to induce the Legislatures of their respective States, to establish a registration of Births, Marriages, and Deaths, a measure of incalculable value, as regards vital statistics.

On motion of Dr. ATLEE, the report was accepted, and the Committee continued.

Dr. E. R. PEASLEE, of New Hampshire, offered the following:—

Whereas, The various systems of irregular practice are encouraged in the regular Medical Colleges, so far as allowing to graduate such as subsequently engage in them, therefore

Resolved, That it is the duty of the Faculties of the Medical Colleges to refuse to admit to an examination, all persons who intend to engage in any other than the regular practice of medicine; and to give notice to this effect in their advertisement of the annual course of lectures.

Dr. L. A. SAYRE, of New York, offered the following amendment:—

Resolved, That a Committee be appointed to memorialize the several State governments, in reference to the subject of diplomas to medical men, and to petition them, in the name of the Association, for the passage of a law granting to chartered Medical Colleges the privilege of retracting publicly the diplomas of any of their graduates, when, in the judgment of the medical faculty of the college or school granting such diplomas or certificates, they may have forfeited a right to the same.

A debate ensued, which was participated in by Drs. POPE, HOOKER, PALMER, ATLEE, and others, when Dr. GOOCH, of Va., moved to lay the resolution and amendment on the table, which motion was adopted.

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Dr. STEWART announced that two hundred copies of the *Transactions* of the New York State Medical Society had been forwarded to the Secretary of the Association for distribution among the delegates from other States.

On motion of Dr. STEVENS of N. Y., the Association adjourned until to-morrow at 9 o'clock A. M.

Thursday, May 5, Morning Session.

The Association met at 9 A. M., the President, Dr. KNIGHT, in the chair.

The Secretary read the minutes, which were approved.

Dr. STEWART, Chairman of Committee of Arrangements, reported the names of several delegates who had registered their names since last report; he also read a letter from Robert Kelly, Esq., President of the "Society for the Reformation of Juvenile Delinquents," inviting the Association to visit the "House of Refuge."

Dr. W. HOOKER, of Ct., offered the following resolution, which was adopted:—

Resolved, That a committee of five be appointed, whose duty it shall be, in compliance with the suggestions of our late President, Dr. WELLFORD, to report upon plans of organization for State and County Societies, and that the Committee be requested to report, if possible, during the present meeting of the Association.

Dr. G. P. ZEIGLER, of Pa., offered a preamble and resolutions on the necessity which existed for a more general, systematic, and speedy organization of State and County Medical Societies, together with a plan for such organizations.

Dr. CASH, of N. Y., moved that the document be referred to the Committee on State and County Medical Societies. Carried.

The Secretary read a notice, requesting delegates, from any States which were not represented on the Committee of Nominations, to elect representatives, to sit with the Committee now in session at the office of Dr. Joseph M. Smith, No. 56 Bleecker Street.

The President having declared the reading of reports from such special committees as were not ready yesterday, to be the order of the day. Dr. N. S. DAVIS, of Ill., Chairman of the Committee on Medical Literature, read at length, a report, premising that he was to be considered personally responsible for the sentiments contained therein, as the Chairman of the Committee, Dr. LA ROCHE, of Pa.,

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had by ill health been prevented from giving much attention to the subject.

The Report was accepted and referred to the Committee of Publication.

Dr. S. JACKSON, of Pa., stated that he had received a paper from a gentleman, not a member, on Electricity, as applied to predictions of meteorological changes. The paper was laid on the table.

Dr. YANDELL, of Kentucky, offered the following, which were adopted unanimously by a rising vote:—

Whereas, By the dispensation of an inscrutable Providence, Dr. DANIEL DRAKE has been removed, since the last meeting of this Association, from the scene of his earthly labours,

Resolved, That in the death of Dr. DRAKE, the American Medical Association has lost one of its most honoured members, and the American Medical profession one of its brightest ornaments.

Resolved, That his steady devotion to his profession through a long life, and his zeal, activity, and unceasing efforts to advance its interests, afford an example worthy of the imitation of every young physician.

Resolved, That this Association will cherish the memory of Dr. Drake for his many virtues, and for his labours, which have adorned and elevated our profession.

The President read a resolution, offered by Dr. CLEAVELAND, of Vermont, calling for the appointment of a Committee to investigate the value of Galvanism as a therapeutic agent, which, on motion, was referred to the Committee of Nominations.

On motion of Dr. WELLFORD, of Virginia, Dr. J. HANCOCK DOUGLASS, of La., was invited to take his seat as a member by invitation from that State.

On motion of Dr. PARKER, of Va., the resolution offered yesterday from the Virginia Medical Society, recommending the appointment of a chemist, and laid on the table, was taken up for consideration, by a vote on division of 185 ayes, 85 nays.

Dr. PARKER, of Va., offered the following as an amendment:—

Resolved, That this Association recommend Congress to consider the propriety of passing a law compelling all importers of nostrums to state upon all compounds thus imported their true constituents, and in English; and that the Secretary be instructed to forward a copy of these resolutions to the Executive of the General Government.

An amendment to strike out all after the word "English," was accepted.

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After a discussion by Dr. BOND, of Md., Dr. W. HOOKER, of Ct., Dr. SAYRE, of N. Y., Dr. COX, of Md., Dr. BOLTON, of Va., Dr. RICHARDS, of Ohio, and Dr. JACKSON, of Mass., the resolution and amendment were lost.

Dr. CONDIE, of Pa., offered the following resolution:—

Resolved, That we have heard with sincere regret of the death of our late fellow-member, Dr. ISAAC PARRISH, of Philadelphia, who was distinguished by his early and earnest advocacy of the establishment of this Association, by his ardent interest in its proceedings, and by his valuable contributions to its published proceedings.

Dr. COX, of Md., moved the following:—

Resolved, That in the demise of Dr. WILLIAM E. HORNER, which has occurred since the last annual session of this body, the American Medical Association has lost one of its illustrious and useful members, and the science of medicine an indefatigable student and most distinguished teacher.

Resolved, That the memory of the gifted subject of these resolutions, dear as he must ever be to the lovers of medical science universally, will be especially cherished by this Association, to whose great objects and aims his last efforts were, during life, promptly and liberally bestowed.

Both of these resolutions were adopted unanimously by a rising vote.

Dr. YANDELL, of Ky., presented the report of Dr. S. D. GROSS, of Louisville, Ky., On the Results of Surgical Operations for the Relief of Malignant Diseases, and read an abstract giving the conclusions of the report.

The report was accepted and referred to the Committee of Publication.

Dr. GOOCH, of Va., called up the subject of the graduating pledge, proposed by Dr. Peaslee, of N. H., last evening, and laid over, and proposed the following resolutions:—

Resolved, That this Association earnestly recommends to all the respectable Medical Colleges of the United States to administer to their graduates, previous to their receiving the diploma, some pledge that they will maintain, to the best of their abilities, the honour and dignity of the profession; and that they will forfeit their degrees, whenever they desert the orthodox system of medicine.

Resolved, That the schools be urged not to graduate any man without requiring him to read the National Code of Ethics, and publicly give his consent to abide by it, and that they will reserve to

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themselves the right to withdraw the diploma, publicly, whenever *the graduating* pledge has been violated.

Dr. JOHN H. PHILLIPS, of N. J., offered the following amendment:—

Resolved, That it is the duty of all Boards of Examiners, to whom candidates may apply for examination or approval, to admit none but those who give satisfactory evidence of a good preliminary education, and that a regular Course of Medical Practice will afterwards be pursued, and who shall subscribe to the Code of Ethics adopted by this Association.

After some discussion on the resolution and proposed amendment, Dr. SAYRE, of N. Y., moved a recess till 1½ P. M. Adopted.

Afternoon Session.

The President called the Association to order at 1½ o'clock P. M.

Dr. COVENTRY, of N. Y., moved to lay all other business on the table and take up the proposed amendments to the Constitution, which motion was lost.

The consideration of Dr. GOOCH'S resolution, and the amendment thereto, was resumed.

Sundry amendments were proposed by Drs. CARR, of Vt., DOVE, of Va., and STILLÉ, of Pa., when, on motion of Dr. GOOCH, the whole subject was referred to a special Committee of three, to report during the present session.

The President appointed Drs. GOOCH, of Va., PHILLIPS, of N. J., and STILLÉ, of Pa., as the Committee.

The following form of Promise was among the documents referred to the Committee:—

I, A. B., of ——, in the State of ——, do hereby promise, on the honour of a gentleman, that I will conform strictly to the Code of Ethics of this my Alma Mater, in all things pertaining to the practice of my profession; and, when I shall fail to do so, I hereby grant to the Faculty of said School, full power and authority to withdraw said Diploma, and all the rights and privileges which it is intended to confer.

Dr. STEWART, on behalf of Committee of Arrangements, reported the names of delegates who had registered since last report, and recommending to invite Dr. HARVEY P. PEET, President of the Institution for Deaf and Dumb, and Dr. W. C. BUTLER, to take seats in the Association. Accompanying was a letter from Dr. THOMAS SPENCER, of N. Y., asking to be allowed to read a paper

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On the Atomic Theory of Life and Vital Heat, as Applicable to Pathology; Dr. S. also stated that Drs. J. H. GRISCOM and J. L. PHELPS, of N. Y., have applied for permission to read papers before the Association.

The Committee also recommended that, when the Association adjourn, it should be to Saturday morning.

The report was accepted and adopted, except as relates to the time of adjournment.

Dr. BLATCHFORD, of N. Y., moved to have a resolution, offered by him yesterday, on the licensing power, with amendments by Dr. Garnett, taken up for consideration.

After discussion by Drs. W. HOOKER, of Connecticut, JOHNSON, of Missouri, and ATLEE, of Pa., on motion of Dr. ATLEE, the whole subject was referred to a Committee, to report at the next meeting.

The subject was referred to the following Committee: Drs. WELLFORD, of Va., SAMUEL JACKSON, of Pa., T. W. BLATCHFORD, of N. Y., JOHN B. JOHNSON, of Mo., and E. R. PEASLEE, of N. H.

Dr. T. W. BLATCHFORD, of N. Y., proposed the following resolution, which was adopted:—

Resolved, That a committee of five be appointed, of which Dr. J. O. EDWARDS, of Ohio, shall be Chairman, whose duty it shall be to report on the best mode of preventing the domestic adulteration of drugs. The Chair appointed Dr. EDWARDS, Drs. WOOD, and CARSON, of Philadelphia; Dr. C. T. JACKSON, of Boston, and Dr. J. H. GRISCOM, of New York.

Dr. JOSEPH M. SMITH, of N. Y., Chairman of the Committee of Nominations, read the following:—

Report of Committee of Nominations.

The Committee of Nominations, in fulfilling the duty of their appointment, propose to continue most of the Special Committees appointed by the Association, in May, 1851, and May, 1852, and to appoint several new Special Committees. They, therefore, submit the following list of Chairmen of Special Committees, with the subjects to them committed:—

1. Dr. D. F. CONDIE, of Philadelphia, Pa.—On the Causes of Tubercular Disease.
2. Dr. JAMES JONES, of New Orleans, La.—On the Mutual Relations of Yellow and Bilious Remittent Fever.
3. Dr. R. S. HOLMES, of St. Louis, Mo.—On Epidemic Erysipelas.
4. Dr. GEORGE B. WOOD, of Philadelphia, Pa.—On Diseases of Parasitic Origin.

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5. Dr. R. D. ARNOLD, of Savannah, Ga.—On the Physiological Peculiarities and Diseases of Negroes.

6. Dr. JAMES R. WOOD, of New York—On Statistics of the Operation for the Removal of Stone in the Bladder.

7. Dr. F. PEYRE PORCHER, of Charleston, S. C.—On the Toxicological and Medicinal Properties of our Cryptogamic Plants.

8. Dr. GOODRICH A. WILSON, of Virginia—On Cholera, and its Relation to Congestive Fever—their Analogy or Identity.

9. Dr. WORTHINGTON HOOKER, of Connecticut—On Epidemics of New England and New York.

10. Dr. JOHN L. ATLEE, of Lancaster, Pa.—On Epidemics of New Jersey, Pennsylvania, Delaware, and Maryland.

11. Dr. D. J. CAIN, of Charleston, S. C.—On Epidemics of South Carolina, Florida, Georgia, and Alabama.

12. Dr. W. L. SUTTON, of Georgetown, Ky.—On Epidemics of Tennessee and Kentucky.

13. Dr. THOMAS REYBURN, of St. Louis, Mo.—On Epidemics of Missouri, Illinois, Iowa, and Wisconsin.

14. Dr. GEORGE MENDENHALL, of Cincinnati, Ohio.—On Epidemics of Ohio, Indiana, and Michigan.

15. Dr. E. D. FENNER, of New Orleans, La.—On Epidemics of Mississippi, Louisiana, Texas, and Arkansas.

16. Dr. CHARLES A. LEE, of New York—On Domestic Hygiene.

17. Dr. DANIEL BRAINARD, of Chicago, Ill.—On the Constitutional and Local Treatment of Carcinoma.

18. Dr. N. S. DAVIS, of Chicago, Ill.—On the Influence of Local Circumstances on the Origin and Prevalence of Typhoid Fever.

19. Dr. GEORGE ENGELMAN, of St. Louis, Mo.—On the Influence of Geological Formation on the Character of Disease.

20. Dr. HENRY M. BULLITT, of Louisville, Ky.—On the Use and Effect of Applications of Nitrate of Silver to the Throat, either in Local or General Disease.

21. Dr. ROBERT F. CAMPBELL, of Augusta, Ga.—On the Pathogenic Influence of Feather Beds.

22. Dr. JAMES BOLTON, of Richmond, Va.—On the Administration of Anæsthetic Agents during Parturition.

23. Dr. HENRY TAYLOR, of Mount Clemens, Mich.—On Dysentery.

24. Dr. F. DONALDSON, of Baltimore, Md.—On the Present and Prospective Value of the Microscope in Disease.

25. Dr. R. L. HOWARD, of Columbus, Ohio.—On the Pathology and Treatment of Scrofula.

Committee on Plans of Organization for State and County Societies.—ISAAC HAYS, M. D., of Pennsylvania, Chairman; WORTHINGTON HOOKER, M. D., of Connecticut; JOSIAH ANDREWS, M. D., of Michigan; B. R. WELLFORD, M. D., of Virginia; A. L. PEIRSON, M. D., of Massachusetts.

Committee on Medical Literature.—T. S. BELL, M. D., of Kentucky, Chairman; SAMUEL H. PENNINGTON, M. D., of New Jersey;

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Ed. H. Parker, M. D., of New Hampshire; William K. Bowling, M. D., of Tennessee; Zina Pitcher, M. D., of Michigan.

Committee on Medical Education.—B. R. Wellford, M. D., of Virginia, Chairman; R. Lowe, M. D., of Iowa; Lyndon A. Smith, M. D., of New Jersey; Jacob Bigelow, M. D., of Massachusetts; L. A. Dugas, M. D., of Georgia.

Committee on Volunteer Communications.—Drs. C. A. Pope, Thomas Reyburn, John S. Moore, J. B. Johnson, and A. Litton, of St. Louis, Mo.

Committee of Arrangements.—Drs. J. R. Washington, J. S. Moore, S. Pollok, Thomas Reyburn, J. O'Farrar, W. M. McPheeters, C. W. Hempstead, and E. S. Lemoine, of St. Louis, Mo.

Committee on Publications.—Dr. D. F. Condie, Pennsylvania, Chairman; Dr. E. L. Beadle, of New York; Dr. A. Stille, Pennsylvania; Dr. I. Hays, Pennsylvania; Dr. E. S. Lemoine, of Missouri; Dr. G. Emerson, Pennsylvania; Dr. G. W. Norris, Pennsylvania. The report was accepted and adopted—and the nominations confirmed.

On motion of Dr. Watson, of New York, the name of the Committee on Volunteer Communications was changed to that of Committee on Prize Essays.

Dr. Wellford resigned his place as Chairman of Committee on Medical Education, and the President was authorized to fill the vacancy.

The vacancy was subsequently filled by the appointment of Dr. J. L. Cabell, of Virginia.

On motion of Dr. Atlee, the subject of proposed amendments to the Constitution was taken up, and the original articles, with proposed amendments, read by the Secretary.

Dr. A. H. Stevens, of N. Y., moved that the further consideration of the proposed amendments be indefinitely postponed.

Dr. Atkinson, of Va., offered the following, which was seconded by Dr. Mitchell, of Pa., as an amendment, that the further consideration of the proposed amendments be indefinitely postponed, except that portion relating to the admission of delegates from the army and navy.

After some discussion by Drs. Coolidge, U. S. Army; Bolton and Atkinson, of Virginia; Coventry, Stewart, and Watson, of New York, the amendment of Dr. Atkinson was adopted; and the section contained in article second, section nine of the proposed amendment was then adopted, which is as follows:—

Delegates representing the medical staffs of the United States

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Army and Navy shall be appointed by the chiefs of the army and navy medical bureaux. The number of delegates so appointed shall be four from the army medical officers and an equal number from the navy medical officers.

Dr. BOLTON, of Va., gave notice that at the next meeting of the Association, at St. Louis, he would propose the adoption of the amendments to the Constitution, submitted to this Association by the meeting at Richmond last year, and which have been here indefinitely postponed.

Dr. CONDIE presented the following amendment to the Constitution:—

Resolved, That the second clause of Article 2 of the Constitution be so amended as to admit the American Medical Society in Paris to representation in this body upon the same terms as the medical bodies in this country.

Dr. ALFRED STILLÉ, of Pa., Chairman of the Committee to whom was referred sundry memorials touching the course to be pursued by Medical Colleges and other Boards in the examination of candidates and the granting of Diplomas, reported, by submitting the following resolution for adoption:—

Resolved, That in order to preserve the purity and honour of the Medical Profession, and to place around young practitioners additional safeguards against temptations to do wrong, as well as to draw a more distinct line of separation between true and false physicians, it be and is hereby recommended, that every graduate in Medicine be required to subscribe a pledge to submit to the revocation of his diploma upon conviction of having knowingly violated the Code of Ethics of this Association. It is also recommended to the several Medical Colleges and such other Boards as are by law authorized to examine candidates for admission into the Medical Profession to require from every graduate or licentiate his signature to the Code of Ethics of this Association, and to furnish him with a copy of the same. It is further recommended that the formal administration of a pledge, faithfully to observe and keep the said Code, form part of the public exercises of Medical Commencements.

The report was accepted. A discussion ensued, which was participated in by Drs. PALMER, WINSTON, BOLTON, CONDIE and others, when Dr. STILLÉ proposed to withdraw the first resolution from the report. On the question being raised on the ability of the Committee to do so, the Chair decided that they had the right to withdraw a resolution after debate had been had upon it. After some further

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discussion, Dr. ATKINSON appealed from the decision of the chair; on putting the question the appeal was not sustained.

Dr. SAYRE, of N. Y., moved that the first resolution withdrawn by the Committee be taken up, and adopted as the sense of the Association. It was taken up, and referred to the Committee on the licensing power, Dr. WELLFORD, Chairman.

Dr. GEORGE J. ZEIGLER, of Pa., moved the following, which was laid on the table:—

Resolved, That a committee of three or more be appointed by the President, to devise or consider some comprehensive plan or system by which subjects connected more especially with Medical Science can be more speedily, systematically, generally, and thoroughly investigated and examined.

On motion of Dr. F. W. RODDEY, of Va., the resolution of Dr. ZEIGLER was taken up, and referred to a Committee, consisting of Drs. ZEIGLER, of Pa., RODDEY, of Va., and JACKSON, of Pa.

Dr. W. W. PARKER, of Va., moved the following, which was adopted, and referred to the Committee named in the resolution:—

Resolved, That the Standing Committee on the administration of anæsthetic agents during parturition, be instructed to inquire into all cases of death that may be reported as occurring from the use of anæsthetic agents during the present year in the United States, and report to the next meeting of the Association.

Dr. CAMPBELL, of Georgia, submitted a paper, on a question of priority in reference to the discovery of the reflex relation subsisting between the cerebro-spinal and sympathetic system of nerves.

Referred to Committee of Publication.

Dr. MILTENBERGER, of Maryland, offered the following resolution, which was seconded by Dr. ATLEE, and unanimously adopted:—

Resolved, That the cordial thanks of the American Medical Association be, and they are hereby tendered to the Committee of Arrangements, the profession, and the citizens generally of New York, for the generous and elegant hospitality extended towards its members during its present session.

Dr. W. HOOKER, of Ct., proposed a vote of thanks to the officers of the public institutions which had been thrown open for members of the Association during their stay. Adopted.

Dr. BOLTON, of Va., moved the thanks of the Association to the Press of this city, for its accurate reports of their proceedings. Adopted.

Dr. COX, of Maryland, proposed a vote of thanks to the officers

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of the Association, for the very able manner in which they have discharged their duties. Adopted.

The Members of the Association were then invited to meet on board the steamboat Hero, at foot of Pier No. 8, to-morrow morning, at 9 o'clock, and proceed to visit the public institutions belonging to the city of New York.

The President congratulated members on the close of their deliberations, and expressed his wish that they should have a safe return to their homes, and should all meet at St. Louis next year.

The Association then adjourned *sine die*.

EDWARD L. BEADLE,
EDWIN S. LEMOINE.

A SPECIAL Meeting of the members of the American Medical Association remaining in New York was held on the 12th of May, in the "Bleecker Street Presbyterian Church." Dr. Joseph M. Smith, of New York, was called to the Chair, and Dr. E. L. Beadle, requested to act as Secretary.

Dr. SMITH, on taking the Chair, stated that, by the request of a number of the members of the Association, the Secretary had called this meeting of the delegates resident in the city and its vicinity, and such as were remaining here from abroad, for the purpose of expressing their feelings respecting the late disaster on the New Haven Railroad, at Norwalk, in Connecticut, which resulted in the death of so many valued members of the Association, and that it was now for the members present to adopt such measures as they saw fit, to carry out the object in view.

Dr. WM. ROCKWELL, of New York, moved that a committee be appointed to draft resolutions expressive of the sentiments of the Association. The resolution was adopted; and the Chair appointed the following gentlemen to act as the committee: Drs. John H. Griscom, of New York; S. Hanbury Smith, of Ohio; P. Claiborne Gooch, of Virginia; L. A. Smith, of New Jersey; Theo. Goodloe, of Alabama; R. La Roche, of Pennsylvania; John Watson, of New York.

The Committee retired, and on their return presented the following:—

WHEREAS, amidst the wide-spread affliction caused by the recent catastrophe at Norwalk, Connecticut, the members of the American Medical Association, recognizing in this mournful event the hand of an all-wise Providence, feel called upon to express their grief at the sudden removal from life of Abel L. Pierson, M. D., of Salem, Massachusetts; Archibald Welch, M. D., of Hartford, Connecticut; Josiah Bartlett, M. D., of Stratham, New Hampshire; Samuel Beach, M. D., of Bridgeport, Connecticut; James M. Smith, M. D., and James H. Gray, M. D., of Springfield, Massachusetts; Wm. C. Dwight, M. D., of Moscow, Livingston County, New York, late members of the Association; and whereas it is the earnest desire of the members still present in the City of New York to record a suitable expression of their feelings upon an occasion equally unprecedented and distressing; therefore,

Resolved, That the members have received with profound sorrow the lamentable intelligence of the loss which the community as well as the profession

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has sustained by the death of so large a number of their fellow-members of the American Medical Association.

Resolved, That as a suitable, though inadequate external mark of their grief at the sudden demise of friends from whom they had so recently parted, the members of the Association in general are recommended to wear the usual badge of mourning for the space of thirty days.

Resolved, That the members of the Association most deeply sympathize with the relatives of the deceased in their bereavement, and that a copy of these resolutions, duly authenticated, be transmitted to their respective families.

Your Committee further recommend that a committee of seven be appointed to devise some suitable method of commemorating the event, and the worth and professional character of our lamented associates; and to recommend said plan to the next annual meeting of the Association.

On motion of Dr. Rockwell, of New York, seconded by Dr. J. R. Wood, the report of the Committee was accepted, and the preamble and resolutions adopted.

The recommendation contained in the report was also adopted, and the following committee appointed: Joseph M. Smith (chairman, on motion of Dr. Watson), Drs. F. Campbell Stewart, J. W. G. Clements, William Rockwell, Isaac E. Taylor, Edward L. Beadle, and John Watson.

Dr. Gooch, of Virginia, moved that the Secretary be instructed to transmit the minutes of this meeting to the Committee of Publication.

On motion, it was resolved that the proceedings be signed by the Chairman and Secretary, and published in the daily papers.

The meeting then adjourned.

JOSEPH M. SMITH, M. D., *Chairman.*

EDWARD L. BEADLE, M. D., *Secretary.*

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REPORT

OF

THE COMMITTEE OF PUBLICATION

AND OF

THE TREASURER.

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REPORT OF THE COMMITTEE OF PUBLICATION.

The Committee of Publication respectfully report:—

That of Vol. I. of the *Transactions*, there were on hand, at date of last report, 70 copies; received since 8, making 78.

Sold to members and others . . .	11 copies.
Remaining on hand	67
	— 78

Of Vol. II. there were on hand, at the date of last report, 24 copies; received since 10, making 34.

Sold to members and others . . .	5 copies.
Remaining on hand	29
	— 34

Of Vol. III. there were on hand, at the date of last report, 61 copies; received since 8, making 69.

Sold to members and others . . .	12 copies.
Remaining on hand	57
	— 69

Of Vol. IV. there were on hand, at the date of last report, 41 copies; received since 7, making 48.

Sold to members and others . . .	20 copies.
Remaining on hand	28
	— 48

Of Vol. V. there were printed 1000 copies.

Delivered to members and sold . . .	561 copies.
Presented to journals, etc. . . .	26
Remaining on hand	413
	—— 1000

The fifth volume of the *Transactions* was put to press at as early a period after the receipt from the Secretaries and Chairmen of Committees, of the minutes and several documents referred for publication, as the funds paid into the hands of the Treasurer would

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warrant, and the work was carried through the press with as much celerity as the character of much of the manuscript from which it was printed, the nature of its contents—including over one hundred pages of complicated tables—and the unavoidable delays always incident to the composition of an original work of over nine hundred octavo pages, would permit.

Complaints are annually made of the late period at which the *Transactions* of the Association are issued from the press; and censure, either direct or implied, is cast upon the Committee of Publication, who, it is believed, could, if they were attentive to their duties, issue the volume many months earlier in the year.

They who have not acquired from actual experience a knowledge of the difficulties and delays which are unavoidable in printing, in a proper manner, a work similar to that embracing the *Transactions* of the Association, it would be difficult, perhaps, to convince that the non-appearance of the volume at as early a date after the adjournment of the Annual Session as would be desirable, is not to be attributed to any neglect on the part of the Committee of Publication; yet such is actually the case.

The delay that has heretofore occurred, could, it is true, be in part obviated, were sufficient funds placed at the disposal of the Committee of Publication to warrant them in putting the volume to press immediately upon the adjournment of the Annual Session of the Association, instead of their being obliged, as at present, to wait some six weeks, before the necessary funds are received by the Treasurer.

The payment of an assessment of three dollars by one thousand of the permanent members, at, or immediately after the Sessions of the Association, would not only insure the publication of the *Transactions* at least two months earlier than they have heretofore appeared, but enable the members to be furnished with them without any augmentation in the price, notwithstanding the increased expense that will hereafter attend their publication, in consequence of the larger amount of matter to be printed, and the number of illustrations with which it is anticipated this matter will be accompanied.

Of the fifth volume of the *Transactions* the Committee decided to print an edition of only one thousand; still, the amount of funds received by the Treasurer has been insufficient to meet the expense by upwards of one hundred dollars. Had all the matter referred for publication been inserted, the deficiency would have amounted to a much larger sum. The reasons why the Paper of the

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late Dr. Drake, and the Report on the Cryptogama, by Dr. Porcher, were omitted, are set forth in the Preface to the *Transactions*, and it is believed they will be sufficient to justify, in the opinion of the Association, the action of the Committee in respect to them.

In consideration of the deficiency to be made up in the treasury, and the increased expense that it is certain will be incurred in printing the next volume of *Transactions*, the Committee are of opinion that the assessment of the present year should be raised to five dollars. They present, therefore, for the adoption by the Association the following resolutions:—

Resolved, That the assessment for the present year shall be *five dollars*.

Resolved, That the Committee of Publication be authorized to decide upon the terms at which the volume of *Transactions* for the present year will be furnished to the members of the Association and others, and that it shall be their duty to inform the members of the terms agreed upon by a circular.

Resolved, That the Committee be further authorized to take such measures in relation to the disposal of the copies of the *Transactions* remaining, after all such members are supplied as shall comply with the terms set forth in the circular of the Committee, as they may deem expedient.

Annexed is the Treasurer's Report, showing a balance against the Association, of *one hundred and seven dollars*.

D. FRANCIS CONDIE,
ISAAC HAYS,
GEO. W. NORRIS,
GOV. EMERSON,
Committee of Publication.

PHILADELPHIA, *April* 28, 1853.

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REPORT OF TREASURER.

The American Medical Association in Account with D. Francis Condie, M. D., Treasurer.

DR.

To amount of T. K. & P. G. Collins's bill for printing, paper, and binding Vol. V. of *Transactions*	$1546	74
" " " Th. Sinclair's bill for engraving and printing six maps	270	00
To cash paid for colouring maps	60	00
" " " Dr. Austin Flint, by award of Committee on Prize Essays	50	00
" " " Clerk hire, postage, express parcels, etc.	78	86½
	$2005	60½

CR.

By cash received from Dr. Isaac Hays, being the balance in Treasury, May 1, 1852	$56	75
" " received from Delegates, for assessments, and for sale of *Transactions*	1660	16
" " received from Dr. Geo. B. Wood, on account of Prize Fund	50	00
" " received from Messrs. Blanchard & Lea for *Transactions*, etc., sold by them	96	60
Balance due Treasurer	107	09
	$2005	60½

E. E. D. FRANCIS CONDIE, *Treasurer.*

PHILADELPHIA, *April* 27, 1853.

We have examined the above account with the vouchers, and find the same correct.

ISAAC HAYS,
GEO. W. NORRIS,
GOV. EMERSON.

April 27, 1853.

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ADDRESS

OF

BEVERLEY R. WELLFORD, M.D.

PRESIDENT OF THE ASSOCIATION.

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ADDRESS OF BEVERLEY R. WELLFORD, M. D.

PRESIDENT OF THE ASSOCIATION.

GENTLEMEN: Custom, not less imperious in its demands than stern necessity itself, requires that I should address you on the present occasion. Could I have consulted my own inclination, I would not have occupied one moment of the brief space of time to which your sessions are unavoidably restricted: but I may not disregard the precedent which has been established. I will not, therefore, apologize for appropriating the narrow isthmus which intervenes between the termination of one presidential term and the commencement of another: as the traveller pauses in his journey, to survey the pathway he has trod, and to glance forward on that which is still before him, we may not inappropriately avail ourselves of this, our resting-place, to look back upon the past, and to contemplate the interesting future of the American Medical Association.

Seven years have now elapsed since, at the suggestion of the New York Medical Society, a national convention of physicians assembled in this metropolis, to take into consideration the condition of the profession in the United States, and to adopt some concerted action. The medical community was then, as to a great extent it still is, a chaotic mass; containing, indeed, many gems of untold value—the materials of a new and beautiful creation—but so disconnected, disjointed, and disarranged, as to require the exercise of much genius, skill, and patience, in order to combine and arrange them to make each element contribute its quota to the utility and symmetry of the whole. The discordant character of the materials that were to be operated on; the various and conflicting interests which were to be reconciled; the difference of tastes, and habits, and motives to be amalgamated, so as to constitute one harmonious whole, presented a com-

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bination of difficulties apparently insuperable, by the unaided power of lofty, pure, and disinterested purpose. Under a despotic government, where the issue of an edict would have commanded the obedience of all whom it was intended to regulate, a well-digested system of requirement would at once have accomplished the purpose; but here no power existed to enforce a decree, and success depended on an appeal to the profession, not of a limited district, nor of a single government, but to that of thirty-one distinct republics, differing in climate, laws, habits, and institutions. Nor were these the only embarrassments. Medical education, under a vicious system of rivalry among the schools, was rapidly deteriorating; and the laudable exertions of the more eminent teachers were impotent to counteract the sordid efforts of minor institutions, resolved to sustain themselves, regardless of their own reputation, or the dignity and usefulness of the science they professed to teach. The honours and emoluments of medical professors had excited the ambition and cupidity of aspiring individuals throughout the country; charters were profusely granted by the different States; degrees were obtained with so much facility that many entered the profession without proper qualifications, either literary or moral, and the title of Doctor of Medicine, which had for centuries conferred on its possessor veneration and respect, was, in many parts of the country, shorn of its high and ancient honours; bestowing no enviable distinction, and meriting no regard.

Such were the circumstances under which the National Medical Convention of 1847 assembled in this city; and so apparent was the necessity for the convocation, that, although the invitation of the New York Medical Society was very imperfectly circulated, it was promptly responded to by the attendance of about one hundred delegates from nineteen of the States of the confederacy. Many and great as were the difficulties incident to the movement, they were boldly met, and the Convention

"*Resolved*, That it is expedient for the medical profession of the United States to institute a National Medical Association, for the protection of their interests; for the maintenance of their honour and respectability; for the advancement of their knowledge, and the extension of their usefulness."

For the support of these principles, a committee was instructed to report a plan of organization to a subsequent meeting, to be held in the City of Philadelphia the ensuing spring. At the appointed time, near two hundred and fifty delegates, from schools and societies of twenty-three States, assembled in Philadelphia. A plan of organi-

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zation was reported and adopted, which asserts that "an institution so conducted as to give frequent, united, and emphatic expression to the views and aims of the medical profession in this country, must at all times have a beneficial influence, and supply more efficient means, than have hitherto been available here, for cultivating and advancing medical knowledge; for elevating the standard of medical education; for promoting the usefulness, honour, and interests of the medical profession; for enlightening and directing public opinion in regard to the duties, responsibilities, and requirements of medical men; for exciting and encouraging emulation and concert of action in the profession; and for facilitating and fostering friendly intercourse between those who are engaged in it."

This, gentlemen, is the official announcement of the objects and aims of the American Medical Association; these are the principles it asserts, these the high purposes you are now assembled to advocate and advance. Your plan of organization will materially influence the achievement of your object. That which was originally adopted has already undergone some modifications; others are proposed, which it will be your duty, during your present session, to consider. It is not my purpose to discuss them, but merely to exhort you to great caution in the introduction of material changes. That the original constitution was in many respects defective, is doubtless true; nor is it surprising that it was so; imperfection was the inevitable consequence of the circumstances under which it was devised. No wisdom, however mature, could at once have originated a system competent to neutralize all the difficulties which then existed; indeed, it would appear almost a hopeless effort to suggest any plan which would equally meet the exigencies of the schools and the laity; of the city practitioner, and of him who labours in a remote and thinly populated region; of the slave districts of the South, and the manufacturing ones of the North; of the States where there are laws regulating the practice and of those where there are none. In adopting fundamental rules to which all are expected to yield obedience, it was necessary to respect each of these conflicting interests, and, as far as possible, to conciliate and to harmonize all. The same principle applies with equal force to any alteration or amendment which may now be proposed. As the Association advances to the consummation of its purposes, circumstances will be modified, original necessities will no longer exist; others will arise, experience in the working of our system will be acquired, and, of consequence, our organic law must be occasionally changed and adapted to circumstances, as they may

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exist from time to time. I am not, therefore, opposed to all alteration; but I pray you to make it cautiously, wisely, and with deliberate foresight into the effects it will produce. Let us carefully avoid all desultory and useless innovation, and, with equal prudence, eschew all sudden and radical change. Let us seek reform, not revolution; and let every reform emanate from calm conviction of its practical efficacy, not from a restless disposition to indulge in wild and heedless experiment. Impatience to arrive at final results, is a common error to which the American character is peculiarly obnoxious. To "learn to labour and to wait," is always a difficult lesson. If we do not immediately realize all that we desire, we are prone to attribute the delay to a defect in our system, when it is in truth an intrinsic element of the enterprise itself. Our Association is still in its infancy, and cannot reasonably claim the matured and perfect organization of adult manhood. Patient experiment, and cautious alteration, where alteration is necessary, will finally effect all that organization can effect, and effect it safely. Even in the limited period of human life, six years is not a moiety of the season of preparation for the active duties of intellectual pursuits. It is unreasonable, therefore, to expect that an institution like this, which, it is hoped, will live and diffuse its blessings through all coming time, should, in so brief an existence, have triumphed over difficulties numerous and peculiar, and be already at the flood-tide of successful experiment. That it has not yet fulfilled its whole design, is readily conceded; but when its enemies predict its final failure, we may confidently refer them to what it has already accomplished, as the best omen of the falsehood of the prophecy. Such individuals very imperfectly appreciate the effects of moral power; of the stern, determined, persevering effort of a body of able and influential men, engaged in a great and noble cause, and resolved on success. Their progress may at first be slow—it may be, to the timid and irresolute, discouraging, but in each successive year they will gain a firmer hold on the omnipotence of public opinion, until, like the avalanche of the hills, they will sweep from their pathway every obstacle to their triumphant march.

Whoever, then, will contemplate, in a spirit of calm philosophy, the objects and aims of the American Medical Association, the vast boundaries of its operations, and the obstacles which oppose its success, must perceive that both time and effort are necessary to achieve them. But there are those who affect to believe that its course has hitherto been imbecile, and who taunt its advocates with the assertion

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that it has yet effected nothing. Is it nothing to have aroused attention, throughout the length and breadth of this confederacy, to the imperfections of medical cultivation? Is it nothing to have demonstrated the necessity of raising the standard of medical acquirement to that of other civilized countries, and of rendering medicine here, what it is elsewhere, a science of the purest and most elevated character? And this not only in cities and districts of limited extent, where a dense intellectual population has always demanded scientific attainment as an indispensable preliminary to personal consideration and success, but in the interior of the country, where too frequently the mere title of Doctor has been, with the multitude, all that was required, and the same respect and confidence has been accorded to the most accomplished and the most ignorant. If any one doubts that such effects are being developed by the influence of this Association, let him go to the villages and crossroads of the country, and hear the opinions expressed by those who have heretofore been indifferent to the subject, only because their attention had not been directed to its importance, but who now appreciate it, because they learn, from the discussions of medical men, that it is something more than a trade, the stock of which consists in a collection of old women's recipes, as sovereign as "parmaceti for an inward bruise." Let him look at the number of local societies recently established, at the State societies originated or reorganized in various localities. Does not this indicate, nay, does it not prove, that both public and professional interest is already excited, and, as revolution never goes backward, afford to the medical philosopher and philanthropist a cheering assurance that his favourite science will speedily assume an honoured position among her more prosperous sisters?

The influence exerted by this Association has been manifested not only by the people and the profession, but has been acknowledged by our legislatures both State and Federal. At an early meeting, the Association "recommended and urged upon the various State governments the adoption of measures for procuring a registration of the births, marriages, and deaths occurring in their several populations." The limits I have assigned to this address, will not permit me to enlarge upon the benefits which will result to the people of these States, from the passage and execution of a law of this character. Nor is it necessary. The interests connected with the "three important eras of human life—birth, marriage, and death," render it apparent on the slightest reflection, that "upon the circumstances connected with them are dependent, to a very great extent,

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the physical, moral, and civil condition of the human family; and that a knowledge of these circumstances is necessary for a full comprehension of important means for the certain advancement of the population of States in prosperity and civilization."* Accurate information on these subjects can only be obtained by legislative enactment. It is of inestimable value to the political economist, the legislator, the statesman, the medical philosopher, indeed, to all who would base practical efforts for the alleviation of physical and moral evil, and the promotion of national prosperity and happiness, on the solid foundation of material and authentic facts. In obedience to this recommendation of the Association, and in consequence thereof, the Convention which recently set in Virginia, for the purpose of amending her constitution, made the passage of a registration-act obligatory on her legislature. In compliance with that requisition, a law was passed at its late session, the first under her new constitution, fully executing the intentions of the Association. Other States may have passed similar laws, but of their proceedings I am not informed.

Nor has the action of the federal government been less respectful to the wishes of the Association. At the meeting in Baltimore, in 1848, its attention was directed to the immense amount of adulterated and sophisticated drugs prepared in foreign countries and imported into our own: a memorial to Congress for the passage of an act on this subject was prepared and transmitted to Washington. The memorial was also referred to, and contained in, an elaborate and eloquent report, made to the House of Representatives, by Dr. T. O. Edwards, member of Congress from Ohio, chairman of a special committee. Congress promptly responded to the recommendation of the Association, by the passage of an act to prevent the importation of adulterated and spurious drugs and medicines—approved the 26th day of June, 1848. As suggested by the Association, inspectors were appointed, and the passage through the custom-house of any drug or medicine so adulterated or deteriorated as to be inferior in strength or purity to the standard established by the United States, Edinburgh, London, French, and German pharmacopœias and dispensatories, positively prohibited. The wisdom and salutary effect of this law is clearly evinced by the statements of Dr. M. J. Bailey, Examiner of Drugs at the New York Custom-House, who in the report of 1849, to this Association, is said to be equally distinguished for his success

* See proceedings of the National Medical Convention, p. 127.

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in detecting and exposing the frauds that were practised, and for his fearless and faithful manner of executing the laws enacted for their prevention. The Report proceeds to say that "according to this gentleman's statement, the amount of drugs, medicines, chemical preparations, &c., entered at the New York Custom-House, during the year 1847, was nearly one million and a half of dollars. More than one half of many of the most important chemical and medicinal preparations were worthless and often dangerous. Within the short space of about five months after the law, petitioned for by this Association, went into operation, the same examiner, Dr. Bailey, condemned and rejected, as adulterated or worthless, no less than 13,000 pounds of rhubarb, 2,500 pounds of opium, 7,200 pounds of jalap, 1,414 pounds of gum gamboge, 1,400 pounds of senna, 30,000 pounds of spurious yellow bark, 3,000 pounds of iodine, 1,700 pounds of gum myrrh, all of which, but for this law, would have found their way throughout our extensive country, and into the stomachs of the sick and suffering. It has justly been remarked by a distinguished senator in Congress, that if the National Medical Association achieved no other reform than this, its labours would entitle its members to the gratitude of the country. The beneficial effects of the law have certainly surpassed the expectations of its friends."* The honourable and distinguished Dr. T. O. Edwards, employed by the Secretary of the Treasury, to visit the principal ports, and ascertain the working of the law, reports its results to be: 1. An elevation in the quality and purity of the medicinal agents imported. 2. An entire prevention of adulterated and deteriorated drugs, &c., from entry and use. 3. No embarrassment to the honest importer and dealer. 4. An increased revenue. 5. Protection to the medical profession and community; an increasing confidence and an earnest desire on the part of the people for the continuance of the law and its faithful application. Dr. Bailey says that it "has prevented foreign consignments to agents in this country, insomuch that not one-tenth of the spurious and adulterated articles arrive in the port of New York that there did a year since, before the passage of the law." These beneficial effects have continued, and have become more and more decided, until spurious and adulterated articles are almost entirely excluded from our markets, with the exception of those of home manufacture. Even these have not increased, and the reason assigned is, "that the fact that Congress had passed such

* See Dr. Parsons's Report, Transactions Am. Med. Assoc. 1849, pp. 655–6.

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a law has drawn public attention to the whole subject, in such a manner that home adulterations cannot be practised with so much impunity as formerly."* Nevertheless, it must be conceded that home adulteration is still perpetrated to a very injurious extent, and will, unless arrested, go on to increase. I shall, hereafter, suggest action on the part of this meeting on the subject.

The protection of the interests of the medical profession and the maintenance of its honour and respectability, were not, however, the sole incentives to the institution of the American Medical Association. It also contemplated the advancement of its knowledge and the extension of its usefulness. And can it be said that it has done nothing to promote these high and laudable objects? Look into the published *Transactions*, and then say whether in learning, in ability, and originality, the essays and reports would not be worthy of an honourable place in any similar volume. The plans recently adopted for the purpose of encouraging research and the investigation of special subjects, have already elicited some papers of distinguished merit; and I hazard nothing in the prediction, that the contributions they will hereafter supply to medical literature, will be an emphatic rebuke to those who would restrain our onward progress, by unfounded and injurious remark. Still in its minority, our institution has not as yet perfected even its plans for developing the wealth which lies imbedded beneath the rough surface of the medical mind of the United States. We have, as yet, only its surface washings; metal pure and bright, and of the finest carat, but in quantity a mere indication of unknown treasures in the veins, of which it is the outcrop. But the sinking of the grand shaft has been commenced, the matrix of the vein has been already pierced, and the priceless ore, sparkling with glittering gems, but still obscured by admixture with less valuable material, is presenting itself for reduction and refinement. When our shaft shall have penetrated farther and deeper into its recesses; when our tunnels and adits are completed, our machinery arranged, and our furnaces in blast, the American Medical Association will proudly exhibit its ingots of mental gold, the rich result of its labours, and the glorious reward of its elevated and disinterested enterprise.

But while some content themselves with the remark that the Association has, as yet, effected nothing; others, more censorious, affirm that it has injured the profession in public estimation, by exposing

* See Dr. Huston's Report, Transactions Am. Med. Assoc. 1850, p. 297.

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its deficiencies, and has thus induced the patronage of Homœopathy and other delusions of similar character. This assertion is by no means admitted to be true. It is, to say the least, a sort of *post hoc, ergo propter hoc*, mode of reasoning, which is so apt to be fallacious, that we may be permitted to doubt the philosophy, even if we admit the fact. The solution will more probably be found in the existence of the deficiency than in its disclosure. The Spartan code of morals may not be adopted as our model; the censure should be applied to the fault, not to its detection. Frank and manly candour need never fear results; and admitting that the fact be as stated, and the causation logically and fairly deduced, it is better that the temporary evil be submitted to, than that it be perpetuated by concealment, which

> "Will but skin and film the ulcerous place;
> While rank corruption, ruining all within,
> Infects unseen."

We should esteem him a bad surgeon, who would permit a local malignant disease to contaminate the system, and destroy life, lest the decisive use of the knife should cause a frightful but temporary wound.

But, gentlemen, if it be nothing to have reduced an amorphous, chaotic, professional mass to something like symmetry and order; nothing, to have awakened attention to important defects in preliminary and medical education, and, in some measure, to have removed them; nothing, to have caused the establishment and reorganization of local and State medical societies; nothing, to have caused the enactment of wholesome laws, both by the Federal and State Governments; nothing, to have produced scientific papers of acknowledged ability and erudition; nothing, to have awakened dormant talent, and elicited a vernal bloom, which promises a rich harvest of autumnal fruit; nothing, to have acquired a reputation, which renders even its membership a title of honour and distinction; is it also nothing, by unaided, intrinsic moral power, to have bound in one code of medical ethics, thousands of men in every section of this wide Union, each one free to act according to his own individual views, but yielding them in cheerful obedience to the opinions of the Association, with as much deference and submission as if it were armed with the power and the terrors of penal law? Is it nothing, unendowed with the compulsory authority of legal enactment, or the seductive influence of mileage and per diem pay, annually to convoke such an assembly, as I have now the honour to address? Gentlemen, such a congress, of

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such materials, and under such circumstances, presents a spectacle of moral beauty, to which the opponents of medical reform cannot be insensible. It must command their respect and admiration, even if it fail to secure their co-operation. I know not, gentlemen, what may be the effect on others of an occasion like this, but for my single self, when I thus recognize the denizen of the city and the forest; him of the frozen north and of the sunny south; him of the mountains, the rivers, and the prairies of the west, with him from the borders of the broad Atlantic, bringing their various opinions and prejudices, and casting them together, as a sacrificial offering on the altar of science and professional patriotism; when I see them animated by the same honourable and lofty impulse, and, in fraternal harmony, uniting their efforts to attain the same grand results, I feel an honest pride in my profession and my country, and an abiding confidence, that, to such sons, the high destinies of both may be safely intrusted.

It may, then, be fairly assumed that the American Medical Association has thus far succeeded to an extent fully as great as ought to have been anticipated. Nevertheless, it is admitted that much remains to be effected before its power is fully developed, or its objects and its aims are fully attained. Dependent as it of necessity is on the co-operation of the individual members of the profession, it is not possible that the Association can consummate its designs, or attain the zenith of its power and influence until the medical men of every neighbourhood are formed into local societies, and State societies are organized in all the States of the Union. The more uniform the organization, the more effective it will prove in its practical working, and uniformity can only be obtained by some plan of general application recommended by this body. The central orb of medical organization in the United States, it must throw its light on the periphery before the reaction can be felt, and the concentrated influence of the whole profession act as a combined and powerful unit. We have the material—abundant, effective material—but as yet unavailable, and, to a great extent, unappreciated, because it has not yet been illuminated by the light of medical progress. That light must emanate from this Association, and it will be reflected and thrown back upon its parent from every hill and valley in the land. Go to yonder heights, while darkness yet rests on the face of the deep; no object meets your gaze, no sound salutes your ear, save the music of the waves as they fall in measured cadence on the shore. Unacquainted with your position, would you conceive the scenes of wealth, and

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beauty, and grandeur that repose around you? In the gray twilight of the morning, as the sun approaches the horizon, bronzing the light mists which float in the eastern atmosphere, each moment spreading his radiance from cloud to cloud, you perceive that you stand amidst a landscape teeming with every constituent of natural and artificial loveliness. But it is not until his risen beams glow on the remotest verge of the horizon, and are reflected back in their thousand gorgeous hues, that the full glory of the scene is disclosed to your view. Then you observe the majestic ocean, bearing on its bosom the treasures of every nation of the earth; its green islands bristling with the implements of war; the proud metropolis, with its busy hum of men, its spires, and domes, and castles; the forest, tinted with the verdant hues of spring; the fields, the rivers, and the distant mountain, each contributing its quota to the magnificent panorama which lies before you. But how was this scene, so full of darkness and obscurity, changed into the spectacle of bright and glorious day? The material was there—the ocean and the city, the hills and the vales, and every other component of the enchanting whole—unseen, unappreciated, unknown, until the magic power of light displayed each object, and combined them all in one harmonious assemblage of associated beauty.

Such, gentlemen, has been, and still is, the condition of our profession in these United States. The material is here, the genius, the talent, the labour. It only requires the animating influence of this Association to display its resources and develop its power, to confer honour on itself and blessings on mankind. It has been my fortune to meet with and to know many of our professional brethren engaged in practice in obscure country situations; and I know that there are among them talent, and sagacity, and practical information, which, under more auspicious circumstances, would shed lustre on their names and calling; but, unfortunately, their path of life lies among counteracting influences which prevent the full evolution of professional enthusiasm. In the cities, the perpetual collision of intellect and the pleasures of scientific intercourse excite and maintain an unceasing interest in everything connected with the subject of medicine. Each physician has his circle of professional friends, all engaged in the same pursuits, familiar with the same subjects, and animated alike by the same impulse. The discussions in their medical coteries, the books they read, the occurrences at the hospitals, and the incidents of private practice, always afford interesting topics of conversation, to the exclusion of the ordinary subjects of popular

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excitement. The country practitioner is less advantageously situated. In his daily associations, he rarely meets with those who sympathize with his professional anxieties, and were he habitually to introduce them as subjects of conversation, he would of course realize no satisfactory response, and be regarded as a pedant and a bore. He may be permitted to assert that the doctrine of the circulation of the blood is orthodox, and to maintain that vaccination is preventive of smallpox, and perhaps, if he is sufficiently cautious, even to resort to auscultation as a means of diagnosis; but if he attempt to introduce any of the more recent improvements in diagnosis or practice, he is most happy if he escape the denunciations of the public, and of his less enterprising brethren, as an experimenter on the health and welfare of his patients. His ardour for improvement thus checked, he naturally seeks other subjects of intellectual pleasure, and is forced into the current of popular excitement, whatever it may be, and is insensibly led to feel more interest in farming, in neighbourhood gossip, and in that infallible substitute for all healthy mental occupation, politics, than in aught connected with medicine. Newspapers, speeches, political meetings, and party, are the subjects discussed by those with whom he associates, and they of course soon occupy thoughts and feelings which, under other circumstances, would be more agreeably and more profitably directed. Let not this by the more fortunate class of practitioners be made a subject of reproach. Man is at best but a sort of mental chameleon, and "*conditione mutata de te fabula narratur.*"

While, therefore, it would be utopian to expect to devise any plan which could place all the advantages of the city in the possession of the country practitioner, a very desirable improvement would be attained by the universal establishment of local societies, which would at least have the effect to associate the medical men of the country at intervals, and supply them with materials for thought and conversation, excite an *esprit de corps*, and foster a taste for subjects connected with professional advancement. The general diffusion of local associations would necessarily increase the prosperity of the State societies, and these, in turn, acting on this Association, would, I trust, finally effect that object so essential to medical reform, the universal organization of the physicians of the United States; so that each one should constitute an integral part of the great whole. The more uniform the organization, the more effective will be its practical working, and uniformity can only be obtained by suggestions emanating from this body. In very many instances, a form of

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organization carefully prepared by those familiar with such matters, would materially promote action, as inexperience is in many parts of the country one of the great obstacles to the formation of societies. Allow me, therefore, to suggest that, at the present session, a committee be assigned the duty of preparing a form of organization of both local and State societies, and that its general adoption be earnestly recommended.

Another topic of primary importance, to the advance of medical reform, is a proper regulation of the license to practise. The peculiar character of our system of government places this subject exclusively under the control of the State sovereignties, and to them we must appeal for the necessary legislation. In a very large proportion of the States, the diploma, which should be regarded merely as a collegiate honour, is virtually the license to practise. In truth, no license whatever is required, and, hence, whoever chooses to assume the title of Doctor of Medicine, is permitted to practise, or, rather, any individual, however ignorant, who offers his services to the public as a practitioner, has the title at once conferred, and is forthwith invested with all its privileges and immunities. The necessary and practical consequence is, that the intelligent and trustworthy, as well as the unqualified graduate, the Homœopathic, the Thompsonian, and all other medical heretics of whatever faith, are confounded under the same designation, and medicine, as a science, is held responsible for the effects of doctrines and practice which she repudiates and abhors. Common justice would accord to her the right to exclude from her honours, those at least, who wear her livery without acknowledging her authority, and to claim of the State governments some mode of distinguishing the Doctor of Medicine from the pretender and the charlatan. I know of no plan by which this can be effected other than by a law in each State establishing a Board of Medical Examiners, to whom shall be confided the exclusive power to grant the license to practise within her limits. It should be required to subject each candidate to a rigid examination, and forbidden to grant its certificate to any applicant who was found to be imperfectly qualified. The Board should be unconnected with any school of medicine; but no candidate should be admitted to examination, excepting regular graduates of some respectable institution, nor should even the diploma be recognized, unless it was conferred by a school teaching a full curiculum, and demanding terms of study of sufficient duration to authorise the belief that the diploma emanated

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from a worthy source, and was bestowed on a worthy recipient. The beneficial effects of a law so framed would be threefold. It would establish a marked line of distinction between the regular and the counterfeit physician; it would make the reputation and prosperity of a school dependent on the proficiency of its pupils; and it would teach the pupil to understand that his permission to practise depended on his attainments, and not exclusively on his diploma. A fair and honourable competition would be created among medical institutions. They would graduate only those who were really fully instructed; because that institution, whose pupils were frequently rejected as incompetent by the examining board, would be deserted in favour of those whose teachings were more perfect. The diploma would be of no practical value, unless ratified by the licensing board, and the inducement to grant it unworthily would not only be annulled, but every untrue certificate of qualification would be positively injurious to the reputation of the institution from which it emanated. As the diploma, moreover, would be of no value to the student unless deservedly conferred, it would be earnestly sought only as the necessary prelude to his ultimate object, the license to practise; and the same motive would induce him to matriculate in that school which would offer him the fullest instruction, not the easiest access to the degree of M. D. An examining board, therefore, in each State, demanding the diploma, would exert a salutary influence on the schools as well as the pupils, and its license would be to the public a certificate that the recipient was a worthy graduate in medicine, was qualified to practise his profession, and was, in truth, what he professed to be, a Doctor of Medicine in its pure and ancient signification. I am aware of the many difficulties which oppose the success of this measure. If it be deemed impracticable, I trust that some other more feasible and equal to the emergency, will be suggested. Deeply impressed with the magnitude of the evil, I beg leave to call the attention of the Association to the subject, and if it be considered expedient to invoke State legislation, to propose that a committee be appointed to investigate it, to report some efficient plan, and to draft a bill for the purpose, that the laws of the several States may be as uniform as possible.

I have already alluded to the domestic adulteration of drugs and medicines. The federal law appears to have achieved all, or nearly all, that could be desired or expected from that source, and rigidly administered will continue to protect the community from foreign fraud.

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The profit accruing from this species of dishonesty, however, appeals to the cupidity of unprincipled dealers so strongly; the fraud is so easily perpetrated, and detection, without the intervention of law so difficult, that so long as it is not liable to punishment, it will prevail to a greater or less extent. Accordingly, we find that such is now the fact, and unless restrained, it is to be apprehended that the benevolent influence of the federal law will after a time be entirely neutralized by American manufacturers. The general government can do no more in the premises. The States and the States only can complete the work so auspiciously begun. An appeal to them from an assemblage like this, of physicians from every section of the Union, would not be disregarded. I desire, therefore, to commend the subject to your consideration, that you may adopt such measures as will abate the evil, either those recommended by Dr. R. M. Huston, in his report of 1850; the invocation of State legislation, or such other expedients as in the wisdom of the Association shall appear most likely to accomplish the object. Any notice of the frauds practised proceeding from this body will have a salutary effect by its commanding influence on public opinion, and the consequent notoriety it will stamp on the mischievous deception.

At the last meeting of the Association, a resolution was adopted, directing the appointment of one member from each State represented, to travel in Europe, and to report on foreign medical affairs. By a subsequent resolution, the President was empowered to make the appointments at any time during the year. Under these resolutions but one appointment has been made, Dr. John F. Lamb, of Pennsylvania. No other nomination was made, and I was aware that it would be fruitless to make appointments unless the party was already in Europe or contemplated a visit thereto. The terms of the resolution restricted me to the members of the Association, and I did not feel at liberty to adopt any latitude of construction. I should otherwise have been enabled to make some appointments which would doubtless have elicited valuable reports, but which I felt compelled reluctantly to withhold.

Dr. Wm. M. Boling, of Alabama, Chairman of the Committee on Epidemics of South Carolina, Georgia, Florida, and Alabama, and Dr. Robert W. Haxall, of Virginia, Chairman of the Committee on Epidemics of Virginia and North Carolina, have resigned their respective appointments. The former vacancy has been filled by the substitution of Dr. D. J. Cain, of South Carolina, who has accepted

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the trust: that arising from the resignation of Dr. Haxall has occurred very recently, but has been supplied by the appointment of Dr. J. F. Peebles, of Virginia.

A melancholy duty still remains, "for I am sent to you with heavy tidings." Since our last meeting death has been in our midst, and has robbed us of those who then participated in our counsels, and devoted their influence and their gifted intellects to the advancement of medical reform. We had hoped to have met them here on the present occasion, to have enjoyed with them the pleasures of social intercourse, to have profited by their wisdom, and been cheered by their countenance; but their vacant seats renew our sorrows and afford another illustration of the sad truth, that "Hope is the gay to-morrow of the mind, that never comes." The calm and philosophic DRAKE, the eminent and learned HORNER,

> "Have given their honours to the world again,
> Their better part to Heaven, and sleep in peace."

alas! gentlemen,

> "Earth's highest station ends in 'Here he lies,'
> And 'dust to dust' concludes her noblest song."

> "The boast of heraldry, the pomp of power,
> And all that genius, all that wealth e'er gave;
> Await alike the inevitable hour—
> The paths of glory lead but to the grave."

But, gentlemen, though dead, they yet speak. They speak to us by their deeds, by their example, by the evidence they have left of devotion to the great cause in which you are engaged. Could their spirits hold communion with yours, they would still urge you forward to the goal of your aspirations; they would tell you that you are engaged in the cause of benevolence, of patriotism, of humanity; that you are seeking to ennoble and to improve the highest and most beneficent charity that ministers to the temporal happiness of man; to diffuse its blessings on your country, and to carry to the humblest cottage in the land the consolations of the same succour in distress which wealth and power can command. Gentlemen, we have lost some of our brightest jewels, but their lustre yet lingers in our pathway, and others still remain, which, like the books of the Sibyls, become more precious as time and circumstance diminish their number.

There is a period in official, as in natural life, when,

> "They say, the tongues of dying men,
> Enforce attention, like deep harmony."

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That period has now come to me, and I would employ its last moments in earnest exhortation to press forward to the consummation of your enterprise; under a solemn conviction that in protecting the interests, maintaining the honour, advancing the knowledge, and extending the usefulness of the profession of medicine, you are sustaining the dearest concern of society, of your country, and of mankind.

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REPORT

OF THE

COMMITTEE ON MEDICAL EDUCATION.

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REPORT OF THE COMMITTEE ON MEDICAL EDUCATION.

THE undersigned, members of a standing committee, being required by a rule of the National Medical Association to prepare a report on the general condition of medical education in the United States; to compare it with the state of medical education in other enlightened countries; to notice the courses of instruction; the requirements for graduation; the modes of examination for conferring degrees; the number of pupils and graduates at the several medical institutions in the United States; to notice, also, the prerequisites to appointment in the medical staff of the Army and Navy; the legal requirements exacted of medical practitioners in the several States; and all such measures, established or prospective, in reference to medical education, and the reputable standing of the profession, as may be deemed worthy of special consideration, beg leave to submit the following remarks, made in compliance therewith, as their Report.

On turning to the records of the Association, the Committee have had the gratification to find that the ground assigned them to occupy, has been so faithfully cultivated by their predecessors, that there remains but little for them to do. A brief analysis of the reports heretofore submitted to the Association, aiming, as they all do, to advance the dignity and augment the legitimate influence of the profession of medicine in the United States, might seem to be a sufficient compliance with the requirements of the rule under which the Committee are called to act; but as each of their predecessors, notwithstanding the unity of their purpose, have expressed opinions, in some respects, distinctly, it has been thought best, by your Committee, even if the *Transactions* should appear tautological, to present their own views on some of the subjects referred to them, and leave to the Association the responsibility of their repetition.

In one or another of the reports on the general subject of medical

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education, ranging from 1847 to 1851, careful and accurate statements on the several subdivisions which follow have been made, to which we beg leave to refer, in order to save the necessity of recapitulation.

1. The number of medical schools in the United States.
2. The number of professors in each school.
3. The length of the lecture term in each.
4. The qualifications or prerequisites to admission.
5. The qualifications of candidates for degrees.
6. The number of students in attendance.
7. The ratio of practitioners to the population.
8. The names of those States which have and those which have no laws on the subject of medicine.
9. The effect of repealing the statutes in those States where conservative laws once existed in relation to medical practitioners.
10. A comparative view of the colleges of medicine in the United States and in Europe; of the preparatory training to which medical students are subjected on the two continents and the islands of Great Britain; of the subjects they are required to study after matriculating; the relative length of their terms of pupilage; and the relative number and rigor of the examinations to which they are subjected after their course of instruction has terminated.

The latter topic was fully discussed by the Committee for 1849, and was presented to the Association in a manner well calculated to arouse the pride of the profession in the United States, and intended evidently to stimulate that sentiment, so as to bring its power to bear in such a manner as would erase the evidences of the disparity which confessedly exists between our own schools and those of the continent of Europe. A disparity which has no reference to the character of our public teachers, but grows out of the difference in the political organizations or governments of the different countries in which the schools are situated. A strongly conservative government, which exercises an attribute of sovereignty in restraining unqualified persons from assuming to exercise the functions of a physician, has a right also to require the highest order of qualifications from those to whom it accords that privilege, and the right of exacting a pecuniary *honorarium* therefor. Very different is the case where sovereignty resides in the masses, and where that sovereign is morbidly jealous of everything to which the idea of monopoly can attach. Hence it is important to keep in mind that there is some relation between

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the government of a country and the practice of medicine therein, in all that we propose to do to increase its respectability.

Although the report, just quoted, was drawn up with great care and ability, and was the result of laborious inquiry, and embodies a much greater amount of information than either of the others, it will be found that each one possesses merits peculiar to itself; some dwelling most upon the facts of history, whilst others have ventured more widely into the region of opinions, and the discussions necessary to their elucidation. All, however, tend to one point; the aim of all being, through the instrumentality of this Association, to arouse the profession of the country to such an effort as shall shake from itself the reproach of ignorance and of quackery, by which it has been humbled in its own estimation, and abased in that of its fellow-men.

Your Committee are pleased to bring, from the different sections of the country in which they reside, gratifying assurances, that the labours of this Association have thus far been fruitful of good results. The voluntary principle in its organization, and its representative character, increases from year to year its moral power and influence, which have been made manifest in the increase of State and County societies, and in the efforts which many of the medical schools have made to conform to its recommendations.

With a view to remedy the evils that are admitted to exist in our system of medical instruction, the following suggestions have been offered by previous committees:—

1. That there should be made, in all cases, by those designing to become medical students, a more extended and thorough preparation for entering upon professional studies than is usual in the United States.

2. That all candidates for the degree of M. D. shall have studied three years, including the time allotted to attendance upon lectures.

3. That they shall have devoted at least three months to dissections, one term to hospital practice, and

4. Shall have attended two full courses of lectures.

5. That the faculty of the several medical colleges insist upon the fulfillment of these requirements, and upon a faithful attendance by the students on the lectures of the professors.

6. That candidates for degrees be examined at the close of the first term in certain specified studies; the effect of which would be, practically, to divide the studies into those appertaining to a junior and a senior year.

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7. That the number of professors be increased to seven, and the lecture term extended to six months.

8. That measures be adopted to prevent the multiplication of medical schools.

9. That the business of teaching be conducted by bodies distinct from those who may be authorized to grant licenses or diplomas.

The foregoing is an epitome of the subjects which the several committees, in whose footsteps we are compelled, in a great degree, to walk, have presented to the consideration of the Association from year to year, and, by it, has been laid before the profession of the United States.

Of the opinions that have been expressed, and not embodied in the form of distinct propositions, for the adoption of the Association, it is not so easy to give an abstract.

In 1848, an invitation was given by the Association to all the Medical Institutions in the Union, to forward each year, to the Chairman of the Committee on Medical Education, a copy of their catalogue and the annual announcement of their plan of instruction for that year. It is to be regretted, that this invitation has not been so far complied with, as to enable the Committee to show what changes have been made by the medical schools, or what advances they have made towards the realization of the ideal of the National Association.

Enough, however, has been received to authorize the Committee to say, that there is a general desire, not only on the part of the schools, but in the great body of the profession, to come up to the national standard. This disposition has manifested itself in various ways. Some of the colleges have lengthened their regular term of lectures, or created new professorships, and added new subjects to the curriculum of studies. Others have established preparatory or supplementary courses of lectures as a means of meeting the new demand made upon them, in consequence of the agitation of this subject, since the organization of the National Association. The private practitioner has taken new views of duty, and refused to admit into his office the unqualified applicant, and the local societies have voluntarily erected barriers to his taking the initial step in that direction.

Yet, notwithstanding the vigorous efforts already made, and still in action, judiciously designed to elevate the character of the alumni of the various schools in the country, the number of those who fill the ranks of the profession without education is not perceptibly diminished. For this evil, in the present state of public sentiment, and in the absence of laws, which are but organizations of public

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opinion, for the prohibition of quackery, there seems to be but one effective remedy. Although the education of those who flock to the public schools may become more general, and even more thorough than at present contemplated, so that the professional character of the pupils may reflect honour upon their alma mater, there will be found, in the same theatre of action, another set of men, naturally as astute, perhaps, who, knowing the worth or pecuniary value of a popular, in contradistinction to a professional reputation, successfully appeal to the prejudices of that public whose higher judicial tribunals confirm in every man the right to assume the title of Doctor, and his equal right to demand and receive the *honorarium* due to medical attainment and professional skill.

Your Committee, as they conceive, have sufficiently elevated ideas of the mission which the medical colleges now in being are designed to fulfil, and feel an honourable pride in tracing their professional paternity to this source. They also feel, that one thing more is wanted as a remedy for the evil just referred to, and that is the establishment of free colleges for the preparatory and professional education of the young men now scattered over the wide and half-cultivated domain of the West.

To show that such a measure can be carried into effect, notwithstanding the popular character of our political institutions, the Committee take the liberty of alluding very briefly to the organization of the University of Michigan, which is the only institution in the country endowed by Government and regulated by State authority. By the medical faculty of the university, all candidates for the degree of M. D. are rigidly required to comply with the conditions of the National Medical Association, except attendance upon hospital cliniques, and have been notified that, in the future, it is in contemplation to exact the same preparation for admission to the medical as is now prescribed for membership of the department of arts. As an incentive to the prosecution of classical studies, one year will be deducted from the term of medical studies of all students who graduate first in the department of arts.

The lecture term in the college of medicine of this university extends from October to April—seven months. During this time, there are five professors daily or continuously on duty, who are restricted to four lectures each day. Daily examinations are made on the subjects of the day preceding. The pupils are divided into two classes, seniors and juniors; and the seniors into two sections, one of which reads essays on medical subjects every Saturday. The

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juniors may engage in this exercise, and, being stimulated by the example of the seniors, many of them do so, to their obvious advantage. At the close of the first term, those who design to graduate at the university are examined in anatomy, physiology, materia medica, and chemistry. Their final examination takes place in presence of the Censors of the State Medical Society, and, as an incentive to excellence, at that time, the Board of Regents, by whom the degrees are conferred, authorize the Faculty to select one of the theses, if one be deemed worthy, for publication, by the Superintendent of Public Instruction in his annual report, which, being a public document, is printed by legislative authority.

We are aware that former committees have objected to an increase in the number of medical schools, so situated as not to give to their pupils the benefits of hospital practice, the reasons for which, at first sight, appeared entirely satisfactory, and may do so now to a large majority of this Association. We believe, that this subject should be examined from two distinct points of view, one of which exhibits the character and qualifications of those who graduate at the best schools, and the other, the character and influence in society of that other multitude, who choose to exclude themselves from all these advantages, but come, nevertheless, by some illegitimate entrance, into the great professional amphitheatre. If there was a necessity for studying this subject only from the first point of view—of looking only to the effects produced upon the pupil by his attendance upon the college lectures and the hospital clinique, and if we could leave out of sight and out of mind the great number of practitioners with which the country would be filled—if the elementary schools of medicine were any less accessible than they now are—there would be no division in sentiment; no dissent from the opinions expressed by preceding committees, touching this matter. If there were in existence laws, which could be enforced, for restraining irregularities in practice, and if none dared exercise the functions of the physician except he were regularly invested with the baton of office, there would be great propriety in limiting the number of schools, and at the same time increasing their demand upon the mental energies of the pupil, as conditions precedent to receiving a degree.

In the absence of such laws, and for the want of any power to bear upon the profession, other than the power of opinion, the wisest course, in the judgment of your Committee, would be, to extend the hand of fellowship to all such schools as, having a competent faculty, shall give a course of lectures on general, comparative, and descriptive

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anatomy, physiology, and its application to pathology; chemistry, with its relations to pharmacy and toxicology; materia medica and therapeutics, the principles of surgery, obstetrics, including the doctrine of ovology; and the principles and practice of medicine.

If such schools could be restricted in conferring degrees to the grade of Bachelor of Medicine, a practical and valuable distinction would be made between those which teach the elements of medical science, and that other class, having their seat in the great commercial cities, and whose propinquity to hospitals enable them, if their advantages are rightly improved, to combine the inculcation of principles with their practical application and elucidation.

By this remark, the Committee are led directly to the subject of clinical instruction, on which they propose to speak more freely than has been customary in this Association. We admit its great importance; but we question its utility to the majority of students who annually throng the lecture-rooms of our metropolitan schools; to which they are led, not for the sake of seeing practice in the hospitals, but by the attractive talent of the professors, and the intellectual charm of the lecture-room.

It is not our design to treat with disrespect any remark which may have been made heretofore on this subject, yet our sense of accountability to the great body of our constituency, compels us, in some measure, to dissent from former opinions, or, at least, to state the case in such a way, as shall lead to opposite conclusions, if not to reforms in practice.

To avoid misapprehension, we will reassert the expression of our faith in the value of bedside experience to the medical neophyte, and propose only in the remarks which follow, to show that this cannot be attained by young men passing in groups, and in all stages of preparation, through the wards of a hospital, at the heels of a professor. To suppose an example. An elegant and practical lecture has just been delivered on the subject of pleuro-pneumonia. The class is dismissed from the lecture-room and reassemble at the hospital. The surgeon leads them from ward to ward, in which they find varicose ulcers, chancres, buboes, fistulas in ano, and such like affections, incident to men worn out by vice, and who are brought together by disease, vice, and want. Of course, the students see no particular relation between the theme of the professor and the cases in the hospital.

But having purchased the tickets of the professors, and paid for the privilege of visiting the hospital, in conformity to the requirements of the faculty, and in compliance with what he supposes to be

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the prevalent opinion of the profession, the student makes another visit, and by dint of effort, he gets within sight of the patient, and in hearing of the gentleman, who devotes a minute's attention to a case which may be one of obstructive disease of the mitral and tricuspid valves. The young man's preparation for this interview may have been a lecture, from his anatomical professor, on the osseous tissue, in which he had been shown its mode of development and chemical composition, without ever having dreamed that the earthy material of bone, or the cartilaginous nidus, in which it is naturally deposited, could ever, by abnormal action or arrangement, be the occasion of the dyspnœa and the serous deposits, which he sees in the case before him. In the next bed, perhaps, is an idiopathic dropsy. He perceives that decumbitus is painful in both—that respiration is embarrassed in each, and that the cellular tissue in the two are alike filled with water. He hears just enough from the teacher to confound his own perceptions of the analogies in the two cases, gets a wrong impression of the value of clinical instruction, and soon discontinues his visits to the hospital. This is not a fancy sketch, but a scene of yearly occurrence.

If this sketch is not a fiction, it seems to the Committee, to teach the necessity of placing restrictions upon admission to the wards of a hospital, and an equal necessity of filling the minds of young men with a knowledge of the structure, functions, and chemistry of man's organization, and all that relates to his normal condition as a psychic and physical existence, before they be allowed to take note of the abnormal deviations to which man is liable, or to think of prescribing a remedy for those deviations, when they have occurred.

An eminent professional writer, of the early part of the present century, in his letters on the education of a surgeon, makes some remarks so *germain* to the object we have in view, that of showing the futility of teaching clinical medicine to young men who have no knowledge of anatomy, that the Committee are tempted to incorporate them into this part of their report.

"If it were my wish to inspire a young man with professional enthusiasm, I should expect to accomplish it in no way so happily as by teaching him to study, night and day, the structure of the human body; to witness no form of disease, without comparing the symptoms with the natural functions, the probable changes which caused disorder, with the natural and healthy condition; to dissect and to meditate. The man thus prepared for practice, and pursuing through life those inquiries with diligence, would show himself in every pro-

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fessional act, a feeling and a thinking man; for reasoning on the probable changes going on within the body, has a powerful effect on the manners and feelings of a physician; and he will be most involved in serious thoughts and compassionate feelings, who connects every symptom with some probable change, and that change with the sufferings and probable fate of his friend. He examines with unaffected anxiety the seat of internal disease, and is agitated by every alteration of countenance, and by every sensible change. He takes a double interest in the fate of his patient; because, along with a malady painfully affecting a fellow-creature and a friend, he is occupied in watching certain phenomena in the animal machine, certain progressive disorders in its particular organs, which, from a long study of their structure, he foresees are likely to happen, and the signs of those changes are the presages of life or death." (*John Bell.*)

This is what, in the language of the botanist, may emphatically be called the "natural method" of showing that medicine may be exalted to the dignity of a science, though often reduced to the level of a trade. It is the method the Committee ask the Association to indorse. A thorough knowledge of the principles and elements of the science of medicine should precede any attempt at their practical application.

On the subject of clinical instruction, the Committee, from its intrinsic importance, have been induced to bestow a good deal of attention. Its Chairman, having for twenty years of his professional life been in charge of a hospital, either in the army or in civil life, feels authorized to ask the Association to revise its opinions heretofore expressed. He knows, and feels what everybody else should know, how improper it would be to allow a class of medical students, one after another, to auscultate a patient in the advanced stage of consumption, and how little they would learn from a clinical lecture without it. One object to be accomplished in bringing together the pupil and the patient is to train the perceptive faculties of the student; to educate, in fact, his sense of touch, his eye, and his ear. How much progress is made in this process, except by the resident pupil of a hospital, is very well known by students and teachers.

Because the end has not yet been attained, we would by no means relinquish the pursuit. We would reconstruct our hospital organizations, and adapt them to the wants of the time. This could be done by erecting them into schools of practice, with a special faculty,

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whose plan of instruction should have a direct relation to the cases in their wards, so that each one should become an illustration of the text of the professor. Schools thus constituted, and authorized to confer some distinctive honour upon their graduates, would fill a hiatus in our system of medical teaching. A supplementary school of practice, conducted on the plan proposed, would obviate the objections now made to clinical instruction, and do away very much with the sentiment or feeling of jealousy entertained towards the medical schools in the country, which not only do great good, but prevent infinite evil. How they do this, is best understood by those members of the Association who come from sections of the country sparsely populated. They are best aware how many young men, without their instrumentality, would pass from the office of the private preceptor without the advantages these schools can give them, wanting in that professional *esprit de corps*, which the associations of the anatomical theatre and the lecture-room, do so much to foster the growth of in the breast of every graduate who recognizes the influence of a medical alma mater. This Committee, then, would not discourage the organization of medical schools in different sections of our country, but would foster them as places fitted and designed to teach the elements of the science of medicine, and trust to the influence and example of the private instructor, who is emphatically the true clinical teacher, and to the hospitals, as schools of practice, to teach the art of applying the principles, which the faculties of these schools have imparted. In this way, the number of uneducated young men, now permitted, by the absence of salutary laws in all the States of the Union, with only one exception, to exercise the functions of physicians, will annually diminish, and be replaced by those having a higher sense of professional honour, because a wider range of professional attainments.

Having achieved this, and so far added to the respectability of our profession, the Committee would urge the Association to take one more step, scarcely less important as a means of augmenting its usefulness, on the supposition that its usefulness depends essentially upon its respectability, and to insist upon a more thorough and extended course of preparatory study before commencing that of medicine. In the existing state of public sentiment, we would give to preparatory scarcely less consequence than to professional education. The first, being more appreciable by the public, helps the physician to take that social position which furnishes to his colleagues the strongest

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guarantee that he will never sink to the level of the quack or the mountebank. When, as at the present time, by the establishment of schools of science and departments of agriculture in our colleges, the artisan is made familiar with the elementary forms of the matter to which his skill is to be applied, and the tiller of the soil is made acquainted with the geological structure of his farm, of the principles in chemistry by which he applies the elements of fertility to his fields, of the organization and habits of the plants best adapted to the soil, and of their relations to the meteorology of the climate in which he lives, there is an evident necessity for his medical friend having some general knowledge of these subjects, on account of their hygienic relations to his patrons, among whom may be individuals of the classes just described.

The necessity of a thorough preparation for professional study is not an idea of recent origin. The father of Medicine held, that to be an eminent physician, it was necessary not only to be well acquainted with the structure of the human frame, but also to be skilled in logic, astronomy, and other sciences; and of him it may be truly said, that he cultivated the art of medicine upon the strict principles of the inductive philosophy, more than two thousand years before the world gave Lord Bacon the credit of introducing this method of philosophizing. His devoted admirer, Galen, was skilled in all the sciences of the day; in logic, mathematics, rhetoric, and philosophy.

Aside from the influence of authority, or the obvious power it gives him of making an impression on the popular mind, there are other considerations which should induce the student to devote his early years to the study of the classics; to researches in metaphysics, mathematics, and the natural sciences. The light that they shed upon his professional path, shows that each of these departments of study has the quality of a lens, which concentrates its rays upon and gives brilliancy and distinctness to every medical subject on which they are brought to bear.

In our utilitarian age, the study of the classics is discouraged, because they do not obviously tend to increase the amount of "daily bread" in any given community. "That they conduce to mental vigor, and prepare the mind for professional pursuits, and lead to those refinements of thought and delicacies in sentiment and action so essential to the physician, is it not a matter of sufficient importance to counterbalance the loss of time expended in their pursuit? Polished language, delicate manners, and fastidious taste, and soli-

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tary studies, an indifference to common praise, or vulgar abuse, characterize the man devoted to learning. A classical education is found to exalt the generous affections, and, by examples of prudence, patience, and self-command, teaches at once a just contempt for the vexations of the world, and enthusiasm in behalf of humanity and of science, and purifies the mind from low pursuits."

Although the practical metaphysician has been styled the Jesuit of modern literature, and the study itself been treated with scorn, we would not have it omitted by the student of medicine.

It has been asked, what will a head filled with the Categories of Aristotle, the forty Summa Genera of Bishop Wilkins, or even the refined and exquisite metaphysics of Dr. Gregory, do, in curing the maladies of the human body? This becomes a pertinent question at a time when the professional mind is directed to psychological studies in connection with insanity, and especially as the eminent men whose labours have shed lustre upon themselves and blessings upon humanity, find reason for arranging themselves into distinct schools, taking the name of Psychics, Somatics, or Psycho-somatics, according as they are led by their researches into the belief that insanity is a mental, physical, or physico-mental malady. They who look into the "Law of Lunacy," or examine the cases of those dispossessed by disease of the natural agency of the mind, who commit criminal acts under the direction of a morbid or delusive idea, will cheerfully admit that even metaphysics has its practical uses. Aside from its direct application in professional life, it has been said by Bishop Barclay, that it is of the number of those preparatory studies, which may be compared with crops, raised not for the sake of the harvest, but to be ploughed in as a dressing to the land.

Botany, zoology, and meteorology are so obviously the allies of the physician, that no argument seems necessary to establish that relation.

Among the many interesting subjects of study which should arrest the attention and challenge the investigation of the medical student, there are few which can justly claim so large a share of his time as geology, and particularly that branch of it which treats of the structure and relations of mountain chains. Next to the division of the surface of the earth into land and water, the direction, height, and form of the elevated ridges which traverse the land, play the most important part in its physical history. Whilst the student of philology, ethnography, medicine, and physical geography, alike see

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the immense influence which the direction and position of mountain chains have exercised in the distribution of languages, in the migration of races of men, in giving form and outline to continents, in the modification of climate and origination of disease; the geologist acknowledges in them the grandest and most imperishable record of the great cycle of events, the study of which is his peculiar province.

As the medical student wanders from the strictly professional path of study into the domain of language, of metaphysics, of physical sciences, or the philosophy of nature, he will find that "the plan of the universe has proceeded from one All-comprehensive mind; and, as his circle of knowledge enlarges, that the unity of truth, and the consequent harmonious connection of knowledge, cause the boundaries of different sciences, and the possible subdivision of the general domain of knowledge, so to shade into each other, that it is sometimes difficult to determine them with precision."

When the young physician has attained eminence in social position, by moral character and reputation for intelligence, out of his profession, if he has not received the most practical kind of medical instruction, we have reason to believe that the instinct of self-respect will prompt him to an active cultivation of his profession, as a means of achieving equality with those who may be moving in the same sphere, lest he be thrown out of his orbit. Such an individual, by strengthening his feeling of self-reliance, will ride over all obstacles which lie in his way to professional distinction, and take a manly stand against all the forms of error, no matter in what guise they make their appearance.

For the considerations already set forth, as well as for the additional reason that such studies furnish a dignified and rational relaxation to the physician in his moments of leisure, which would otherwise run to waste, we would urge an early and faithful devotion to them. They will prove also a happy and pleasing resource in the wane of life, when necessity no longer impels him to labour, and his great and active duties to society draw near to a close.

The Committee beg leave to conclude their report by presenting the following resolutions:—

1. *Resolved*, That the Association reaffirm its formerly expressed opinions on the value and importance of general education to the student and practitioner of medicine, and that it would gladly enlarge its rule on this subject, so as to include the Humanities of the schools, and the natural sciences.

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2. *Resolved*, That in the opinion of this Association, a familiar knowledge of the elements of medical science, should precede clinical instruction.

3. *Resolved*, That in order to accomplish the latter, the hospitals, when they shall be elevated to the rank of schools of practice, and the intelligent private preceptor, are the most effectual instrumentalities to be employed.

Z. PITCHER, *Chairman.*

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REPORT

OF THE

COMMITTEE ON MEDICAL LITERATURE.

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REPORT OF THE COMMITTEE ON MEDICAL LITERATURE.

During the month of November last, the writer of this Report received a letter from Dr. R. La Roche, Chairman of the Committee on Medical Literature, stating that his health was too precarious to enable him to discharge the duties imposed upon him by the Association, and requesting me, as a member of the same Committee, to assume the duties of its Chairman.

In complying with this request, the general subject assigned to your Committee will be considered under three heads, viz.: The medical periodicals of our country; the medical publications, including monographs and books; and the best means of elevating the character and extending the usefulness of our national medical literature.

Periodicals. The general character of our medical journals has been so fully and ably discussed in the four Annual Reports on Medical Literature heretofore presented to the Association, and published in its *Transactions*, that further comments on the same subject seem scarcely desirable or necessary.

There are now published in the United States twenty-eight medical periodicals; of which, four are issued quarterly, six bi-monthly, fifteen, monthly, two, semi-monthly, and one weekly.

One of the monthlies, the *Union Médicale de la Louisiane*, is printed in French at New Orleans. All the rest are printed in the English language. The general arrangement of all these journals is similar; embracing first, a department for original articles, reported cases, &c.; second, one for reviews and bibliographical notices; third, one for selected matter; and fourth, one for editorial and miscellaneous items.

The periodicals and publications examined in this report, were issued from the press during the year embraced between the first

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day of April, 1852, and the thirty-first day of March, 1853. The aggregate number of pages embraced in the twenty-eight medical periodicals published during the year, is about 15,872. Of which 7,100 pages are devoted to original matter; 2,387 to reviews and notices of books; 6,385 to selected and editorial matter. The selections are chiefly made up of extracts from foreign and domestic journals, and are intended to present a summary of the more important improvements in the various departments of medical science. They are generally judiciously chosen, and present to the reader matter of practical interest.

The editorials, with very few exceptions, are written in a spirit of candour and professional liberality worthy of commendation.

From the brief summary given, it will be seen that a very respectable number of our medical periodicals are filled with original matter; embracing essays, statistical tables, and reported cases, relating to almost every branch of medical science and literature. The whole number of articles embraced in this department is about 1,200.* Of these, 284 relate to the department of Surgery; 391 to that of Practical Medicine; 64 to Obstetrics; 50 to Materia Medica; 120 to Physiology and Pathology; and the remainder to miscellaneous topics, chiefly of a hygienic and sanitary character. A very large proportion of all these articles are short, and consist of cases occurring under the observation of the narrator, either in hospital or private practice, and reported for the purpose of illustrating some feature in the history or progress of the disease, or some supposed effect of the therapeutic agents employed.

Many of them are of no value, for want of fulness of observation and completeness of detail. Others present isolated facts of interest. And a comparatively small proportion are related with such minuteness and accuracy, and in such numbers, as to afford deductions of great practical and scientific value.

To this latter class belong the articles embodying Cases of Maculated Typhus, by J. B. Upham, M. D., of Boston, and published in the *New York Journal of Medicine*, for May and July, 1852.

Cases in Surgery, by Willard Parker, M. D., of New York, in the November number of the same journal.

Contributions to the Practice of Midwifery, by Samuel S. Purple, M. D., of New York, in the same journal.

Pleuritic Effusions, and the Necessity of Paracentesis for their

* These figures are not critically exact, from a few omissions in our file of journals.

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Removal, by H. G. Bowditch, M. D., of Boston; and Surgical Cases, by John Frederick May, M. D., of Washington, in the *American Journal of Medical Sciences*, for April, 1852.

Cases of Urinary Calculus, by Paul F. Eve, M. D., of Georgia, in the same journal, for July, 1852.

On the Use of Bebeerine and Cinchonia in the Treatment of Intermittent Fever, by W. Pepper, M. D., of Philadelphia, in the same journal, for January, 1853.

Cases of Inflammation and Ulceration of the Cervix Uteri, by James M. Green, M. D., of Macon, Georgia, in the *Charleston Medical Journal and Review*, for January, 1853.

Observations on Excision of the Inferior Maxillary Bone, by S. D. Gross, M. D., of Louisville, Kentucky, in the *Western Journal of Medicine and Surgery*, for October, 1852.

Cases treated with Veratrum Viride, by D. C. O'Keefe, of Georgia, in the *Southern Medical and Surgical Journal*, for April, 1852.

Cases of Strangulated and Reducible Hernia, by Alden March, M. D., of Albany, New York, in the *Western Lancet*, for May, 1852.

Cases of Fracture of the Skull, with Loss of Brain, by Ariel Hunton, M. D., of New Jersey, in the *New Jersey Medical Reporter*, for June, 1852.

Two Cases of Painful Tubercle, by Frank H. Hamilton, M. D., of Buffalo, New York, in the *Buffalo Medical Journal*, for August, 1852.

On the Exciting Cause of Asiatic Cholera in Buffalo, July, 1852, by F. H. Hamilton, M. D., in the same journal, for September, 1852.

Closely allied to the foregoing class of articles, is another, embodying the statistical results of important diseases and surgical operations. Of these, the following are particularly worthy of mention, namely:—

Contributions to Aural Surgery; an Analysis of 120 Cases of Disease of the Ear, by E. H. Clark, M. D., of Boston, in the *American Journal of the Medical Sciences*, for July, 1852.

Statistics of Fractures and Dislocations treated in the Pennsylvania Hospital, during the ten years from 1840 to 1849 inclusive, by George W. Norris, M. D., of Philadelphia, in the same journal, for October, 1852.

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Statistics of the Operation of Amputation at the Hip-Joint, by Stephen Smith, M. D., of New York, in the *New York Journal of Medicine*, for September, 1852.

Cases in Surgery: Amputation at the Knee-Joint, by W. Parker, M. D., of New York, in the same journal, for November, 1852.

Statistics of Amputations at the Shoulder and Elbow-Joints, by Stephen Smith, M. D., of New York, in the same journal, for January, 1853.

History, Etiology, and Pathology of Fevers, in the *Transylvania Medical Journal*, for October, 1852.

Vital Statistics of St. Louis, Missouri, for 1851, by William M. McPheeters, M. D., in the *St. Louis Medical and Surgical Journal*, for July, 1852.

Fracture Tables, being a Supplement to those published by F. H. Hamilton, in the *Buffalo Medical Journal*, for April, 1849, by G. Boardman, M. D., of Buffalo. *Buffalo Medical Journal*, for September, 1852.

Clinical Report on Chronic Pleurisy, by Austin Flint, M. D., of Buffalo. *Buffalo Medical Journal*, for November and December, 1852, and January, 1853.

Remarks on Medical Statistics, by D. L. Starr, M. D., of Cincinnati. *Western Lancet*, for July, 1852.

A Comparison of the Weekly Bills of Mortality of New Orleans and Boston, for 1851, by G. C. Simonds, M. D., of New Orleans. *Charleston Medical Journal and Review*, for May, 1852.

On the Topography, Climate, and Diseases of Middle Georgia, by E. M. Pendleton, M. D., of Sparta, Georgia. *Charleston Medical Journal*, for July, 1852.

Sequel to Meteorological and other Observations in reference to the Causes of Yellow Fever in Charleston, by William Hume, M. D. *Charleston Journal*, for January, 1853.

Mortality of Philadelphia, arranged from the Record kept at the Health Office, by Wilson Jewell, M. D. *Medical Examiner*, for June, August, and November, of 1852, and February, 1853.

The two foregoing classes of articles relate almost exclusively to simple observed facts; and they are valuable in proportion to their number, and the fulness and accuracy with which they are stated.

A third class of articles, which in a literary aspect are more important than either of the preceding, consist of essays on special sub-

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jects, and original experimental inquiries. Among these, may be enumerated:—

Contributions to Experimental Physiology, by Bennet Dowler, M. D., of New Orleans, in the *New Orleans Medical and Surgical Journal*, for July, 1852.

Selected Items of Microscopic Observation, with lithograph plates, by G. L. Riddell, M. D., of New Orleans, in the same journal, for September, 1852.

Experiment with the New Orleans Battle-ground Crocodile, by Samuel A. Cartwright, M. D., of New Orleans, and other papers in relation to Mrs. Willard's Theory of the Circulation of the Blood, in the *Boston Medical and Surgical Journal*, for August 25, 1852, and subsequent numbers.

On Respiration, by John W. Draper, M. D., of New York, in the *American Journal of Medical Sciences*, for April, 1852.

The Practical Application of the Microscope to the Diagnosis of Cancer, by Francis Donaldson, M. D., of Baltimore, Maryland, with plates, in the same journal, for January, 1853.

On Pulsating Tumours of Bone, with Illustrations, by Charles D. Smith, M. D., of New York, in the *New York Journal of Medicine* for March, 1853.

Pathology, its Relations to Physiology as based upon Abnormal Nutrition, by W. G. Burnett, M. D., of Boston, in the *Southern Medical and Surgical Journal* for May, 1852.

Observations on Excision of the Inferior Maxillary Bone, with Cases, by S. D. Gross, of Louisville, Kentucky, in the *Western Journal of Medicine and Surgery* for October, 1852.

An Inquiry into the Nature of the Disease called Milk-sickness, by L. P. Yandell, M. D., in the *Western Journal of Medicine, &c.*, for May, 1852.

A Treatise on the Causes, Constitutional Effects, and Treatment of Uterine Displacements, by William Edward Coale, M. D., of Boston, in the *Boston Medical and Surgical Journal.*

Respiration subservient to Nutrition, by E. Leigh, M. D., of Townsend, Massachusetts, in the same journal.

Essentials of Inflammations, by F. Carter, M. D., of Columbus, Ohio, in the *Ohio Medical and Surgical Journal* for November and January, 1852–3.

On some of the Connections of the Medical and Physical Sciences, by John Locke, M. D., of Cincinnati, in the same journal for September, 1852.

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On Vital Organic Chemistry, by C. Caldwell, M. D., of Louisville, Kentucky, in the *Western Journal of Medicine and Surgery* for August, 1852.

On Provisional Callus in Fractured Bones, by Frank H. Hamilton, M. D., of Buffalo, N. Y., in the *Buffalo Medical Journal* for February, 1853.

Remarks on the Disorders of the Cerebral Circulation, with special reference to the Opinions of Dr. Burrows, by V. Kersey, M. D., of Georgia, in the *Western Lancet* for February, 1853.

Lectures on the Pathology, Diagnosis, and Treatment of Diseases of the Chest, by L. M. Lawson, M. D., of Cincinnati, Ohio, in several numbers of the same journal.

The Anatomy and Physiology of the Ganglionic Nerves, by Carthon Archer, M. D., of Virginia, in the *Stethoscope* for July, 1852.

The Sulphate of Quinia; its Therapeutic Action, by R. L. Madison, M. D., of Petersburg, Va., in the *Stethoscope* for September, 1852.

Observations on the Functions of the Gall-bladder: Is it not a Secreting Viscus? by John Dove, M. D., of Virginia, in the *Stethoscope* for December, 1852.

Remarks on the Uses and Effects of Sulphate of Quinia, by James M. Carr, M. D., of Richmond, Virginia, in the same number of the same journal.

Atomic Theory of Life, by Thomas Spencer, M. D., in the *Philadelphia Medical and Surgical Journal* for December, 1852.

Remarks on Typhoid Fever, by S. B. Graham, M. D., of Barnwell, South Carolina, in the *Charleston Medical Journal and Review* for May, 1852.

Remarks on the supposed Connection, Pathological and Etiological, between Pneumonia and Periodic Fever, by R. La Roche, M. D., of Philadelphia, in the *Charleston Medical Journal* for September, 1852, and January, 1853.

Remarks on Epithelial Tumours and Cancer of the Skin, with Illustrations, by J. Da Costa, of Philadelphia, in the *Medical Examiner* for April and May, 1852.

Philosophy of the Negro Constitution, by S. A. Cartwright, M. D., of New Orleans, in the *New Orleans Medical and Surgical Journal* for September, 1852.

On the Use of Quinia in Continued Fever, by William M. Boling,

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M. D., of Montgomery, Alabama, in the *New Orleans Medical Journal* for July, 1852.

The Motive Power of the Blood, by Albert Welles Ely, M. D., of New Orleans, in the same journal.

The Abortive Treatment of Fevers by the Sulphate of Quinia, by E. D. Fenner, M. D., of New Orleans, in the same journal, for November, 1852.

Is Typhoid or Epidemic Pneumonia identical with Periodic Fevers? by S. L. Grier, M. D., of Mississippi, in the same journal, for January, 1853.

On the Climate and Diseases of California, by James Blake, M. D., in the *American Journal of Medical Science* for July, 1852.

A Case of Saccharine Diabetes, with Tabular Observations of its Pathology, by Charles Frick, M. D., of Baltimore, in the same number of the same journal.

The Microscopical and Histological Relations of Enchondroma, by J. W. Burnett, M. D., of Boston, in the *American Journal of Medical Science* for April, 1852.

There are perhaps a few other papers that might, with propriety, be included in this enumeration, but the list is already sufficiently extensive for our present purpose.

Regarding the duties of the Committee on Medical Literature as limited to a review of the *general character* of our medical periodical publications, as indicated by the more important articles contained therein, we have refrained from any comments on the *matter* presented in the several articles deemed worthy of mention. Another reason for this omission is found in the fact that such an analysis of the several important articles as would adequately represent the views and facts embraced in them, would extend our report to an undue length, and make it a *resumé* of what has been said on the various branches of medical science and practice, rather than a simple report on the literary character of our medical publications.

From the brief summary which we have presented, it is apparent that in *quantity* the department of our Medical Journals devoted to original matter and reported cases, is ample; and that during the past year, at least, it has contained numerous articles, exhibiting not only the results of patient and successful scientific research, but also evidence of high literary attainments. This remark is true, not merely in reference to a limited number of articles relating to practical medicine and surgery, like those of Dr. Upham on Typhus Fever, Dr. Flint on Chronic Pleurisy, &c. &c., but also in reference

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to several relating to the more abstruse and purely scientific subjects of inquiry, requiring alike the skilful use of the dissecting-knife, the microscope, and the analytical processes of the chemist. For examples of this kind we need only refer to the papers of Drs. Draper, Donaldson, Dowler, Frick, Burnett, Riddell, Da Costa, and others, the titles of which have already been given.

Judging from the comments made by the several committees on medical literature, heretofore appointed by this Association, we are constrained to believe that a decided improvement has taken place in this department of medical journalism during the last two years. Certain it is that the use of the microscope, and its application to physiological and pathological researches, have become familiar to a much larger number of the members of our profession; and an increasing disposition to enter upon those inquiries requiring original experiments, and a thorough knowledge of organic chemistry, is plainly visible to the careful observer. That much of this is due, either directly or indirectly, to the influence of this Association, and its auxiliary Societies in the several States, is undoubtedly true.

But while we have derived much pleasure from the examination of some portions of our periodical literature, we are constrained to acknowledge that other parts of it are extremely faulty, if not entirely worthless. Thus, we find some articles embodying allusions to cases of disease or injuries, so meagre and incomplete in detail that neither the writer nor any one else can deduce a conclusion from them, or, in some instances, be certain of the nature of the disease intended to be described. These seem to answer no other purpose than to fill a certain space in the *Journal*, and afford the author the pleasure of seeing his name in print. Another class of articles, equally defective and far more mischievous, are made up of what is intended for a report of individual experience in the treatment of some particular disease, and hence are generally regarded as eminently *practical* in their character. They most frequently commence by stating the residence of the writer, and the fact that within a certain length of time a particular disease, or class of diseases, has prevailed to such an extent that "a specified number of cases has come under his own observation." Perhaps a brief description of some of the leading symptoms is given, couched in the most general terms; and sometimes a vague opinion is expressed in regard to the causes and pathology of the disease. An outline of the treatment adopted, with its effects, stated in terms equally general and indefinite, completes the article. The object of such writers seems to be simply to

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set forth the efficacy of some particular remedy in the treatment of a particular disease. Hence, they omit altogether a clear statement of the topography of their locality; the particular season when the disease was most prevalent; the character and habits of the class most affected; and such an accurate and detailed description of the disease, with its effects on the various organs and functions of the system, as will enable the reader either to judge correctly of the true relations of the remedies employed to the course and terminations of the disease, or to compare it with the phenomena of the disease called by the same name in his own locality.

Thus, we have in most of this class of articles but two simple items of information, namely, that the writer had observed a given number of cases of disease which he had designated by a particular name, such as typhoid fever, continued fever, pneumonia, &c., and that he had made use of a certain remedy or remedies in their treatment, with a specified result. If it were true that the *name* of a disease always indicated a well-defined and invariable morbid condition, then the two items of information just named, and the articles affording them, would be extremely valuable. But so long as the names of diseases indicate only their general character and tendencies, while their particular or *specific* character varies with every variation of climate, locality, season, and class or condition of people, so long will such articles increase the confusion and contradictions which now mar our medical literature rather than contribute to the advancement of our profession. And while our brethren content themselves with such incomplete observations and descriptions of disease, we shall continue to hear one extolling the curative powers of large doses of quinia in continued fever, while another will regard it with indifference, and a third decidedly condemn it as injurious, each claiming that his opinion is founded on abundant experience or clinical observation.

Indeed, while examining that portion of our literature now under discussion, we have been most forcibly reminded of an *agricultural* discussion to which we once listened in the capitol of the Empire State. The particular subject under consideration was the application of lime as a manure. Mr. A. stated that he had used lime for a few years past, putting a certain number of bushels on an acre, cultivating the soil in a particular manner, and with immense benefit to his crops. Mr. B. next detailed his experience, differing somewhat from that of A., but still with results decidedly favourable to the use of lime as a manure. Mr. C. now rose, evidently in rather

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a querulous mood, and, after catechizing briefly those who had preceded him, proceeded to state with great accuracy his experience. He had purchased several tons of lime, applied it in the most approved manner, and on a pretty extensive scale; but, much to his chagrin, had found it positively injurious to his crops. In the midst of the confusion resulting from these contradictory statements, a learned professor of natural science arose, and by questioning Mr. C. soon ascertained that the soil of his farm had been analyzed, and known to contain naturally a full supply, if not an excess of lime in its composition; while that of Mr. A., which was located in a distant part of the State, was deficient in the same ingredient; thus explaining perfectly what had before appeared so irreconcilable and contradictory.

We have no doubt that much that now appears so conflicting in the published experience of different physicians, would be fully reconciled by a more complete description of the modifying causes, and a full recognition of the special character of the morbid phenomena in each case. An accurate knowledge of these is just as essential to enable the physician to make a rational choice of remedies, as is a knowledge of the composition of the soil to enable the agriculturist to choose the appropriate manure. And it is the absence of this knowledge, arising from the extremely defective mode of observing and recording both the immediate phenomena of disease and the modifying agencies that accompany it, which constitutes one of the most important deficiencies in the current literature of our profession.

During the past year, perhaps the most prominent topic of discussion, among the contributors to our medical periodicals, has been the relations of the different types of fever to each other, and the utility of quinia in the treatment of that variety called typhoid. And nowhere is the deficiency just alluded to more apparent, or the occasion of less satisfactory results, than in this discussion.

The next department of our Medical Periodicals requiring attention, is that devoted to reviews and notices of books. This, in many of the Journals, is meagre and unsatisfactory in the extreme, consisting of little more than a copy of the title-page of the book purporting to have been examined, with a single paragraph embodying a very general expression of opinion, and that almost always of a commendatory character. To this remark, the reader will find some gratifying exceptions, more especially in the *American Journal of*

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Medical Sciences, the *Western Journal of Medicine and Surgery*, the *Charleston Medical Journal and Review*, and the *New York Journal of Medicine and Collateral Sciences*. But, as a whole, it must be acknowledged that the review department of our medical periodicals is far more defective than any other. We may look in vain over the Journals of a whole year for one of those comprehensive critical analyses which will present to the reader a *just* and full conception of the arrangement, style, and sentiments of a work examined, with such comments as clearly show its relation to the existing state of our knowledge of the subjects on which it treats. This is much to be regretted, because few things contribute more to the elevation of the literature of any country or profession than the efficient discharge of the duties of the critic, in a spirit of justice, liberality, and candour. To perform these duties, however, in such a manner, requires a more thorough mental discipline, and a more ready and extensive knowledge of medical literature generally, than to discharge any one, or even all, of the other duties of the journalist. The neglect of the review department of our medical periodicals is doubtless attributable, in a great degree, to the fact that the editors cannot find time personally to review all the important works sent them; and the proceeds of their respective Journals will not enable them to offer a sufficient compensation to induce others to do it for them.

It has already been remarked that the selections and extracts contained in our medical journals are generally judiciously chosen, and present matter of interest and value. That the more important of these selections should appear in many different Journals is neither surprising nor objectionable, inasmuch as the number of readers who peruse more than one or two medical periodicals is very small. That the whole number of Journals published in this country is far greater, in proportion to the population, than in any other country on the globe, there can be no doubt. Whether this prolific increase in the number of professional Journals is productive of more good than evil, is a question often discussed, and not unfrequently answered in the negative. On this point, we fully agree with the following sentiments of one who has himself been one of the most ardent and successful cultivators both of our periodical and general medical literature:—*

"One of the most obvious benefits," he says, "is an increase of

* See Second Discourse before the Cincinnati Medical Library Association, by Daniel Drake, M. D.

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the number of readers. Many physicians subscribe for a Journal published near them, who would not send to a great distance; others, through feelings of State pride; and others still, from its being conducted by the professors of their *alma mater*. And, although these motives are not of a high order, the acts to which they prompt are productive of improvement. Another benefit resulting from the multiplication of Journals, and their wide dissemination over the country, is that they call forth communications from neighbouring physicians, who would never be aroused or emboldened to write for distant and perhaps more distinguished periodicals; and thus our science receives many contributions of facts which otherwise would perish with those who observed them, while the writers themselves are by the very efforts stimulated to improvement. On the other hand, it is undeniable that the demand for new materials is so far beyond the supply that much finds its way to our periodical press which, by its want of originality or intrinsic importance, with a still greater lack of simplicity and purity of style, retards the improvement of our medical literature, and postpones the time when it will enjoy the confidence and receive the homage of men of profound science or accomplished scholarship. Still further, the want of extended and productive subscription lists renders it impossible for publishers to make such compensation for communications as would greatly contribute, not only to swell their number, but improve their quality."

Since the organization of the American Medical Association, State and District medical societies have so much increased in number and influence, that their published transactions may now be regarded as constituting an important part of our medical literature. A list of those societies, whose transactions have been published in a separate form, at least so far as we have been able to ascertain them, will be found in the Appendix to this report. Among the papers thus published, the following may be regarded as particularly worthy of mention, viz.:—

Dislocations of the Femur on the Dorsum of the Illium, reducible without Pulleys, or any other Mechanical Power, by William W. Reid, M. D., of Rochester, New York. *Transactions of the New York State Medical Society* for 1852.

On Deaf and Dumb Institutions, by Peter Van Buren, M. D., of Albany, New York. *Ibid.*

The Topographical Reports concerning several Counties in the State of Pennsylvania, three of which are accompanied by well-exe-

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cuted Geological Maps. *Transactions of the Medical Society of Pennsylvania*, 1852.

A Report on Variola and Vaccination, by a Committee, of which G. Emerson, M. D., was Chairman. *Ibid.*

Report of Twenty-five Cases of Urinary Calculus; in Twenty-three of which the Bilateral Operation was performed, by Paul F. Eve, M. D., of Augusta, Georgia. *Transactions of Third Annual Meeting of Medical Society of State of Georgia.*

Report of the Committee on Surgery, by Henry F. Campbell, M. D. *Ibid.*

Reports of the Committees on Medicine and Surgery, made to the Third Annual Meeting of the Indiana State Medical Society. *Transactions*, New Albany, 1852.

Reports of the Committee on Surgery, by C. A. Pope, M. D., of St. Louis; of the Committee on Obstetrics, by M. M. Pallen, M. D.; of the Committee on the Adulteration of Drugs and Medicines, by Thomas Reyburn, M. D. *Transactions of Medical Association of State of Missouri*, 1852.

Reports on Vital Statistics, by W. S. Chipley, M. D., of Lexington; and on Surgery, by S. D. Gross, M. D., of Louisville. *Transactions of the Kentucky State Medical Society*, 1852.

On the Treatment of Incomplete Anchylosis of the Knee-joint, with a new instrument for making Extension and Motion, by E. S. Cooper, M. D., of Peoria, Illinois. *Transactions of the Illinois State Medical Society* for 1852.

Report of the Committee on Practical Medicine, by N. S. Davis, M. D., Chairman, and Thomas Hall, M. D., one of the members of the Committee. *Transactions of Illinois State Medical Society*, for 1852.

Essay on Tetanus, by J. J. Abernethy, M. D., of Tennessee. *Transactions of the Medical Society of Tennessee*, published in the *Nashville Journal of Medicine and Surgery*, vol. iii. No. 1.

In addition to the published *Transactions* of the State societies, we find some very valuable papers emanating from societies of a more local character. These may be found in the *Quarterly Summary of the Transactions of the College of Physicians of Philadelphia;* in the Discussion on Anæsthesia in the Philadelphia County Medical Society, published in the *Medical Examiner* for April and May, 1852; in the Reports from the New York Pathological Society, the New York Academy of Medicine, and the New York Medical Asso-

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ciation, published in the *New York Journal of Medicine*, and the *New York Medical Times;* in the *Reports of the Boston Society for Medical Improvement*, published in the *American Journal of Medical Sciences;* in the papers read before the Cook County Medical Society, and published in the *North-Western Medical and Surgical Journal;* in the *Transactions of the Medical Society of Southern Central New York* for 1852; and the Reports made to the Medical Society of East Tennessee, in the *East Tennessee Record of Medicine and Surgery.*

Of the papers above alluded to, we would particularly call attention to that of Dr. Gross, of Louisville, which is not only written with ability, but contains a very full history of operative surgery in the State of Kentucky, from the first settlement of the country to the present time. It constitutes, therefore, an important chapter in the history of American Surgery, and will be found no less valuable for future reference than interesting for present instruction. It is very gratifying to observe the increasing attention which all our medical societies are bestowing on the subject of medical topography, as connected with the prevalence of endemic and epidemic diseases.

Some of the papers on this subject elicited during the past year are very valuable; and there can be little doubt but more extended investigations in the same direction will soon enable us to explain much that is now obscure or contradictory in reference to the nature and treatment of diseases, and make our knowledge of their remote causes much more definite and satisfactory. Most of the papers and reports contained in the proceedings and published transactions of our medical societies are written with more care than the ordinary articles that fill the original department of our periodicals; and yet many of them exhibit the same defects, alike in their conception, in their style of execution, and in their matter.

Few instrumentalities are capable of doing more to improve the literature of our profession than the medical organisations which have become so generally formed in almost all sections of the Union; and hence, they should be encouraged and sustained by every friend of science and humanity.

Monographs and Books.—The second department of our report must be occupied with a consideration of the number and character of the medical publications or books issued from the American press during the past year. These may be conveniently arranged in three divisions, namely: New American works; new editions of Ameri-

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can works; and republications of foreign works. As complete a list of each as we have been able to collect will be found in the Appendix; and hence, we shall only notice in this place such books belonging to the first two divisions as seem most important, especially in reference to their position and influence as a part of our national medical literature. Whoever refers to the Appendix, will see that the additions made during the past year to the list of valuable American medical works have been, at least, respectable; while the numerous new editions of important works indicate a gratifying degree of appreciation on the part of the profession, as well as of prosperity to native works of merit.

Among the more important new works that have appeared during the year embraced in this report, those relating to Practical Medicine are:—

A Treatise on Diseases of the Chest: being a Course of Lectures delivered in the New York Hospital, &c., by John A. Swett, M. D., one of the Physicians to the New York Hospital, &c. New York, 1852.

Clinical Reports on Continued Fever, based on an Analysis of one hundred and sixty-four cases, &c., by Austin Flint, M. D., Professor of the Practice of Medicine in the Medical Department of the Buffalo University. George H. Derby & Co.: Buffalo, 1852.

A Treatise on the Constitutional Effects of Uterine Displacements, and their Treatment, by William Edward Coale, M. D. Boston, 1852.

Those relating to Surgery are:—

A System of Operative Surgery, based on the Practice of Surgeons in the United States, and comprising a Bibliographical Index and Historical Record of many of their Operations for a period of Two Hundred Years, by H. H. Smith, M. D., Surgeon to St. Joseph's Hospital, &c. &c. Philadelphia, 1852.

Operative Surgery, illustrated; containing more than nineteen hundred engravings, &c. &c., by R. U. Piper, M. D., of Boston. Published by Ticknor, Reed, & Fields, 1852.

On the Surgical Treatment of Polypi of the Larynx and Œdema of the Glottis, by Horace Green, A. M., M. D. New York, G. P. Putnam, 1852: pp. 124, with plates.

Those relating to Materia Medica are:—

A Review of Materia Medica for the Use of Students, by John B. Biddle, M. D., &c. Philadelphia: Lindsay & Blakiston, 1852.

Materia Medica, Pharmacology, and Therapeutics, by William

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Tully, M. D., &c. &c. Issued in numbers, at Springfield, Mass., 1852 and 53.

Those relating to Physiology are:—

Essays on Life, Sleep, Pain, &c., by S. H. Dickson, M. D., Professor in the Medical College of South Carolina. Philadelphia: Blanchard & Lea, 1852.

Notes on Carpenter's Human Physiology, containing some Original Views of the Economy of Nature, &c., by Louis Mackall, M. D., of Washington, D. C.

In addition to the above, we have found many interesting essays or monographs on particular subjects, the titles to which are given in the Appendix. Also, Biographical Sketches, and Memoirs of several distinguished Members of our Profession, who have recently finished their course on earth, and left us their writings and their imperishable memories, to induce us to emulate their noble example. Among these, may be mentioned the very full and interesting sketch of the late Daniel Drake, of Cincinnati, by S. D. Gross, M. D., of Louisville; that of J. Kearney Rogers, by Edward Delafield, M. D., of New York; and that of James B. Rogers, by Joseph Carson, M. D., of Philadelphia.

The work of Dr. Swett, on *Diseases of the Chest*, is an octavo volume, of 585 pages, published by D. Appleton & Co., 200 Broadway, New York. It consists of lectures which were originally delivered in the New York Hospital, and published a few years since in the *New York Lancet*. In revising and republishing these lectures, the author has preserved their original didactic style, which is direct, pure, and accurate. The work is elementary, and thoroughly practical, embracing the history, diagnosis, and treatment of the various diseases of the respiratory passages, the lungs and the heart, with two preliminary chapters on the means of diagnosis.

The presentation of the matter in the form of lectures, gives it the appearance of being addressed to students or hearers, and in our estimation, detracts somewhat from the literary merits of the work. The author, very deservedly, ranks among the most eminent and experienced auscultators in this country; and though there are some views presented which, were it our province, we should feel constrained to criticize, yet his work must be regarded as a highly valuable addition to the permanent literature of our profession.

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The *Clinical Reports on Continued Fever*, by Dr. A. Flint, of Buffalo, is a well-executed volume of 390 pages, from the press of George H. Derby & Co., of Buffalo, New York. It is made up of three separate reports, embracing an Analysis of 164 Cases of Continued Fever; with Remarks on the Identity of Typhus and Typhoid Fever; Relapsing Fever; their Diagnosis, &c.; and a Memoir on the Transportation and Diffusion by Contagion, of Typhoid Fever. All but the last-mentioned part, were originally published in the *Buffalo Medical and Surgical Journal;* extra sheets of which were reserved, at each issue, to constitute the present volume. While this mode greatly diminishes the expense of publishing a first edition, it also served to detract materially from its literary merits; inasmuch, as each successive report constitutes a repetition of the arrangement, the ideas, and the general facts of the preceding, though drawn from a new collection of cases. Of this the author is fully conscious, as stated in the Preface. Notwithstanding this fault, however, the work is written in a chaste and accurate style, and embodies the results of a most extensive and laborious series of observations and analysis, which renders it, perhaps, the most original and truly valuable work issued from the American medical press during the past year. This value consists in the addition which it makes to our knowledge of the natural history and phenomena of Fevers of the Continued Type, rather than in the advancement of any new or important view of their pathology or treatment.

The *mode* of investigation adopted, especially in the chapter on the *Identity* of Typhus and Typhoid Fever, we regard as faulty, and well calculated to lead to erroneous conclusions. In this respect, however, he simply follows the example of Louis, Jenner, and many others, and, therefore, almost necessarily arrives, like them, at the conclusion that these two varieties of fever are essentially distinct from each other, or non-identical. As the erroneous mode of investigation here alluded to (if erroneous it is), has received the sanction of the highest authorities in medical literature, and is by no means restricted to the work of Dr. Flint, we shall consider the subject more at length in the concluding part of this report.

The work of Dr. Coale, of Boston, *On the Constitutional Effects of Uterine Displacements, and their Treatment*, first appeared in successive numbers in the *Boston Medical and Surgical Journal.* Though an unpretending volume, it is written in good style, and on a subject of great practical importance. That uterine displacements

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are far more frequently dependent on morbid conditions affecting the whole system, than is generally supposed, we have no doubt. And the author of this work has not only made a valuable addition to our medical literature, but he has afforded a plain, practical treatise on a subject hitherto much neglected.

The System of Operative Surgery, based upon the Practice of Surgeons in the United States, by Henry H. Smith, M. D., of Philadelphia, which was issued from the press in five parts, is now complete, in one most beautifully illustrated octavo volume of 698 pages. The style in which the numerous steel plates in this work are executed, as well as the general typographical appearance of the text, does great credit not only to the publishers, Messrs. Lippincott, Grambo, & Co., of Philadelphia, and the artists employed, but to the art of book-making in our country. Dr. Smith's work is, therefore, not merely a valuable addition, but also emphatically an ornament, to our medical literature. We regret, however, that the author has not always maintained a simplicity, elegance, and perspicuity of style in writing, corresponding with the beauty of typographical execution attained by the publishers. We believe Dr. Smith is entitled to the honour of having made the first attempt to produce a systematic treatise on operative surgery, based exclusively on the practice of American surgeons. The task is certainly one of great difficulty and delicacy; and it must be generally conceded that the author has displayed in its execution a degree of industry, research, and ability worthy of high commendation. It is true that, if we regard it as an intended embodiment of American Operative Surgery, it is incomplete, and open on many points to criticism. There are some topics passed over so lightly, as to convey an unfair idea of the state of that branch of the profession among us. All these, however, are defects capable of being supplied in future editions. Another item which we regret to find in the work, is that a large portion of the illustrations or plates are copied from Parisian authors. Inasmuch as the work purports to be based on the practice of American surgeons, we should have preferred to see the plates exclusively American also, even if we had been compelled to accept a smaller number. Though thus defective in some particulars, the volume before us is creditable alike to the author and the profession, and is deserving of a place in every medical library in our country.

The work of Dr. Horace Green, of New York, on the *Surgical Treatment of Polypi of the Larynx and Œdema of the Glottis*, is

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a well-executed Monograph of 124 pages, accompanied by plates, and issued by G. P. Putnam, of New York. It is a plain, practical treatise on two subjects, which have, until recently, attracted very little attention. Dr. Green is well known to have made the affections of the throat a specialty in practice, which has given him greater opportunities for observing the more rare forms of disease in that important region, and more experience in their treatment. These are circumstances which will give to the present little volume additional claims on the attention of the profession.

Dr. Biddle's *Review of Materia Medica for the Use of Students*, is a moderate-sized octavo volume, published in good style, by Lindsay and Blakiston, of Philadelphia. It is one of those brief compends or summaries of an important branch of medical science, of which we have too many for the good of the profession. We are satisfied that their value, as brief works of reference, is more than counterbalanced by the encouragement which they afford to the habit of superficial reading. Aside from this objection, the work is arranged and written in a style that is creditable to the author.

The work of Dr. Tully, on *Materia Medica, Pharmacology, and Therapeutics*, unlike the preceding, is evidently designed to be an extensive and thoroughly original work on all of the subjects embraced in its title. Its publication has been commenced in monthly numbers, giving evidence of a fair style of typographical execution, and of high literary attainments on the part of its able and experienced author. But we will not judge of the merits of the whole work by the small part that has yet appeared, waiting patiently with the hope that its completion will not only fully sustain the reputation of Dr. Tully, but also add another to the list of elaborate and valuable American medical works.

Dr. Dickson's *Essays on Life, Sleep, Pain, &c.*, are written in the well-known elegant style of that distinguished author and teacher, and published in the equally well-known style of Blanchard and Lea, of Philadelphia. The volume contains six essays, which treat of Life, Sleep, Pain, Intellection, Hygiene, and Death. The style of these essays is so pure, and free from technical words and phrases, and the subjects treated are of such a nature, that the whole work is almost as interesting and profitable to the *general* reader, as to the members of our profession.

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Among the important American works, of which new editions have been published during the past year, are, the well-known standard Treatise on the *Practice of Medicine*, by George B. Wood, M. D., Professor of Practice of Medicine in the Medical Department of the University of Pennsylvania, third edition; the *History, Diagnosis, and Treatment of the Fevers of the United States*, by Elisha Bartlett, M. D., Professor of Materia Medica, &c. in the College of Physicians and Surgeons, New Brunswick, third edition; and *Obstetrics; the Science and the Art*, by C. D. Meigs, M. D., and Professor of Obstetrics, &c. in Jefferson College, Pennsylvania, second edition. These have all been before the profession too long, and are too highly appreciated both at home and abroad, to require any special notice in this place.

Although the new edition of each has been carefully revised and enlarged by its distinguished author, yet in no respect has its literary merits and defects been materially altered.

In addition to the above, the works of Dr. John Neill, of Philadelphia, entitled *Outlines of the Nerves*, and *Outlines of the Arteries, with Short Descriptions, designed for the Use of Medical Students*, have passed to a second edition. Also the *Analytical Compendium of the Various Branches of Medical Science*, by Drs. John Neill and Francis G. Smith; and the *Practical Treatise on Dental Medicine*, by Dr. T. E. Bond, of Baltimore.

Several other works have passed to new editions, affording a gratifying evidence that there is no longer *ground* for the oft-repeated complaint of neglect shown to our own authors. But neither our time nor our sense of propriety will permit us to allude more in detail to these, or to mention individually the more important Translations and republications of foreign works. Accordingly, we pass to the consideration of the last, and we would hope most important part of the duties devolving upon the Committee, namely, the *means* that are best calculated to elevate the character and foster the growth of a medical literature truly national or American.

On the other side of the Atlantic, the charge has sometimes been made that Americans are mere copyists—that we have no medical literature of our own; and eminent members of the profession among us have occasionally acknowledged the truth of the charge. Our own reading, however, has led us to a different conclusion. Deriving our existence as a nation from Great Britain; existing, indeed, for a long period as a part of that empire, we necessarily inherited at our birth a portion of her science and her literature, which furnished

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the basis for our own. And for a long period, also, the circumstances and habits of our countrymen, and especially of the members of our profession, were not favourable to the cultivation of the more abstruse and experimental branches of science. Hence, for the advancement of experimental physiology, animal chemistry, and the minuter researches in morbid anatomy, we have been mainly indebted to Europe. But, notwithstanding this, we have had a medical literature of our own; not borrowed, not a mere imitation; but originally and truly American, from the days of Benjamin Rush to the present time. As we should expect in a thinly populated country, subject to the prevalence of acute diseases of an active grade, the chief attention of the profession was concentrated on matters of direct practical interest and application. A direct inquiry into the history, causes, and treatment of the more important diseases would be the first object of the practitioner.

The same circumstances that were unfavourable to the carrying on of abstruse and experimental investigations were equally adverse to the production of extensive treatises on any of the departments of science. Hence, as the attention of the profession was fixed chiefly on topics of immediate practical interest, it naturally found its expression in essays, monographs, and periodical journals, rather than in volumes requiring more leisure for their preparation and involving greater expense in their publication.

Thus, within a very few years after the establishment of our national independence, and at a period when medical journalism in Europe scarcely maintained an infantile existence,* that noble quarterly, the *New York Medical Repository*, was established in New York, and ably maintained by Drs. Mitchell, Miller, and Smith; and was speedily followed by two in Philadelphia, and others in the principal Atlantic cities. This highly important department of medical literature, once planted in the genial soil of America, has continued to increase with a rapidity unparalleled in any other country in Christendom. Neither has this been a mere parasitic growth, deriving its pabulum or means of subsistence chiefly from the press of Europe.

Whoever reads diligently the earlier Medical Journals of our country, the *Medical Inquiries and Observations* of Dr. Rush, and the numerous papers by Warren, Baily, Bard, Jones, Barton, Mitchell, Dewees, Chalmers, Moultrie, and their compeers and co-

* See "Second Discourse before Cincinnati Medical Library Association," by Daniel Drake, M. D.

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labourers, will find a literature, neither copied from a foreign source nor unduly subservient to the then prevailing dogmas of Boerhaave, or the cumbersome Nosology of Cullen.

It is true that they did not enter upon original investigations in anatomy, healthy and morbid, or experimental physiology, to compete with a Bell or a Bichat; and for the same reason that our countrymen, at the World's Fair, made no attempt to excel in the fine arts, but preferred to represent the true characteristics of their country in the excellence of their useful products and labour-saving machinery. And if our broad and richly-laden harvest-fields have hitherto attracted the attention of our countrymen more to the invention of reapers than the moulding of statuary, so have the modifications induced by our climate, our richly-varied geographical position, and above all the peculiarities of our endemic and epidemic diseases, fixed the attention of our profession, and given direction to its investigations.

The causes, modes of communication, phenomena, and treatment of our endemic and epidemic fevers have ever been with us the leading subjects of study. And much of the fruits of such study has found a record in our medical periodicals, in the transactions of medical societies, in monographs, and more latterly in standard and systematic treatises; constituting a literature marked as truly national by those characteristics which distinguish everything else American, namely, independence of thought and capability of practical application. A close study will show that these characteristics have led to the adoption of new modes of practice, both in medicine and surgery, and to the use of new remedies of great importance, wholly independent of their adoption in Europe. This is as true now as when the bold, free thoughts of Rush were first promulgated to his admiring countrymen. If any doubt this, let them inquire from what foreign school our Western and Southern brethren have derived a knowledge of those means by which they control the long prevalent remittent and intermittent diseases of their localities, almost as if by the wand of the magician? It is not surprising that some of our brethren who, by foreign travel in the early part of their professional career, had become familiar with the schools and hospitals of Paris, where the results of a physiological experiment, or the developments of a *post-mortem* examination, are looked after with as much anxiety as the life of the patient, should have returned with the impression that our country was not only destitute of originality, but destitute also

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of any literature of its own. The fault, however, was in their own vision; or rather in the direction to which their eyes were turned.

They looked not for a literature partaking strongly of our national characteristics, and therefore thoroughly and almost exclusively practical in its character; but they looked at home for a literature characterized by patient and extended physiological and pathological researches corresponding with that of Europe, and in their disappointment perhaps too hastily declared our want of any medical literature worthy of the name.

We have introduced these general remarks here, not merely to counteract the idea that, as a nation, we are without a national medical literature, but, also, by asserting its general character, to enable us to make its defects and means of improvement more apparent and more clearly understood. For, while we assert our claim to a medical literature truly important and national in all its essential features, we by no means claim that it is without defects, or unsusceptible of improvement.

A comprehensive and impartial survey of the medical writings of our country, whether contained in periodicals, monographs, or books, has induced us to refer their defects, directly or indirectly, to four primary sources. These are, *first*, deficiency of preliminary education; *second*, the absence of a clear and definite perception of the fundamental principles of physiology and pathology; *third*, defective modes of investigation; and, *fourth*, too much haste in the preparation of matter for the press. These are more especially the prolific sources of error and imperfection in that part of our literature found in the original department of our medical periodicals.

The *first* is not only productive of those defects which consist in the use of ungrammatical, ambiguous, and inelegant language, but it greatly removes the field of mental vision, and isolates the study of medicine by blotting out its legitimate and necessary connection with the whole vast chain of natural sciences. Man constitutes but one of the links in the lengthened chain of animate beings, and his physiology is so connected with the laws of life and organic action throughout the whole, and the causes of diseases with their means of cure are so intimately interwoven with those sciences that take cognizance of the various departments of the exterior world around us, that the man who attempts to study medicine without a fair preliminary education, undertakes a task much like an effort to comprehend clearly a whole chain by scanning one of its links, or of a complete skeleton by studying one of its prominent bones. Such an

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individual may, and indeed generally does, learn, more or less arbitrarily, the principal facts and precepts of medical science, and often applies them with tolerable skill in the routine of daily practice. But in all his attempts to extend the boundaries of knowledge by contributions to our medical literature, he at once betrays ignorance of the true relation of the facts he has learned to those belonging to all the kindred branches of science. Hence, he is often led to view objects and subjects of study in one aspect only; thereby developing partial and even erroneous conclusions and practices. But the defects in our literature resulting from this source are so obvious as to require no further illustration.

The *second* source of imperfection named, though itself originating in a great measure from the first, is scarcely less prolific of evil consequences. In examining the medical writings of the day, we have been struck with the large amount that appears in isolated statements or cases, reported apparently for the mere purpose of putting them on record; and yet, being observed and written out without any reference to their relation, either to other statements and cases, or to the general principles of that department of science to which they belong, they are generally so imperfectly recorded as to destroy their value, even as isolated facts. We do not mean to intimate that facts should be observed and recorded with special reference to the support of some preconceived theory, but simply that the human mind, when not possessed of a clear perception of the elementary and general principles of a science, is not prepared to comprehend fully and record accurately the observed facts in relation to that science. On the contrary, there will be manifested a constant tendency to omit, both in the observation and the record, some of the points most essential to their value. This, in our estimation, is one of the most prominent faults that mar the medical literature of the day. Its prominence in the medical writings of our country arises in part from those national peculiarities already briefly indicated.

Coming, as many of our young men do, to the study of medicine, without any adequate knowledge of general science, and urged on by the national propensity to grasp for the immediately *practical*, they are not induced to devote that time and attention to the study of physiology, human and comparative, to general pathology, and animal chemistry, which is necessary to the comprehension of those elementary principles and general truths, that must constitute the basis of a sound medical education. They consequently remain through

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life partial observers, often gaining a clear view of only one aspect of a series of facts, by which they almost necessarily develop conclusions in direct conflict with those drawn by others, who approach the same facts from another direction. And thus our literature becomes filled with contradictions apparently irreconcilable, when a clearer knowledge of fundamental principles and a more comprehensive study, would show all the facts to be in strict harmony with each other.

The *third* source of errors and defects in the writings of our professional brethren, namely, defective modes of investigation, is an almost necessary result of the two preceding. Like them, its effects are too plainly visible to be overlooked, even by the casual observer. To investigate any subject properly, the mind must perceive clearly both the relations of the subject to all the surrounding and modifying circumstances, and the means necessary to guard against fallacies in the result. Thus, in performing a physiological experiment, it is not enough that we cut off the head of a chicken or a reptile, and, after watching the motions of the headless trunk, infer that all other decapitated animals are capable of exhibiting the same phenomena. The first object must be to examine minutely the anatomy of the nervous system of the animal experimented on; to ascertain carefully the location and connection of its ganglia or centres, and their comparative relation to the same parts in other animals and in man. The same must be done in relation to its natural habitudes or modes of life. Then, and not till then, can we rightly interpret the phenomena elicited by experiments carefully executed. So, too, in the study of diseases, it is not enough that we observe and record carefully the immediate phenomena they present, and the direct effects of certain remedies. These must be considered in connection with all the surrounding circumstances capable of exerting an influence, such as the climate, season, soil, habits of the people, &c., of the locality where the diseases occurred. Otherwise the data for comparing the phenomena and results of disease in one locality with those in another, are imperfect, and, consequently, the results of such comparison are unsatisfactory, often involving contradictions and controversies almost without end.

Few will fail to recognize here the origin of many most mischievous errors, even in the best portions of our literature. The same rigidly inductive and philosophical methods should be adhered to in the investigation of diseases, that have been pursued with so much success in many other departments of science. The first object

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of the naturalist, for instance, when presented with a number of new objects, differing in some respects from each other, will be to ascertain which of their apparent qualities or phenomena are constant, and which are variable or accidental; next, how far the constant phenomena of each agree with those of all the rest. He is thus enabled to recognize their true relations, and arrange them into classes, orders, and varieties or species.

In the same manner, the true medical philosopher, in examining a class or group of diseases, like the great family of Fevers, for instance, must first ascertain those phenomena connected with their origin, progress, and results which are constant in each case, and separate them from those that appear inconstant and variable. Then, by comparing the constant or essential phenomena of each with all the others, he will be very certain to perceive clearly their true relations and differences. This method, and this alone, will enable him to determine with some degree of certainty, how far diseases of the same general type are identical in their essential pathology, and how far they are distinct. We are satisfied that much of the controversy which has been so long maintained in regard to the *identity*, or *non-identity*, of typhoid and typhus fevers, has arisen from an essentially erroneous mode of investigation.

For instance, in the midst of that ardent cultivation of morbid anatomy, which has rendered the modern medical schools of Paris so renowned, it was found that, in a large proportion of the fatal cases of typhoid fever, ulceration of certain glandular structures in the alimentary canal existed, and of a character so uniform, that it was announced as one of the essential features of the disease. Farther investigations, however, soon developed the fact, both in Paris and elsewhere, that cases presenting all the ordinary phenomena of continued fever during life, were frequently occurring and terminating fatally without leaving any visible ulcerations after death. This, instead of leading to an extensive investigation, for the purpose of determining whether the local lesions found in fatal cases of continued fever were to be classed among the constant and essential or the variable and accidental phenomena of that disease, immediately led to the inference that there were *two* continued types of fever essentially distinct or non-identical. This was no sooner done than the symptoms of the local intestinal lesion became the most prominent means of diagnosis; thus leading to the very unphilosophical practice of basing the diagnosis of the two diseases on the presence or absence of symptoms of the local lesions, and then adducing the

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presence of these same local lesions after death in the one variety, as evidence of its non-identity with the other. We cannot divest ourselves of the belief that much of the controversy, and, perhaps, also many of the errors, which pervade that portion of our literature relating to the great and important subject of fevers, are attributable to this source. And hence we have chosen it at this time to illustrate the importance of adhering, in all our medical investigations, to correct and strictly inductive methods of study. If this could be done, it would soon result in a vast improvement in the character and value of our medical literature. It is much easier, however, to point out errors and even to trace them to their sources, than to devise such means for their removal as will be capable of practical application.

The particular means which have been pointed out as calculated to elevate the character of our medical literature, by preceding committees of this Association, are, the adoption of some measures for increasing the amount of preliminary attainments on the part of students; greater strictness on the part of colleges in their examinations for the Degree of Doctor of Medicine; the public recommendation of the best Inaugural Theses; the offering of premiums for the best original essays by national, State, and county societies; the support of our medical periodicals; the formation of medical libraries, or reading clubs; and the enactment of a proper international copy-right law. The benefits to be derived from the faithful practical application of these means have been fully considered in former reports. They are such as commend themselves to the favour of every enlightened member of the profession. If we have traced the faults of our medical literature to their true sources, there can be no doubt that an improvement in the extent and character of the preliminary education of medical students is a primary and essential prerequisite to the improvement of our literature. It is very true, that examples can be found of individuals who entered upon the study of medicine ignorant of nearly all the branches of a common English education, and yet attained, in after years, great eminence in the profession.

And there may have been periods and countries, in which the time and expense required to obtain an early education were such as to preclude many from its advantages. But most assuredly that time has passed away, so far as our country is concerned; and we feel no hesitation in making the assertion that any individual, who attains

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to the period of early manhood, in the midst of our cheap school books and free schools, without acquiring, in addition to the elementary branches of learning, a good knowledge of the more important department of natural science, possesses neither the ambition, the energy, nor the industry requisite to make him a skilful physician or a successful cultivator of medical science. It is a mistaken idea that the adoption of a rigid rule on this subject, by the whole profession, would exclude many from its study, who would, if admitted, become its most successful cultivators. On the contrary, the adoption of the rule would only serve to impress the minds of such with the importance of the education required, and stimulate them to its full attainment. Hence the rule, once adopted and adhered to, would operate as a timely and proper guide, instead of a barrier to the entrance upon medical studies. We therefore strongly urge upon the Association the adoption of some measures which will cause the State and local societies, throughout the whole country, to take such action as will deter their individual members from receiving young men as students of medicine who are not properly qualified to commence such study. The common and long-continued custom of referring the whole subject of qualifications, both preliminary and medical, to the final examination by a college faculty, is not more injurious to the profession than unjust to the student himself. If the college faculties prove as indifferent in regard to the general education of the student as his private preceptors (and what right have the profession to expect they will not?), the whole profession becomes filled with ignorance and corresponding degradation. On the other hand, if the faculties discharge their duties even with moderate exactness, a large number of students must be annually rejected after they have lost three years of most valuable time, with all the accompanying pecuniary expense; and that too, very often, after they have quietly held the intimate relation of pupils in the offices of respectable practitioners during their whole term of study.

Such results are so manifestly unjust to the pupil that they require no comments. Besides, a rejection, under such circumstances, seldom has the effect to exclude the individual from entering upon the practice of medicine. There is, therefore, no available method of removing the evils, whether literary or otherwise, arising from deficient preliminary education, but to establish the proper guards at the primary threshold in such a way that every student shall know the full extent of the requirements necessary, before he is committed, irretrievably, by a sacrifice of time and money. In fact,

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the same remark is equally applicable in reference to the best means for removing all the evils and imperfections in our literature of which we have complained in this report. They may all be traced ultimately either directly to deficient education, or indirectly to an imperfect comprehension of the extent and relations of the task undertaken. Hence, the great and efficient remedy is to create throughout the entire profession a more liberal appreciation of the extent and relations of medical science and practice, and a more just idea of what the character of our medical literature should be. It matters very little, comparatively, what resolves we as an Association may adopt, or even what laws may be enacted by the legislatures of the several States. In our country, resolutions and laws are alike operative only so far as they accord with the public sentiment of our people.

Our great reliance, therefore, must be on the creation of such a sentiment throughout the profession as will not only restrain individual members from encouraging uneducated young men to enter the ranks of the profession, but as will also develop a nobler idea of what a physician should be, and with it an appetite for a purer, more philosophical, and comprehensive professional literature. The means for creating such a sentiment are already in existence and active operation. They embrace the several faculties connected with the medical colleges, the conductors of our medical periodicals, and the active members of our national, State, and local medical organizations. Besides the direct and simple communication of medical knowledge, those connected with the schools as teachers can and should do much to impress on the minds of their classes a clear view of the nature and extent of the field of study upon which they have entered, and its necessary relations to and dependencies on all the other sciences. A mental task once clearly comprehended is already half accomplished. Many of the more important defects in our literature arise from an inadequate knowledge of what should be included in the investigation of any given subject. All such should be remedied by the teaching of the schools. If the time allotted to collegiate attendance is too short to admit of full instruction in all the departments of medicine and the collateral sciences, it should at least leave a vivid and accurate outline of what ought to be learned, and its importance in view of the honour and usefulness, both to the individual and to the profession of which he is to form a part.

The conductors of medical journals have a still more direct if not more important duty to perform in relation to the creation of that

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professional sentiment of which we have spoken. These journals, as we have already seen, constitute a most important and essential part of our medical literature; and their conductors are not only responsible for the literary character of their contents, but they very properly assume the attitude of censors over every other department of medical writing. It is in their power to exert a very strong influence for good or for evil.

It is true that they cannot control the general character of the articles sent them for publication, much less that of the books furnished them for review. But they can exclude the gross errors of composition, and all unkind personalities from the one; and, by candid and liberal, yet just and truthful notices, do much to elevate the character and improve the qualities of the others; and for this much, at least, they should be held rigidly accountable by the whole profession.

And they can do even much more than this. In performing the task of selecting matter for the pages of their own journal, they pass in review much of the current literature of the profession, and almost necessarily bring under their immediate observation every attempt of their professional brethren to enlarge the boundaries of knowledge, and add to our literature by original investigations in any department of medical science. By a prompt, full, and candid notice of these, let them come from whatever source they may, they would do much to improve a department of medical literature hitherto more neglected than any other in America.

In this respect, the censors of our medical press have not always faithfully fulfilled their obligations to the American medical profession. They have occasionally fallen into the awkward predicament of copying an important article from a foreign journal—translating it from a foreign language—that had been originally published in their own country and mother tongue.

We by no means claim that our medical journalists should sanction the correctness or commend the character of every attempt that may be made to prosecute original inquiries in our country. On these points we would accord to them the widest liberty compatible with an adherence to truth and justice. But, as the legitimate and accredited exponents of the medical writings and investigations of their own country, they are under obligations in all cases to inform their readers promptly of all the original inquiries that are instituted. We would encourage no national prejudices or bigotry on the wide domain of science. Neither would we erect the slightest barrier to the free interchange of knowledge on all subjects. But each

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enlightened nation is represented by its own literary and scientific periodicals, which, by affording encouragement to their own contributors, thereby most effectually advance the interests of the whole. Hence, if the conductors of American medical journals would foster, to the full extent of their power, the literature of their own country, they should make it a rule to give American writings and investigations precedence as authority over those of foreign origin, whenever both are equally applicable to the purpose for which they are required. Another important duty which the editors of our medical journals owe to the literature of the profession, is to improve that department devoted to reviews and notices of books. But this topic was so fully and ably discussed in the report of the Committee, of which Dr. Alfred Stillé was chairman, and which was published in the third volume of the *Transactions* of the Association, that we will simply refer to that paper for a full expression of our sentiments in relation thereto.

The third and last means which we shall notice as capable of aiding to create such a professional sentiment as will result in a rapid improvement of both the medical education and literature of our country, is afforded by our medical organizations. These, if rightly directed, are capable of extending an ennobling and genial influence to the remotest hamlet on our vast national domain.

We have sometimes been asked, "What the great American Medical Association has accomplished? What, indeed, has been effected by any and all our medical societies?" In reply, we would ask, who has come up to our annual gatherings, taken his professional brethren from other States by the hand, mingled with them in the social circle, discussed with them the merits and demerits, the wants and wishes, of the various interests connected with medical science and practice, without having had kindled in his own soul a purer love for his profession, and more active zeal for its elevation and usefulness, and a clearer, nobler view of its ultimate destiny? And who that has returned home has not imparted a portion of all this to his brethren who compose the State and county organizations? Who that listens to, or reads, the elaborate prize essays, and other important papers reported to our national and State societies, does not himself catch something of the spirit of investigation, and feel his ambition rising to a higher purpose?

In these and many other ways our medical organizations directly extend the mental vision, elevate the sentiments, and excite the dor-

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mant energies of the profession, and thereby directly advance all its essential interests, but none to a greater extent than its literature.

All these results of our organizations are already plainly visible in every section of our country, and the organizations themselves only require to be still farther extended, and sustained with a steady and firm hand, to complete their important mission. The Association, at its former annual meetings, has adopted resolutions proposed by former committees on medical literature, touching almost all the particular means that have been alluded to in this report.

Hence we have prepared no new resolutions for the adoption of the Association at this time. And we close the task assigned us with the confident conviction that the literature of our profession has already commenced a rapid improvement, which is destined to continue until it attains an elevation and influence corresponding with the high social and political destiny of our country.

N. S. DAVIS, *Chairman.*

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APPENDIX TO REPORT ON MEDICAL LITERATURE.

ORIGINAL MEDICAL WORKS.

Title.	Author.
Essays on Life, Sleep, Pain, &c. Philadelphia: Blanchard & Lea, 1852,	S. H. Dickson, M. D., &c. &c., Charleston, S. C.
A Treatise on Diseases of the Chest; being a Course of Lectures delivered in the New York Hospital. New York, 1852,	John A. Swett, M. D., &c. &c., New York city.
Review of Materia Medica, for the Use of Students. Philadelphia: Lindsay & Blakiston, 1852,	John B. Biddle, M. D., &c., Philadelphia.
An Essay on Organic, or Life Force,	J. H. Watters, A. B. and M. D.
Hints to the People on the Profession of Medicine. Buffalo, N. Y.,	Maxwell Wood, M. D., U. S. Navy.
A Table of all the known Operations of Ovariotomy, including their Synoptical History and Analysis. Philadelphia, 1852,	Washington L. Atlee, M. D., &c., Philadelphia.
Discourses delivered by Appointment before the Cincinnati Medical Library Association. Cincinnati, 1852,	Daniel Drake, M. D., &c. &c., Cincinnati, Ohio.
Clinical Reports on Continued Fever; based on an Analysis of one hundred and sixty-four Cases, &c. Buffalo: George H. Derby & Co., 1852,	Austin Flint, M. D., &c., Buffalo, Y. Y.
Operative Surgery Illustrated; containing more than nineteen hundred engravings, &c. Boston: Ticknor, Reed, & Fields, 1852,	R. N. Piper, M. D., Boston.
On the Surgical Treatment of Polypi of the Larynx, and Œdema of the Glottis. New York: P. G. Putnam, 1852,	Horace Green, M. D., &c. &c., New York.
System of Operative Surgery, based on the Practice of Surgeons in the United States, &c. &c. Philadelphia: Lippincott, Grambo, & Co., 1852,	H. H. Smith, M. D., &c., Philadelphia.
Notes on Carpenter's Human Physiology; containing some Original Views of the Economy of Nature, &c. 1852,	Louis Mackall, M. D., Washington.
A Treatise on the Constitutional Effects of Uterine Displacements, and their Treatment. Boston, 1852,	Wm. Ed. Coale, M. D., Boston.
Materia Medica, Pharmacology, and Therapeutics. Springfield, Mass., 1853,	Wm. Tully, M. D., &c. &c., Conn.
Report relating to Births, Marriages, and Deaths in Massachusetts, 1851,	Amasa Walker, Secretary of State.
On the Non-contagious Character of Puerperal Fever. Introductory Lecture,	H. L. Hodge, M. D., &c. &c., Philad.
Discourse on the Times, Character, and Writings of Hippocrates. Introductory Lecture,	Elisha Bartlett, M. D., &c. &c., New York.

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ORIGINAL MEDICAL WORKS.

Title.	Author.
The Present Mental Attitudes and Tendencies of the Medical Profession. Introductory Address,	Worthington Hooker, M. D., Conn.
Superiority of Sanitary Measures over Quarantine. Address to Suffolk District Medical Society,	H. G. Clark, M. D., Boston.
Biographical Sketch of J. Kearney Rogers, M. D., &c. &c.,	Edward Delafield, M. D., &c., New York.
An Address on the Life and Character of Samuel B. Latta, M. D.,	M. B. Wright, M. D., &c., Cincinnati.
A Memoir on the Life and Character of James B. Rogers, M. D.,	Joseph Carson, M. D., &c., Philadelphia.
A Biographical Sketch of the late Daniel Drake, M. D., &c. &c.,	S. D. Gross, M. D., &c. &c., Louisville, Ky.
Elephantiasis Arabum of the Right Inferior Extremity, successfully treated by Ligature of the Femoral Artery: with a Plate. New York, 1852,	J. M. Carnachan, M. D., &c., New York.
Remarks on Osteo-Aneurism, with a Case involving the Condyles of the Left Femur: with two Plates. New York, 1853,	J. M. Carnachan, M. D., &c. &c., New York.
Hydatids of the Liver, Operation and Cure. New York, 1852,	J. Edward Weber, M. D., New York.
New Views on Provisional Callus. Buffalo, N. Y., 1853,	Frank H. Hamilton, M. D., &c. &c., Buffalo.

NEW EDITIONS OF AMERICAN MEDICAL WORKS.

Title.	Author.
Outlines of the Nerves; with Short Descriptions. Designed for the Use of Students. Second edition. Philadelphia: Barrington & Haswell, 1852,	John Neill, A. M., M. D., &c., Philad.
Outlines of the Arteries; with Short Descriptions. Designed for the Use of Medical Students. Second edition. Philadelphia: Barrington & Haswell, 1852,	John Neill, A. M., M. D., &c., Philad.
History, Diagnosis, and Treatment of the Fevers of the United States. Third edition; revised. Philadelphia: Blanchard & Lea, 1853,	Elisha Bartlett, M. D. and Prof., N. York.
A Treatise on the Practice of Medicine. Third edition. Philadelphia: Lippincott, Grambo, & Co., 1852,	Geo. B. Wood, M. D., Prof., &c., Philad.
An Analysis of Physiology. Designed especially for the Use of Students. Philadelphia: Lindsay & Blakiston, 1852,	J. J. Reese, M. D., Philadelphia.
A Practical Treatise on Dental Medicine. Second edition, corrected and enlarged. Philadelphia: Lindsay & Blakiston,	T. E. Bond, M. D., Baltimore.
Obstetrics; the Science and the Art. Philadelphia: Blanchard & Lea. Second edition, 1852,	C. D. Meigs, M. D., and Prof. of Medicine, &c., Philad.
An Analytical Compendium of the Various Branches of Medical Science. For the Use of Students. Second edition,	John Neill, M. D., and F. G. Smith, M. D., Philadelphia.
The Medical Student's Vade-Mecum: a Compendium of Anatomy, Physiology, Chemistry, Materia Medica, and Pharmacy, Surgery, Obstetrics, Practice of Medicine, Diseases of the Skin, Poisons, &c. &c. Third edition; 224 engravings. Philadelphia: Lindsay & Blakiston, 1852,	Geo. Mendenhall, M. D., &c., Cincinnati, Ohio.

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REPUBLICATION OF RECENT FOREIGN WORKS.

Title.	Author.
Illustrated Manual of Operative Surgery and Surgical Anatomy. Edited, with Notes and Additions, &c. &c., by William H. Van Buren, M. D., Surgeon to Bellevue Hospital, &c.; and C. E. Isaacs, Demonstrator of Anatomy, &c. New York: H. Balliere, 1852,	MM. C. Bernard, D. M. P., and C. Huette.
Lectures on the Principles and Practice of Surgery. Philadelphia: Blanchard & Lea, 1852,	Bransby B. Cooper, F. R. S., Surg. to Guy's Hospital, &c., London.
Principles and Practice of Surgery. Illustrated by 316 engravings on wood. Edited, with Additions, by John Neill, M. D., &c. &c. Philadelphia: Blanchard & Lea, 1852,	Wm. Pirrie, F. R. S. E., &c., Aberdeen.
A Practical Treatise on Diseases of the Skin. Philadelphia: Blanchard & Lea, 1852,	J. Moore Neligan, M. D., M. R. G. A., &c.
On Syphilis, Constitutional and Hereditary: with four colored plates. Philadelphia: Blanchard & Lea, 1852,	Erasmus Wilson, F. R. S.
General Pathology, as Conducive to the Establishment of Rational Principles for the Diagnosis and Treatment of Disease, &c. &c. Philadelphia: Blanchard & Lea, 1852,	John Simon, F. R. S., &c.
Letters on Syphilis; addressed to the Editor of the Union Médicale. Translated by W. P. Lattimore, M. D. Philadelphia: A. Hart, 1852,	Ph. Ricord, &c., and Amédée Latour.
God in Disease; or, the Manifestation of Design in Morbid Phenomena. Philadelphia: Lindsay & Blakiston, 1852,	John F. Duncan, M. D., &c.

TRANSACTIONS OF MEDICAL SOCIETIES.

Title.	Where published.
Quarterly Summary of the Transactions of the College of Physicians of Philadelphia, 1852,	Philadelphia.
Transactions of the Twenty-ninth Annual Meeting of the Medical Society of Virginia; with the President's Address,	Richmond, Va., 1852.
Transactions of the Medical Association of the State of Missouri, at its Second Annual Meeting,	St. Louis, April, 1852.
Proceedings of the Medical Association of the State of Alabama. Fifth Annual Meeting,	Mobile, 1852.
Transactions of the Medical Society of the State of Pennsylvania, Vol. II.,	Philad., May, 1852.
Transactions of the Medical Association of South-Central New York. Annual Meeting, June, 1852,	Auburn, 1852.
Transactions of the Third Annual Meeting of the Medical Society of the State of Georgia,	Penfield, 1852.
Transactions of the Illinois State Medical Society for 1852,	Peoria, 1852.
Proceedings of the Third Annual Session of the Indiana State Medical Society,	New Albany, May, 1852.
Transactions of the American Medical Association, Vol. V. pp. 939,	Philadelphia, 1852.

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TRANSACTIONS OF MEDICAL SOCIETIES.

Title.	Where published.
Proceedings of the Louisiana State Medical Society, at its Third Annual Session, March, 1852,	Louisiana.
Transactions of the Medical Society of the State of New York. Held, February 3, 1852,	Albany, N. Y., 1852.
Transactions of the Medical Society of Kentucky, Louisville, 1853,	Louisville, Ky., 1852.

AMERICAN MEDICAL JOURNALS.

Title.	By whom edited.
American Journal of Medical Sciences. Philadelphia, Pa.,	Isaac Hays, M. D.
Boston Medical and Surgical Journal. Boston, Mass.,	J. V. C. Smith, M. D.
Southern Medical and Surgical Journal. Augusta, Ga.	L. A. Dugas, M. D.
New York Medical Times. New York,	H. D. Bulkley, M. D.
New York Medical Gazette and Journal of Health. New York,	D. M. Reese, M. D.
Southern Journal of Medical and Physical Sciences,	J. W. King, M. D., W. P. Jones, " R. O. Currey, " B. Wood, "
Ohio Medical and Surgical Journal. Columbus, Ohio,	R. L. Howard, M. D.
New York Journal of Medicine and Collateral Sciences. New York,	S. S. Purple, M. D.
New Orleans Medical and Surgical Journal. New Orleans, La.,	A. Hiester, M. D.
Buffalo Medical Journal and Review. Buffalo, N. Y.,	Austin Flint, M. D.
The Medical Examiner. Philadelphia, Pa.,	F. G. Smith, M. D., and J. B. Biddle, M. D.
East Tennessee Record of Medicine and Surgery. Nashville, Tenn.,	F. A. Ramsay, M. D.
North-Western Medical and Surgical Journal. Chicago, Ill.,	W. B. Henick, M. D., and H. A. Johnson, M. D.
Western Medico-Chirurgical Journal. Keokuk, Iowa,	J. F. Sandford, M. D.
Medical Recorder,	A. P. Merrill, M. D., and C. F. Quintard, M. D.
Stethoscope and Virginia Medical Gazette. Richmond, Va.,	P. C. Gooch, M. D.
New Orleans Monthly Medical Register. New Orleans, La.,	A. F. Axson, M. D.
Western Lancet. Cincinnati, Ohio,	L. M. Lawson, M. D.
Charleston Medical Journal and Review. Charleston, S. C.,	D. J. Cain, M. D., and F. P. Porcher, M. D.
New Hampshire Journal of Medicine. Concord, N. H.,	E. H. Parker, M. D.
Nashville Journal of Medicine and Surgery. Nashville, Tenn.,	W. R. Borling, M. D., and P. F. Eve, M. D.
St. Louis Medical and Surgical Journal. St. Louis, Missouri,	Drs. Linton and McPheeters.
New Jersey Medical Reporter. Burlington, N. J.,	Joseph Parrish, M. D.
Western Journal of Medicine and Surgery. Louisville, Ky.,	L. P. Yandell, M. D., and T. S. Bell, M. D.
Transylvania Medical Journal. Lexington, Ky.,	L. G. Frazer, M. D.

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AMERICAN MEDICAL JOURNALS.

Title.	By whom edited.
L'Union Médicale. New Orleans, La.,	Charles Delery, D. M. P., and Edward Martin, D. M. P.
American Journal of Pharmacy. Philadelphia, Pa.,	W. H. Procter, Jr.
New York Journal of Pharmacy. New York,	B. W. McCready, M. D.

COLLATERAL JOURNALS.

Title.	By whom edited.
American Journal of Insanity. Utica, N. Y.,	New York State Lunatic Asylum.
American Journal of Dental Science. Baltimore, Md.,	Chapin A. Harris, M. D., D. D. S., and A. A. Blandy, M. D., D. D. S.

REPUBLICATION OF FOREIGN JOURNALS.

Title.	By whom issued.
British and Foreign Medico-Chirurgical Review,	S. S. & W. Wood, New York.
London Lancet,	Burgess & Stringer, New York.

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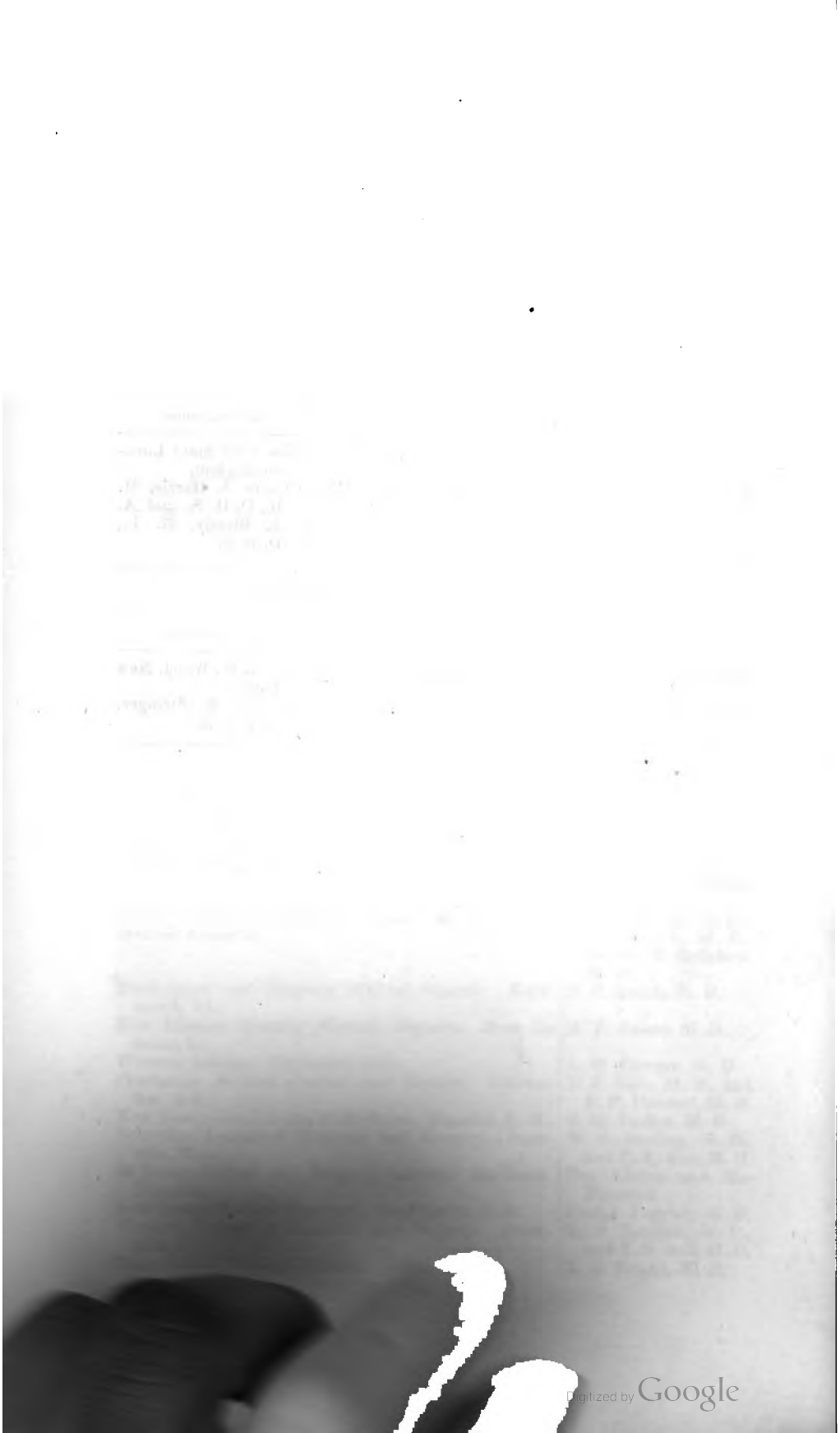
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ON THE

AGENCY OF THE REFRIGERATION,

PRODUCED

BY UPWARD RADIATION OF HEAT,

AS AN

EXCITING CAUSE OF DISEASE.

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ON THE AGENCY OF THE REFRIGERATION, PRODUCED BY UPWARD RADIATION OF HEAT, AS AN EXCITING CAUSE OF DISEASE.

THE profession of medicine not only embraces a knowledge of what may be regarded as the immediate curative appliances, but in its wide scope takes cognizance of all agencies which may operate as predisposing or exciting causes of disease. It is the physician's duty to look after the means of prevention as well as those of cure, and the importance of everything connected with the preservation of health is daily becoming more highly appreciated.

Exposure of the body to vicissitudes of temperature has always been regarded among the most common causes tending to disturb the healthy action of the functions of the animal economy. Whilst the evil influences exerted by excessive heat are numerous, those resulting from sudden reductions of temperature are among the most frequent causes of disease. It is of little consequence how this reduction is brought about, whether through exposure to rain or other atmospheric inclemency, to the cold and damp air of cellars or other apartments, to currents of air, damp sheets or wet clothing—the mischievous effects produced by the refrigeration are the same.

Besides the refrigerating causes thus referred to, another exists which we believe plays a much more active part in the production of disease than is generally supposed, and deserves to be better understood in order to be properly guarded against. We refer to *terrestrial radiation;* the following observations upon which subject are offered as rather suggestive, than the results of matured investigations. They are designed to make better known the activity of an agent to which persons and all objects under atmospheric exposure are subjected, and the effects of which are too generally ascribed to conduction, evaporation, or some other means through which sensible heat is carried off.

For the better understanding of such as have not devoted any par-

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ticular attention to the laws regulating the distribution of heat, it may be useful to add a few explanations, and, at the same time give our precise meaning of the natural process termed *terrestrial radiation.*

In every change and operation connected with the existence of living objects, as well as the movements taking place in the material world, the primary agency of heat is conspicuously displayed. Consequently, we find that physical laws exist to promote its active diffusion and circulation, and by their operation heat is kept constantly passing and repassing from body to body, and from place to place, wherever inequalities of temperature exist. When substances of unequal temperature are in contact, the warmest impart their surplus sensible heat to the coldest, through the agency of the well-known law of *conduction.* But when substances are separated from each other, so that no heat can be thus transmitted, the circulation of this subtile element will still take place, the heat from the warmer being enabled to make its way through the air to the cooler objects, until all attain an equable temperature. This process, which we may observe on a small scale in our apartments, is incessantly going on out of doors, throughout the whole area of nature. By day, when the sun is unobscured, substances on the earth's surface generally receive more heat from the sun than they part with, even during the absence of the sun during the short nights of summer. At night the operation being reversed, objects on the earth's surface give off the heat received during the day with great rapidity, so that during the long nights of autumn and winter, much more heat is emitted from them than was received from downward radiation in the day.

The term *radiant heat,* was, we believe, first employed by Scheele, the celebrated Sweedish chemist and philosopher, to designate the heat cast off from bodies in rays, and it is to him we owe the discovery that these rays pass through the atmosphere without any change in their direction.*

Meteorology contemplates radiant heat as imparted to the surface

* Radiant heat must not be confounded with *reflected heat,* which last consists of rays repelled from the surfaces of objects, whereas, radiant heat, is that which has been received into bodies and subsequently emitted by them. As a general rule those substances which reflect or resist best the entrance of heat, part most reluctantly with that which they contain within them, and *vice versa.* A vessel or any body with a burnished surface will resist the radiation of its internal heat, and, at the same time, repel the entrance of heat coming upon it from without. A dark or roughened surface admits the ready entrance of heat impinging upon it from without, and subsequently favours its speedy passage off by radiation.

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of the earth during the presence of the sun, and as afterwards thrown off from terrestrial objects, when the influence of the sun is withdrawn. The first-named process is termed *solar*, or downward radiation, the second *terrestrial*, or upward radiation. Viewed in either aspect, radiant heat forms one of the most important elements of meteorological science, exercising very active and extensive agencies in the production of natural phenomena. The rapid manner in which heat is accumulated by objects exposed to bright sunshine is a matter of familiar observation. But the exceeding rapidity with which such objects throw off their heat on the withdrawal of the sun would be credited by few, were it not proved by many exact experiments. It is the refrigerating agency exercised by this upward radiation, which led Dr. Wells to solve the previously unexplained phenomenon of the formation of dew, and to furnish one of the happiest examples of inductive science that has ever been given. Before his experimental researches were made, no one suspected the real cause why substances exposed to the clear open sky usually become so much colder during the night than the atmosphere only a few feet or inches above them. The well known refrigerating effects of evaporation was supposed by some to afford a sufficient explanation, whilst others referred it to some obscure chilling influence arising from the formation of dew—an effect and not the prime cause of this deposition.*

When Dr. Wells began the course of experiments which were crowned with such happy results, he knew little or nothing of the active agency exercised by terrestrial radiation, his mind, as he tells us, being fully prepossessed with the common error that the coldness observed during the formation of dew, was an *effect* of the deposition. His main object was merely to ascertain the difference of temperature that might arise from this cause between the herbage and surface of the earth, and the air above them. By examining farther, however, and referring to certain laws of heat recently developed by the researches of Count Rumford, Mr. Leslie, and others, he was afterwards led to regard the subject in an entirely new light,

* Dr. William Charles Wells (M. D., F. R. S., L. & E.), was born and brought up in Charleston, South Carolina, where he studied medicine with Dr. Garden, an eminent physician and natural philosopher. He left this country for Europe at the commencement of the Revolution, finished his medical education in Edinburgh, and afterwards settled in London. Here he became highly esteemed for his sound intelligence and powers of close investigation. These introduced him into the Royal Society, and gained him distinction among the learned. But although he became one of the physicians to St. Thomas's Hospital and Finsbury Dispensary, his success in obtaining private practice was comparatively moderate.

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and finally to demonstrate that the cold which accompanies dew is not an effect, but the true and only cause of its deposition. That dew, in fact, is deposited upon grass, leaves, &c., for exactly the same reason, that moisture condenses upon a pitcher or glass, the outside of which has its temperature reduced by cold within to the degree at which atmospheric vapour condenses. The common expression of "falling dew," conveys, therefore, an erroneous impression, as dew neither falls nor rises, but is merely the deposition of watery vapour which has come in contact with objects cooled down to the "dew-point." The formation of dew must be a process in which heat is developed instead of cold; as, in the condensation of vapour, its abundant latent heat is set free and becomes sensible heat.

The heat thus thrown out acts as a check to the quantity of dew formed, and tends to limit the effects of refrigeration caused by radiation, which on many occasions would proceed so far as to prove highly injurious to vegetation. It might cause the deposition of more dew than was beneficial to growing plants or convenient for the drying and harvesting of others. It might cause, at some seasons, the congelation of light dews into destructive frosts, or heavier depositions into sleet.

Let us now examine the results of some of the experiments which have been made by Dr. Wells, and strictly confirmed by those since made by Mr. Daniell and other accurate observers, for the purpose of ascertaining the extent of coolness produced through the process of terrestrial radiation. It must be borne in mind that different objects radiate heat in different degrees, and hence will exhibit different degrees of temperature when tested by the application of the thermometer. Snow, when exposed to the clear sky at night, is an exceedingly good radiator; and hence a thermometer placed upon its upper surface will, on such occasions, always be found many degrees lower than another instrument suspended three or four feet above, and veiled from the sky to prevent radiation. Swan's-down, taken from the breast of the bird, was found by Dr. Wells a better radiator than snow. In one of his experiments he showed that, on a particular evening, when the temperature of the atmosphere at 8½ o'clock was 22½° at the height of four feet—that of the snow below was 13°, and of swan's-down lying upon the snow, only 8°. At another time, when the snow was about a foot deep, the swan's-down became 13½°, and the surface snow about 17° colder than the air. Snow, therefore, though an excellent radiator, is not quite equal to swan's-down;

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the radiating power of which is about upon a par with that of raw silk, cotton, and flax.* His experiments made in the summer, by placing, during serene and still nights, one thermometer upon the dewed grass, and another from one inch to nine feet above the ground, always showed a great depression in the lower instrument. He, however, generally compared the temperature of dewed grass with that of the air four feet above the ground, and very frequently found the grass 7, 8, or 9 degrees colder than the air at that height. Many times it was 10°, 11°, and once he observed it as much as 14° lower than the air four feet above.

The necessity of a clear sky for the perfect performance of the function of radiation, and the degree to which it is limited by clouds, is beautifully shown by Dr. Wells. He informs us that if the night became cloudy, after having been very clear, though there might be no change with respect to calmness, a considerable alteration in the temperature of the grass always ensued, and this sometimes very suddenly. Upon one night, he says, the grass after having been 12° colder than the air, became only 2° colder than it, the temperature of the air above remaining the same at both observations. On another night, grass became 9° warmer in the space of one hour and a half; on another the temperature of the grass rose 15° in the short space of forty-five minutes, whilst that of the neighboring air increased only 3½°. During a fourth night, the temperature of the grass at half past nine o'clock was 32°. In twenty minutes after it was found to be 39°, the sky having in the mean time become cloudy. At the end of twenty minutes more, the sky being clear, the temperature of the

* In experimenting with different substances, in order to discover which attained the greatest coolness on exposure to radiation, Dr. Wells found those the most productive of cold to be the filamentous and downy, as wool of moderate fineness, very fine raw silk, very fine unspun cotton, fine flax, and swan's-down, all of which were more steadily cold, upon clear and calm nights than grass. Wool became the least cold of the substances named, and had less dew upon it than silk, cotton, and flax; the three last of which were found nearly equally good radiators with swan's-down. Fresh unbroken straw, and shreds of white paper, though not properly to be ranked among filamentous substances, were found nearly equal with wool in the production of cold. The quantity of dew or frost found upon different substances early in the morning, furnishes a good index of their various radiating capacities. Pieces of wood, charcoal, rags, feathers, felt, leather, or other rubbish scattered over a neglected yard exposed to the open sky, afford interesting subjects for observation. The web of the spider must be an admirable radiator, judging by the large quantity of dew commonly found upon it. A field of grass, covered with gossamer and spangled with irridescent dew-drops, is a beautiful sight. From these observations very good notions may be formed of the kinds of clothing affording the best protection against the effects of nocturnal radiation.

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grass was again 32°. He frequently saw during nights generally clear, a thermometer lying on a grass-plot rise several degrees, upon the zenith being occupied only a few minutes by a floating cloud.*

Such are some of the differences which have been observed between the temperature acquired by the herbage covering the ground and that of the air above it. On bare ground the results are very different. In still and serene nights Dr. Wells found, two and a half hours after sunset, the surface of a gravel walk 16½°, and that of the garden mould 12½° warmer than the grass very near to them and similarly exposed to the heavens.

Now this great variation between the temperature of the naked ground and adjacent herbage must not be regarded as owing so much to differences in the capacities for radiation as to another process connected with the distribution of heat, namely, direct conduction, which produces a compensation for the loss through radiation. The grass is a very bad conductor of the heat existing in the soil beneath, which cannot thus operate upon the thermometer lying upon it, and the thicker the herbage the more effectually will it cut off the warmth of the earth when this happens to be warmer than the air above it. But the naked gravel walk and soil, although they may pass off heat rapidly through radiation, will be supplied almost as fast, by conduction, with warmth from below. Such places will, therefore, often be free from the deposition of dew whilst the herbage on the borders may be reeking wet.

It must be obvious that the means employed to protect persons, or any other objects, from the effects of radiation, should not be placed in contact with them, since the cold would be immediately transferred by conduction from the shelter to the surfaces beneath.

In the open country the coolness of the night air is much greater than in towns where more heat is absorbed during the day, to compensate for more limited loss at night by partial radiation. Calm nights in summer, with the sky overcast, are always the most sultry and oppressive, because the heat accumulated in the day is prevented by the clouds from making its escape upwards. On the other hand, the coolness at night commonly observed (especially in

* Meteorological observers seldom take into view the effects of radiation upon their thermometers exposed to the open air at night, and hence often report temperatures not representing those of the air, but such as are acquired by the instrument through radiation. If, during the day, it be necessary to shade thermometers for ascertaining atmospheric temperatures, it is equally proper at night to prevent radiation from them by intercepting the view of the open sky.

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the country), in the lower apartments of houses, arises from the refrigeration produced at the surface of the earth through radiation. Hilly and mountainous regions show the activity of terrestrial radiation most forcibly. In such elevated places the sun is scarcely set before a piercing chilliness is felt in the lower stratum of the atmosphere.

Amphibious animals show a great fondness for the heat derived from solar radiation through the day, but retreat into water at night, to escape the coolness to which they would be exposed on land from terrestrial radiation. This was observed of the Egyptian crocodile by Herodotus, who states that these animals, after spending most of the day on shore, submerge themselves in the Nile at night, in order to escape the coldness which accompanies dew.

Every one acquainted with rural life must have observed that the most abundant dews occur upon very clear nights, when there is little or no wind to favour its evaporation, or to sweep the upper and warmer air in contact with the surface. Sometimes, indeed, even in calm nights, there will be no dew because the *dew-point* may be so low that the reduction of temperature through radiation does not reach it. A fog necessarily obstructs radiation, but if there be no thick clouds above, a partial dew may be condensed, showing that radiation has not been entirely suspended.

By the term *dew-point*, we mean the degree of temperature at which atmospheric vapour begins to condense. Various means have been contrived for ascertaining the *dew-point*. One of the most simple is to take a cut-glass tumbler of water, and add ice or saline mixtures to bring down the temperature of the fluid. When the first film of condensed moisture is observed on the outside of the glass, a thermometer is to be plunged in, and the degree to which the mercury falls is the *dew-point*, or temperature at which vapour condenses in the open air, to constitute dew, fog, mist, or cloud. If this experiment be made in the evening, a thermometer placed upon the grass, will be found at the very same degree when the first dew is observed to form upon the herbage. The dew-point when ascertained, may enable us to estimate the height of clouds floating in the atmosphere. On ascending above the earth's surface, in the temperate latitudes, the air becomes colder at the rate of about one degree Fahrenheit for every 300 feet of elevation. When, therefore, the *dew-point* is found to be 60°, the temperature of the atmosphere at the same time being 70°, the degree of coldness necessary to produce the condensation of

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vapour and formation of clouds would be reached at the height of about 3000 feet.

Having said thus much in explanation of the nature of terrestrial radiation, and referred to some of its ordinary effects, let us next proceed to show how this rapid means of inducing refrigeration may become active in the production of disease.

The ill effects ascribed to nocturnal exposure are proverbial, so much so that a popular notion exists that the air at night is fraught with certain qualities injurious to health not present in the day.

Most authors who have treated upon the atmospheric influences to which persons are exposed, have confined their attention to the conditions of excessive moisture or malarious effluvia, the products of putrefaction. During the existence of such circumstances, according to our view, let any one be subjected to the chilling influences exerted through upward radiation from exposure during the night to the clear open sky, and fever of the specific character prevalent will be the result, or, in the absence of such epidemic agents, diarrhœas, rheumatisms, or other inflammatory affections may occur instead.

We have read of armies exposed in malarious districts, where the troops escaped sickness from having the slightest protection from the open sky. It can hardly be supposed that the air respired by men so little sheltered, was less fraught with the poisonous malaria abroad than such as was breathed by those less sheltered. To what, then, shall the immunity enjoyed by the former be ascribed? We think, mainly to the limits, more or less partial, interposed to radiation, and consequent chilliness, and secondarily to the shade afforded during the day from the direct rays of the sun.

We can well recognize the ill consequences arising from a change of temperature in our apartments of 8 or 10 degrees, below that to which we have been habituated. If, for example, we have kept the thermometer at 70°, and, through neglect, the mercury falls to 62° or 60°, we shall soon be warned of the loss of our natural stock of animal heat, by sneezing or coughing, even before uncomfortable feelings of chilliness may ensue. It only requires this state of things to be kept up sufficiently long, to produce or develop more or less serious disease. If such should be the case within doors, what is to prevent the ill effects from refrigeration produced from exposure out of doors. Here we may be met with the observation that persons frequently pass from warm apartments into the open air, where the change of temperature may be equal to 100° or more, and yet remain well. In Sweden and Russia, bathers pass, we are told, out of the

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chambers of the steam baths heated to 150°, and even higher, into the external air at zero, or have cold water thrown on them, without suffering any injury to health, but, on the contrary, receiving refreshment and invigoration.

Here no partial checks are imposed upon the functions, the impression upon the system being equal, and the reaction consequently general.

The immunity enjoyed by so many who escape serious injury to health from the hydropathic packing in cold wet sheets, when they would probably have suffered from going to bed in merely damp sheets, can, we think, only be explained by the greater care taken in the first instance to superadd a larger amount than ordinary of dry wrappings, such as sheets, blankets, and often down and feather beds, in this way preventing evaporation and the escape of animal heat.

In cities there is a compensation for the loss of heat through radiation. The walls and pavements absorb more heat by day, than they can possibly throw off at night. Hence the remaining heat prevents the detrimental effects of refrigeration from that partial radiation which alone can take place in cities. Persons can therefore go about at night in cities with almost as much impunity, as by day, and this during the prevalence of epidemic influences, which intrude themselves no farther than their precincts.

The prime object of those charged with the duty of preserving the health of armies has been the protection of troops from the heat of the sun. "To palliate the effects of intemperate heat, commanders have found it expedient so to direct the marches, that the men should come to their ground before the heat of the day; and to give orders that none of them sleep out of their tents; which in fixed encampments may be covered with boughs, to shade them from the sun."

So writes Pringle in his great work on *The Means of Preventing Diseases in an Army*. Freely assenting to these views, we will add, that we attach even more sanitary influence to the direction to keep under the tents at night, in order to escape the chilling influences of upward radiation, going on at that time, than to the direct solar radiation by day.

That much sickness is thus produced, must, we think, be apparent to any one who knows that on calm and very clear nights bodies exposed to the open sky are liable to a reduction of 10° or 20° of temperature, through the effect of radiation. That is to say, whilst a

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thermometer suspended in the open air, under a tent or other shelter, may indicate a temperature of 80°, a person outside, exposed to the clear sky, may be subjected to a refrigerating agent, calculated to produce a reduction of temperature sufficient to bring down the thermometer to 65° or even 60°. The effects of a refrigeration much less than here stated, would, if long kept up, tend to disturb the healthy functions of the animal economy, especially in those who are peculiarly sensitive to changes of temperature. Where the air is cold, fires are required to keep up the proper degree of warmth to maintain health. Not so, however, where the effects of cold from radiation are to be guarded against. The slightest covering, even the flimsy protection afforded by a pocket-handkerchief, or a bush from a tree, will suffice to intercept the view of the sky, and thus prevent radiation with its mischievous consequences.

"I had often," says Dr. Wells, "in the pride of half knowledge, smiled at the means frequently employed by gardeners, to protect tender plants from cold, as it appeared to me impossible that a thin mat or such flimsy substance, could prevent them from attaining the temperature of the atmosphere, by which alone I thought them liable to be injured. But when I had learned that bodies on the surface of the earth become, during a still and serene night, colder than the atmosphere, by radiating their heat to the heavens, I perceived immediately a just reason for the practice which I had before deemed useless."

To obviate the uncomfortable and injurious effects of solar radiation during the day, an umbrella or parasol is a common resource. The individual, however, who should hoist an umbrella over his head of a clear night, with a view to protect himself from cold, would be an object of almost universal ridicule. This shows the very little knowledge generally prevailing of the nature and extent of terrestrial radiation, and the evils to which it may give rise.

In farther illustration of our views, let us suppose the case of an army in actual service, the medical officers of which were not well acquainted with the extent to which refrigeration may be carried through the agency of terrestrial radiation. The causes of sickness are abroad, and nothing wanting to develop their effects but some means by which the healthy actions of the economy shall be disturbed. Getting wet, or any other mode of "catching cold," will be the means of letting in the enemy. The weather may be warm and serene, circumstances tempting the men to remain out of their tents, exposed to the clear sky, and here they remain unconscious of

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danger—for they have no one to warn them of it—until they become insensibly chilled to the point of "taking cold," with all the evils to which this may open the way.

In reading over the accounts of military campaigns, we have had the strongest reasons for believing that much of the sickness recorded has had its origin from neglect to guard against terrestrial radiation, the noxious effects of which were not sufficiently recognized.

The following observations, made by the late Dr. Robert Jackson, Inspector-General of the British Army Hospitals, are worthy of remark in connection with our subject.

"Irregular troops, which act in the advanced line of armies, and which have no other shelter from weather than a hedge or tree, rarely experience sickness, never at least the sickness which proceeds from contagion; hence it is inferred that the shelter of tents is not necessary for the preservation of health. Irregular troops, with contingent shelter only, are comparatively healthy, whilst sickness often rages with violence on the same scene, among those who have all the protection against the inclemencies of weather which can be furnished by canvas."

In objecting to the use of tents, Dr. Jackson takes into view two considerations. 1. The effects of moisture, and 2, those of infectious air. When constructed without due regard to ventilation, tents must undoubtedly promote the spreading of sickness; for when tight the pointed tent will hold but little air, and this little being pent up in such a small space, must very quickly become contaminated by numbers crowded into it; so that all the advantages sought from tents as means of protection from atmospheric exposure, would be more than neutralized in a close tent stretched over seven or eight men. A more perfect life-extinguisher than this could not, we think, be easily contrived. It is important to note the testimony borne in the foregoing extract to the security from sickness observed by a veteran campaigner, in those portions of an army, "having no other shelter from weather than a hedge or tree," or, in other words, only sufficient to shield against bleak winds and the effects of radiation. If four poles were stuck into the ground, so as to suspend upon them a blanket or any light material, to break off exposure to the open sky, whilst free circulation of air was maintained among those below, there can be no doubt that better health might be maintained among troops than would follow the use of close tents.

Among a host of errors proceeding out of the common practice of imputing effects to wrong causes, are those in which the moon is

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charged with influences over the animal economy and many natural phenomena, obviously owing to other agencies, and mainly to terrestrial radiation. Of these, we shall adduce a few examples.

At a certain period of the spring, young plants are often observed to become blighted, their leaves assuming russet hues. These injuries have been popularly ascribed to certain properties possessed at this particular season by the rays of what the French term *la lune rouge*, or red moon, and the English the *April moon*. The effects upon the plants here referred to, occur just at the interval between cold and warm weather, a season when the temperature of the atmosphere may continue all night several degrees above the freezing point, at 40°, or more, for example, whilst objects on or near the earth may be reduced to 32°, or even lower, by radiation. If the night be cloudy, no injury is apprehended, and the gardeners generally believe that the escape of their plants is owing to the moon's rays being kept off by the intervention of the clouds. Any one acquainted with the extensive agency exerted by terrestrial radiation in the production of cold, will readily acquit the moon from any agency in the production of the noxious influences ascribed to her in the present instance. It is the clearness of the atmosphere which allows our satellite to shine brightly, and it is the same condition of the air which favours radiation.

Some other accusations are brought against the moon, for deleterious influences, which are also clearly chargeable to the agency of terrestrial radiation. Among these is the effect upon fish and other animal substances, left exposed to the moon's rays, and which are undoubtedly found to spoil sooner than if kept under some kind of shade. The observation is very old, as both Pliny and Plutarch speak of it, and, what is more to the purpose, ascribe the effect to the proper cause, namely, "a copious humidity" shed upon the substances, which hastens their putrefaction. But they ascribe this deposition to certain inherent properties belonging to the lunar rays, whereas, had they been acquainted with the principle of terrestrial radiation and its effects, as demonstrated by Dr. Wells eighteen centuries afterwards, they would have been enabled to designate the true cause of the copious humidity which is shed on clear nights upon meats, and which, when the weather is warm, greatly promotes their putrefaction. This is doubtless as apt to happen during clear starlight nights as on moonlight nights, for radiation and the formation of dew are regulated by the clearness of the atmosphere, being as great in the absence of the moon as during her presence. In this

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as well as the former instance, the moon is but a quiet spectator of the mischief so generally charged upon her.

That the moon, in her changes, exercises an influence upon the atmosphere to determine whether the weather shall be wet or dry, is a very ancient belief, which still remains popular, and has, indeed, the sanction of some few men at the present day eminent for science. Mr. Daniell, of London, a distinguished meteorologist, seems inclined to think there are grounds for entertaining the opinion, his main argument being deduced from the supposed influence exerted over the aerial ocean through gravity. We will not deny that such a force may produce tide-swells in the aerial ocean, as it is known to do in the sea. But the existence of such phenomena does not prove the power of the moon to regulate the weather, whether it shall be wet or dry, as these conditions are not, we think, dependent upon agencies connected with gravity, but solely regulated by temperature. It has been demonstrated by Dalton and others, that watery vapour owes its suspension entirely to heat, deprived of which, through any means of refrigeration, it is condensed into dew, mist, cloud, rain, and other forms of aqueous deposits and precipitations. Gravity and temperature, we think, exercise entirely distinct agencies over terrestrial phenomena.

The notion of lunar influence in the production of disease, still has many supporters in modern times, who cite in its favour the authorities of Galen among the ancients, and Mead and Hoffman in later times. A work has, indeed, been recently published in England, by a Mr. Howard, advocating the opinion of lunar agency in the production of disease. In almost every instance cited in proof, the noxious agencies attributed to lunar influence are, we think, due to terrestrial radiation, or some other physical principle, over which the satellite has no control. The charges against the moon are, therefore, as baseless as those brought by the advocates of astrology against the stars. We would, however, make one observation in favour of lunar influence exerted over persons labouring under mental derangement or cerebral excitement of any kind, upon whom moonlight may exert the same irritating effects, as light derived from any other source.

We have been told by those who have witnessed the facts, that sailors whilst asleep on deck, with the moon shining full upon their faces, had been seen with their features distorted, so as to exhibit frightful expressions. All this we could readily believe; but when told that these distortions were owing to noxious influences inherent

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in the moon's rays, the explanation was entirely unsatisfactory, the cold induced by upward radiation being quite sufficient to account for the spasmodic contortions. Such effects occur as frequently under exposure to the clear sky, when there is no moonlight to render them visible. Violent cases of tetanus are of common occurrence in southern countries, especially among the blacks, who often sleep out in the open air, exposed to the refrigeration induced by upward radiation. The character of the moon has for ages suffered great disparagement, and her bad reputation might have continued for ages longer, but for the developments made by modern philosophers of the natural agencies through which the mischiefs wrongfully ascribed to her were produced.

The course of observations which we now bring to a conclusion, show how an acquaintance with a single law of nature, may, by following out its relations and effects, be made to shed new light upon obscure subjects, and lead to the correction of long-existing errors. They will also, we confidently believe, indicate causes often active in the production of sickness hitherto unsuspected by many. The principles which we have endeavoured to render as familiar as possible, will, we trust, produce an impression upon minds capable of making the proper applications of them as opportunities may present. The sanitary lesson which we have endeavoured to inculcate, is the great importance of guarding against exposure to the refrigerating effects of nocturnal radiation, especially in sickly places and during epidemic periods. The means of effecting this we have shown are extremely simple and always at hand, as anything will answer the purpose which may be interposed, so as to cut off the view of the sky, and thus prevent radiation.

GOUVERNEUR EMERSON,
ISAAC HAYS,
W. S. W. RUSCHENBERGER.

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ON THE

RESULTS OF SURGICAL OPERATIONS

IN

MALIGNANT DISEASES.

VOL. VI.—11

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ON THE RESULTS OF SURGICAL OPERATIONS IN MALIGNANT DISEASES.

PART I.—GENERAL OBSERVATIONS.

§ 1. NATURE, OBJECTS, AND DIFFICULTIES OF THE INQUIRY.

Two years have elapsed since the undersigned had the honour to be appointed by this Association Chairman of a Committee on the Results of Surgical Operations in Malignant Diseases. Soon after he was officially notified of his election, he addressed a circular to his professional brethren, both in the United States and in Canada, setting forth the objects upon which he desired information, and earnestly inviting their co-operation in the fulfilment of his arduous and responsible labours. In addition to this, he wrote nearly one hundred letters to the most prominent surgeons and physicians, entreating them to communicate to him the results of their experience upon a subject of so much interest and importance to the welfare of the human family and the character and dignity of our own profession. To the appeals thus made, the Chairman of the Committee, he regrets to say, received hardly a dozen responses, and not more than half that number embraced any matter calculated to impart real value to his report. The most important contributions that have reached him are from the pens of Dr. J. Mason Warren, of Boston, and Professor Hamilton, of Buffalo. Interesting communications have also been received from Professor Paul F. Eve, of Georgia, Dr. George A. Bethune, of Massachusetts, Professor Pope, of St. Louis, Dr. Tripler, of the United States Army, Dr. Green, of Easton, Pennsylvania, Dr. Norris and Dr. Littell, of Philadelphia, Professor Mussey, of Cincinnati, Dr. Alexander Barclay, of Newburg, New York, and Professor Palmer, of the University of Louisville.

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To my young friends, Dr. William H. Miller, Dr. Alexander Forsyth, and Dr. J. Rowan Pirtle, graduates of the University of Louisville, I am indebted for the tables which accompany the remarks on malignant diseases of the uterus, testis, and mammary gland. My former pupil, Dr. D. D. Thomson, of this city, has also furnished me with several interesting cases of carcinoma of the lip.

Soon after receiving my appointment, Professor Parker, of New York, and Dr. J. M. Warren, of Boston, kindly consented to serve on the Committee; but, owing to his arduous and incessant engagements, the former of these gentlemen has not, I regret to say, been able to afford me any assistance; the valuable contribution of the latter, already alluded to, will be found, without any alteration, in its appropriate place. For the rest of the report I alone am responsible.

It may be proper to observe here, that, in the prosecution of my labours, I have generally employed, as more convenient, and better suited to my purpose, the pronoun "I," instead of the more common phrase, "Chairman of the Committee."

Having failed to obtain from the profession of the United States a full and complete expression of their views upon the subjects set forth in his circular and in his private communications, the Chairman of the Committee, in order to give force and value to his labours, has deemed it his duty to seek information wherever he thought it could be found. For this purpose, he has appealed to the ancient fathers of the profession, and has brought himself as it were into close personal relation with the writers of his own times in Europe, in the hope of being able to extract some light that might serve to guide the practitioner of the present day in a class of affections that have been emphatically styled the *opprobria* of the surgeon and the physician. Like the emmet, he has endeavoured to extract whatever seemed useful, and to throw it into a heap for the benefit of the profession. In a question so momentous as the present, it is needful that the experience of ages should be brought forward, analyzed, and compared, that the wheat may be winnowed from the chaff, and the result exhibited in its true character.

Such a step must, indeed, be considered as indispensable when we remember how many of the works of these writers find their way into the United States, either in their original garb, or in the form of reissues. This remark is particularly applicable to British works, the great majority of which are reprinted in this country soon after they are published at home, and which, consequently, speedily reach the

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libraries of our physicians, by many of whom they become, in due time, to be regarded as authority, perhaps, of the highest order. But this is not all. Many of these works are used as text-books in our schools, and thus, falling into the hands of our pupils at an age when their opinions are easily moulded, create an impression which neither time nor circumstances can efface.

The opinions of the continental writers of Europe, especially of the French, are hardly less authoritative with the American practitioner; for, although their works are rarely translated into our language, yet their contents are sure to be made known to us, at the earliest possible moment, by our medical press, and by those valuable and widely circulated periodicals, *Braithwaite's Retrospect*, and *Ranking's Half-yearly Abstract*.

Our own country is lamentably deficient in native works on surgery; and there is reason to believe that the few treatises which have appeared upon the subject have exerted but little, if any, agency in influencing and shaping the professional mind of the United States. *The Elements of Surgery*, by the late Dr. John Syng Dorsey, of Philadelphia, have long been out of print, to say nothing of the fact that they were composed principally of materials derived from foreign authors, as Desault, Boyer, Cooper, and the Bells; and the *Institutes of Surgery*, by Dr. Gibson, the veteran Professor of Surgery in the University of Pennsylvania, although they have passed through numerous editions, have not, it seems to me, exerted that wide and controlling sway over American practitioners that might have been reasonably expected from the high position of their author. The minds of his own pupils, or, rather, those of the school with which he has been so long connected, have been mainly the recipients of their contents. Few of our colleges have recommended the *Institutes* as a text-book to their students; a fate which has been experienced by most of our native productions, and which must continue as long as foreign works are permitted to usurp the rights of our own. American physicians and surgeons seem to glory in being the slaves of European authority in whatever pertains to their profession, both in matters of opinion and practice; like Esau, they have bartered away their birthright for a mess of pottage; they are unwilling to learn anything that is purely American; they are too proud to acknowledge that anything good can emanate from a native author; and too stubborn to admit that one man knows more than another. They would rather at any time—lest they should seem to be indebted to their own countrymen—quote

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Louis and Chomel than Drake and Chapman, or Cooper and Brodie than Gibson and Mott. It is a bitter humiliation to find this tendency in our profession; a tendency so utterly at variance with the true spirit of patriotism, with our national pride, and with our national advancement as a scientific and literary people. It is mortifying to see that everything in the shape of a foreign book, however shallow and indifferent, is greedily sought and patronized to the neglect of our own productions.

It need hardly be said that the subject intrusted to the Committee is one of great perplexity. On every side, in fact, it presents difficulties which, in the existing state of the science, are absolutely insurmountable, notwithstanding the numerous attempts that have been made to remove them. The information collected in this report, if useful at all, is so rather negatively than positively. It shows what has been done rather than what has been achieved. It proves that this department of the healing art has been more "laboured than advanced." In short, it exhibits the humiliating evidence that our professional brethren, although they often wandered in quest of Truth, have hitherto failed to find her, and that the great subject, in point of fact, remains precisely where it was in the time of Hippocrates, whose wonderful sagacity induced him to declare that malignant diseases, by whatever name they may be known, or whatever organ they may attack, never forgive, but that they are always incurable by the knife and escharotics, and sooner or later destroy their victim.

Much of the perplexity here alluded to grows out of the difficulty which attends the diagnosis of malignant diseases and malignant tumours. Few men, whatever may be the amount of their science, experience, and tact, are able, at all times, or in all cases, to discriminate, with positive certainty, between these affections and those called non-malignant, benign, or innocuous; while the great majority find themselves utterly bewildered at every step of the inquiry. It is not difficult, therefore, to account for the errors which are constantly committed in practice, and to explain the reason why a tumour or disease which one surgeon regards as malignant should be considered as benign by another, and *vice versa*. The daily experience of medical men and the records of the profession abundantly verify the truth of these remarks. The mammary gland has been sacrificed a thousand times for diseases supposed at the time to be cancerous, but which, upon a more careful examination, proved to be nothing but "milk-knots," scrofulous swellings, or chronic abscesses. A tes-

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ticle, affected with sarcocele, or chronic enlargement, has frequently shared a similar fate. There is not a museum or private collection anywhere in the country that does not afford multiplied proofs of the truth of this statement.

M. Broca, in an able and elaborate article on the pathological anatomy of cancer, in the sixteenth volume of the *Mémoires de l'Académie Nationale de Médecine*, states that the late Dr. Blandin, surgeon of the Hôtel-Dieu of Paris, operated, in hospital and private practice, in 1847 and 1848, upon sixty-nine persons, for tumours that were neither fibrous nor adipous, and which, before the application of the microscope to the study of morbid structure, would have been pronounced cancerous. A careful examination, however, with that instrument, enabled Broca to determine their precise character. Thus, of the sixty-nine, two were fibro-colloid; five consisted exclusively of fibro-plastic tissue; fifteen others, of which fourteen were taken from the face and one from the prepuce, were epithelial; and eight, removed from the breast, were simple hypertrophies of that organ. Of the remaining thirty-nine patients whose tumours were really cancerous, eleven died from the immediate effects of the operation; and of the rest who survived, nineteen, whose subsequent history alone could be ascertained, had a relapse of the malady. M. Lebert, in his recent treatise on cancer, bears similar testimony in regard to these errors of diagnosis at the Hôtel-Dieu of Paris. M. Cruveilhier, in a paper read before the French Academy of Medicine, in 1844, broadly asserted that surgeons are continually cutting out fibrous tumours from the female breast, which never undergo the carcinomatous transformation, and which, therefore, might be allowed to remain with perfect safety.* "Diseases of all sorts," says Professor Müller, of Berlin, "have been extirpated for scirrhus and cancer."†

Now, if Blandin and his colleagues are so frequently mistaken, is it not reasonable to conclude that others are equally liable to err? The surgeons of the Hôtel-Dieu of Paris are usually men of the first order of talents, of great scientific attainment, and of abundant experience; and yet we have here the most undeniable proof that they are often unable to discriminate between malignant and simple tumours. It is only by bearing in mind these facts that we are able to account, satisfactorily, for the unparalleled success which

* Amer. Journ. Med. Sciences, vol. xiv. p. 230, 1847.

† On the Nature of Cancer, by West, p. 84, London, 1840.

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attended the practice of Hill, Nooth, Flajani, and a few other surgeons. Who can believe, for a moment, that these practitioners were good diagnosticians, or that many of their operations were not performed upon tumours entirely benign in their character? To come to any other conclusion respecting them would be to impugn the moral honesty of these men, or to assert that human nature and human disease were different then from what they are now.

Another source of difficulty is that the majority of the recorded cases of malignant diseases are deficient in necessary details; thus rendering it impossible to use them for statistical purposes. This remark applies both to our periodical literature, to our systematic treatises on surgery, and to our monographs on cancerous affections. As a proof of the truth of this remark, I may here state that one of the most interesting, and, in other respects, valuable works of the present day, the recent production of Dr. Bennett,* Professor of Medicine in the University of Edinburgh, hardly contains a solitary case available to our present inquiry. While it describes with great minuteness the histological characters of carcinomatous and cancroid diseases, as they appear in different tissues and organs of the body, it generally takes no other notice of the patient after operation than to say that he was discharged cured at such a time; meaning, of course, merely that his wound had healed, and not that he had been permanently cured of his cancer; a circumstance of which no operator can justly judge in so short a period.

To render such reports valuable and available to the interests of science, it is necessary that they should embody a full and connected history of the cases to which they relate; beginning with the earliest period of the disease, and tracing it through all its stages to its final termination in the patient's temporary freedom from relapse, his radical cure, or his death. To say that he has been discharged *cured*, when he has merely recovered from the effects of the extirpation, is absurd. Such a statement is of no benefit to the profession; for it throws no real light upon the final result of the operation. To enable us to do this, the patient should be diligently watched after the operation, with a view of determining whether he remains free from disease; or, if relapse occur, when and where it takes place, and what influence it exerts upon the parts and the system at large. Finally, there should, in all cases, be a careful examination of the morbid growth with the microscope, and a minute dissection

* On Cancerous and Cancroid Growths, Edinburgh, 1849.

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of the body in the event of a fatal termination. To report examples of cancerous affections without these details, may be calculated to throw light upon the site, progress, symptomatology, and other circumstances connected with their natural history; but they can afford no useful information in regard to the results of surgical operations undertaken for their relief or cure.

It is for these reasons that the whole subject of operation for malignant diseases must be investigated *de novo*, before we can hope to arrive at any satisfactory conclusions respecting it. The next quarter of a century will, if practitioners are true to themselves and to their profession, definitively settle this long agitated, important, and momentous question. Let every description of cases be subjected to the knife and to escharotics; let every circumstance, even of the most trifling character, regarding them, be faithfully recorded; let the morbid structure be carefully examined with the microscope; and finally, let the patient be diligently watched with a view to his ultimate fate; and a thousand such cases, brought together and analyzed, will forever put the matter at rest. All conjecture will then cease, and truth will stand forth, like a beacon light, to guide and direct the practitioner in the true path of science.

But are we to make no use of the facts that have been furnished by ancient and modern observers? Are we to discard, as useless and effete, all our so-called knowledge upon the subject of malignant diseases, a subject which has occupied the attention of many of the wisest and ablest minds from the dawn of medical science up to the present moment? Are the results of the operations of Monro, Desault, Earle, Cline, Cooper, Dupuytren, Roux, Scarpa, Dieffenbach, Liston, Langenbeck, Physick, Warren, Mott, Gibson, Mussey, and others, to be repudiated as unworthy of confidence and regard? By no means. The labours of physicians and surgeons have not been in vain; but that they have failed to settle the question intrusted to the Committee, it would be folly to deny. They have elicited important facts, but facts of a negative rather than of a positive character; facts which are but as an *ignis fatuus*, calculated to mislead and entrap the young, the inexperienced, and the unwary. All our knowledge upon the subject must be re-examined and submitted to the ordeal of a stricter logic, and a nicer sense of discrimination. The sight is to be aided by the magnifying-glass, and the hand by the most delicate scalpel and forceps. Patience and perseverance are to take the place of haste and indifference; surgeons must be pathological anatomists, and pathological anatomists

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must be microscopists. The cancer-cell must be found, recognized, and unravelled; mere external appearances must pass for nothing.

Let it not be supposed, from the above remarks, that I would encourage an indiscriminate resort to the knife. By no means. On the contrary, I would solemnly warn my professional brethren at large to abstain from operation altogether, and entreat them to unite in sending their cases to those men who, by their experience and opportunities, are fully competent to do justice to the subject, and who would thus contribute to furnish the light necessary to a full and final settlement of the question.

The duty of the Committee is to report upon the results of surgical operations in malignant diseases. What is a malignant disease? To this question, it is sufficiently easy to make a satisfactory reply. A malignant disease is one which, whatever may be its origin, situation, or structure, has a disposition, sooner or later, to destroy, not only the part, organ, or tissue which it occupies, but also the system at large, and, consequently, as a necessary corollary, the life of the patient. In other words, all its tendencies are to mischief and disorganization. It never forgives or relents; it never disappears spontaneously; and it is never cured by medication, not even by excision, except, perhaps, in very rare cases.

The mere fact that a disease returns after extirpation does not prove it to be malignant. The most benign tumour may, if it is not thoroughly exsected, repullulate, or sprout again, and, in due time, reacquire its original bulk. To be truly malignant, it must possess the properties which we have assigned to it in the preceding paragraph.

The affections which are usually arranged under this head are the different forms of carcinoma and melanosis. To these may be added certain forms of tubercular diseases, or rather, tubercular diseases of particular organs, as of the lungs and lymphatic ganglions. Certain cancroid affections, as they have recently been designated, particularly lupus, or noli me tangere, also belong to this class, and, therefore, require to be considered in this paper.

I am aware that tubercular disease is almost universally excluded from the catalogue of malignant affections, and that, in assigning to it such a position, I lay myself open to the charge of oddity and obstinacy, if not positive ignorance. But is this a fact? How does the question stand? I will not assert that tubercular disease is always malignant, though its tendency nearly always is to destroy the parts into which it is deposited, or which serve as its nidus; I

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wish merely to say, and I make the statement with all possible deliberation, that tubercular disease of certain organs, as, for example, the lungs, is as malignant, as unforgiving, and as incurable as any form of cancer of which we have any knowledge, or of which, in truth, we can form any conception. Nay, farther, it is not only malignant, but eminently malignant, destroying the tissues in which it occurs, and the life of the patient much sooner, on an average, than any form of carcinoma, excepting, possibly, encephaloid in its worst character. But it is different when tubercular disease shows itself in external and accessible parts of the body. Here, it is generally benign in its character, and amenable to treatment, at least comparatively speaking.

But, not to dwell upon this point, let us proceed to inquire more particularly into the practical part of our report; that is, into the nature of those diseases which, under the term malignant, as commonly received by the profession, usually form the subjects of operation. These diseases are scirrhus, encephaloid, colloid, and melanosis, of which the first three constitute the great and important group of cancers, carcinomatous diseases, or cancerous growths. Of the malignant character of these formations no one entertains the slightest doubt; for the experience of ages has amply established the fact in the observations of thousands, I had almost said, hundreds of thousands of physicians and surgeons.

All carcinomatous growths have a peculiar structure, of which a particular cell, now usually known as the cancer-cell, is the distinctive feature. This cell, for the discovery of which we are indebted to Lebert and other microscopic observers, has a definite form, size, and structure, and the faculty also of generating other cells, thereby enabling the disease to spread itself in such a manner as to form tumours, varying in their dimensions from a millet-seed to a man's head. These cells are microscopic in their character, and the malignity of the morbid growth is generally in proportion to their number and the perfection of their development.

In addition to the cancer-cell, there is, generally, especially in the more advanced stages of carcinoma, a peculiar milky juice, the presence of which is almost as characteristic of the disease as the cancer-cell itself. This fluid has long been recognized by pathologists, but the degree of importance to be attached to it, as a test of the cancerous or non-cancerous nature of morbid growths, has been chiefly insisted upon by Lebert, Walshe, and other recent observers.

The term *cancroid* has recently been applied to a class of affec-

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tions, which, as the name implies, bear a close resemblance to cancer, but differ from it in not containing any cancer-cells and cancer-juice. Although capable of contaminating the adjacent lymphatic ganglions, they are generally tardy in their progress, and comparatively limited in their localization; they do not, moreover, make any serious inroads upon the constitution, and they are also less liable to return after excision. Hence they have been often considered as semi-malignant in character. They have also, of late, been occasionally described under the name of *epithelial* cancers, from the identity of their microscopic appearance with epithelium; the cells in each being the same. The number and variety of this class of maladies are not accurately determined, but those which are usually comprehended under it are certain forms of cutaneous ulcers, as lupus, or noli me tangere, and cancer of the lip, tongue, penis, scrotum, and the neck of the uterus. A cancroid affection may, during its progress, become the seat of the true cancerous deposit, and thus assume the worst forms of malignant disease; rapidly contaminating the neighbouring lymphatic ganglions, impairing the general health, and returning after extirpation.

It was observed, long ago, that lupus—one of the varieties of cancroid—and cancer, although supposed to be of the same nature, were not equally amenable to treatment. The former, it was noticed, often disappeared under the influence of mercury and sarsaparilla, while the latter resisted every mode of management that could be devised for its eradication. For these reasons, Dr. Denman,* early in the present century, concluded that there is no identity between the causes of these affections; a conjecture, the truth of which has recently been fully verified by the researches of Lebert and other microscopists.

It is proper to observe that the ensuing remarks are intended to apply more particularly to the various forms of cancer; melanosis is of comparatively unfrequent occurrence, and is, therefore, mentioned only incidentally. The term "operation," has reference mainly to excision, as this has been performed from time immemorial, especially in malignant diseases of the breast, and is almost the only operation thought of by the modern surgeon. The actual cautery and the ligature have long since become obsolete as means for removing malignant tumours; and as to the various escharotics, as arsenic, the Vienna paste, chloride of zinc, and acid nitrate of mer-

* Observations on Cancer, p. 29. London, 1810.

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cury, formerly so much vaunted in the treatment of these complaints, their use seems to be confined chiefly to the hands of the empirics. One of the most able and scientific champions, of the present day, of this mode of destroying cancerous diseases, is Mons. Canquoin, of Paris, who has written an interesting and elaborate treatise to prove their superior efficacy. My young friend, Dr. J. Rowan Pirtle, has done me the favour to translate for me Canquoin's table, embracing twenty-nine cases treated by escharotics. By a reference to this table, under the head of Malignant Diseases of the Mammary Gland, it will be seen that nearly every instance is marked as successful. The reader will not fail to place his own estimate upon Mons. Canquoin's statements.

§ 2. ORIGIN OF MALIGNANT DISEASES.

The question has often been asked, is cancer a disease merely of the part in which it appears, or is it an affection of the constitution? As might be supposed, no uniform answer has been, or, perhaps, can be given to this question, although it has been agitated and discussed a thousand times by practitioners and medical philosophers. The subject is interesting, not merely in a pathological point of view, but eminently so in its practical relations; and hence it is a matter of deep regret that there should still be such a diversity of sentiment concerning it. Without going fully into the consideration of it, which my space will not allow, it may be safely assumed, nay, I think, it might be easily proved, that, while the malady, using the word in its genuine sense, is, generally speaking, of constitutional origin, it is also occasionally, though rarely, of a strictly local character, without any such antecedent or concomitant, as the term constitutional would lead one to suspect. What the relative frequency of this origin is, the present state of the science does not, unfortunately, permit us even to conjecture, much less to assert. My own belief is, and let it be understood that it is a mere belief, that cancer, in forty-nine cases out of fifty, if not in ninety-nine out of a hundred, is dependent for its production upon a peculiar state of the constitution, just as we find gout, rheumatism, scrofula, and some other affections, to be dependent for their production upon a disordered state of the general system. Nevertheless, who does not know that these complaints are occasionally of a purely local origin and character, unpreceded, and, for aught we can ascertain, unaccompanied by the slightest constitutional vice? In the same manner, cancer is sometimes a strictly local

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disease, or, as Hunter would have expressed it, a disease not in the part, but of the part; a disease not localized by the system, but primitively and essentially established by the action and management of the part itself, without any constitutional concurrence or antecedence; consequently, without any vitiation of any kind, either of the fluids or solids. The proof of this position is to be found in those cases in which cancerous tumours, when excised, do not return; or where, if they do reappear, repeated operations may be performed, and the patient survive a long time without any evidence of constitutional dyscrasy, or infection of the neighbouring tissues. Several examples, bearing directly upon this point, will be brought forward in the progress of this paper; and there are few surgeons, of enlarged experience, that have not had occasion to witness the occurrence in their own practice.

It has been alleged that cancer is, occasionally, a local disease, because it is capable of being produced under the influence of local injury. But such a conclusion is erroneous; at all events, it admits of doubt whether such a result ever follows such a cause, unless there is a previous predisposition, a readiness, as it were, on the part of the system, to generate cancer-cells; else, why is it that external violence, as a blow or bruise, so seldom gives rise to this horrible and unmanageable complaint? Why, in other words, is it that a blow will produce cancer in one man and not in another; or, still farther, why will one man suffer and a thousand escape?

The theory of the local origin of cancer, not as an occasional circumstance, but as a constant occurrence, has had many advocates, and is far from being exploded at the present day. One of its most strenuous and influential abettors, in his day, was Sir Everard Home, who wrote an able and interesting treatise on this disease nearly fifty years ago. In the work here referred to, he expresses the opinion that carcinoma is in every case, at one precise period, local, in the strictest meaning of the word.* A similar view had been entertained long previously. Home considers the fact as clearly established by the history of several of his own cases.

Home, also, supposed that "cancer is not a disease which immediately takes place in a healthy part of the body, but one for the production of which it is necessary that the part should have undergone some previous change connected with disease."†

The doctrine of the local origin of cancer, as a general fact, was

* Observations on Cancer, p. 147. London, 1805. † *Op. cit.* p. 147.

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strenuously insisted upon in a discussion which followed the reading of a paper on cancer, by M. Cruveilhier, before the French Academy of Medicine, in 1844.* On that occasion, it was contended by a number of the practical surgeons of Paris, as Bérard, Lisfranc, Blandin, Roux, and others, that this lesion is always first local, and that the constitution is affected only secondarily, by the absorption of the carcinomatous poison. Their mode of reasoning reminds one of that of the syphilographer, who declares, and that very correctly, that there can be no contamination of the system without previous inoculation. Dr. Bennett† believes that cancerous growths are for some time purely local. I am unable to ascertain to what extent the idea of the local origin of cancer prevails at the present day in France and other countries. My opinion, however, from all the information I have been able to obtain, is, that it is on the decline, and that most practitioners are disposed to espouse the opposite side of the question. This, I think, is certainly the case in England and in the United States. It need hardly be added that the doctrine involves important practical considerations. To these, allusion will be made in their proper place.

Another question of great practical importance connects itself with the one just discussed. This is, whether a tumour, of a non-cancerous character, may, in its progress, become cancerous? whether, in other words, such a growth is capable of undergoing what is called the carcinomatous degeneration? It seems to me that it is not difficult to give a correct answer to this question. If the term "degeneration" is restricted to its true and legitimate signification, then we must reply in the negative, for no tumour, whatever be its structure, can, by a mere conversion or transformation, pass into a malignant condition. Such a change can be effected only under the influence of a vital process, involving the development and actual existence of the cancer-cell; and I cannot, for my own part, see any good reason why a new growth, tumour, or deposit should be more exempt from such a disease than a primitive, original, or pre-existing tissue, whatever be its structure. Nay, indeed, may it not be supposed, that the more feebly a part is organized, the more prone will it be to take on such a process? That this change does occasionally occur, may be inferred from the circumstance that tumours, believed to be cancerous, but which cannot positively be proved to be of this

* Am. Journ. Med. Sciences, vol. xiv. p. 230, 1847.

† On Cancerous and Cancroid Growths, p. 232.

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description, remain, occasionally, in a state of latency for ten, fifteen, twenty, and even thirty years, and then, all of a sudden, manifest a malignant and destructive action, generally followed by the worst consequences. Is it possible to keep the cancer-cell in check for such a length of time? The occurrence is, perhaps, possible, but of its probability we are entirely ignorant. To admit more than this, would be to wander beyond the proper limits of observation and induction.

It would be of great practical utility if we knew the origin of cancer, or if we were acquainted with the causes, local and constitutional, under the influence of which malignant diseases, properly so called, are developed. But upon this subject, unfortunately, we are entirely ignorant, nor is it likely that our inquiries concerning it will ever lead to a satisfactory and philosophical solution of a question which has occupied the best minds in the profession from the most remote periods of medicine down to the present time. *All that we know, with any degree of certainty, is, that we know nothing.* Such a confession is humiliating enough, but it would savour alike of folly and of knavery, if, in order to conceal our chagrin, we were to attempt to substitute ignorance for fact, and specious reasoning for solid and substantial argument. The day for such a course is passed; the present age is an age of progress, and an age of realities, not of speculation and conjecture.

Several microscopical observers have shown that the cancer-cell occasionally exists in the general circulating mass. But what does this fact prove, granting it to be so? Does it prove that the cancer-cell is originally formed in this fluid; or is it not more reasonable to conclude, from our knowledge of the nature of the blood, and the office of the vessels in which it is contained, that it finds its way into it through the agency of the absorbents, whether we regard this function as being performed by the lymphatics alone, by the veins alone, or by the joint operation of both? If we admit the truth of the first of these conjectures, then it follows, as a matter of course, that all idea of excising cancerous tumours is to be abandoned, as utterly futile. If, on the other hand, we give our assent to the correctness of the other conclusion, the picture will certainly be of a more gratifying character, and hope will not be entirely banished from the mind of the operator. It seems to be pretty generally believed, that, when cancer has existed for any length of time in a particular part of the body, it has a tendency to contaminate the blood; and hence the doctrine, inculcated by so many writers and

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teachers, that the disease should be removed at the earliest possible moment after its formation, in order that such a contingency may, if possible, be averted.

§ 3. HEREDITARY NATURE OF MALIGNANT DISEASES.

Carcinoma is sometimes *hereditary*. The occurrence, however, is, I am satisfied, much less frequent than is generally supposed. Writers and practitioners, in speaking and thinking of this matter, seem to forget that there is a difference, and that a very wide one, between the transmissibility of this disease from the parent to the offspring, and its coexistence, or successive development, in different members of the same family. The latter occurrence, although also very infrequent, is much more common than the former, of which my own experience has supplied me with only a few examples. Lately, I saw a lady from Tennessee with a well-marked cancer of the mammary gland, whose mother and maternal aunt had died of the same disease. In the summer of 1850, I prescribed for an aged female with a cancer of the lip, whose mother had perished from cancer of the breast, and the father from cancer of the tongue. But the best and most instructive instance probably upon record is that related by Professor Warren, of Boston, in his work on Tumours. It occurred in a family in the neighbourhood of that city, and is given with circumstantial minuteness. A man died of cancer of the lip; his son had a similar disease in the breast, from which, after having undergone an operation at the age of sixty, he finally lost his life. Two of his sisters had cancer of the mammary gland; were operated upon; and ultimately died from a relapse of the malady. A daughter of one of the ladies had a cancer of the breast, which Dr. Warren removed at an early period; she recovered, but perished some years after from disease of the uterus. A daughter of the gentleman had a cancer of the breast, and there is reason to believe that other members of the family were affected by the same malady.

The paper of Dr. J. Mason Warren, accompanying this Report, presents a remarkable case, marked No. 15, of the hereditary predisposition to cancer developed in early life under the influence of external injury. In another case, still more remarkable, a man who died of cancer of the penis, lost his father, grandfather, and great-grandfather from the same disease.

More frequently, as has been already stated, the disease occurs, either simultaneously, or successively, in several members of the same

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family. My own practice has afforded me a number of instances of the kind, and there is hardly a writer on carcinoma that does not narrate examples of it. In one remarkable case in this city, four out of six members of one family have died of the disease; one from cancer of the uterus; another from cancer of the side and mammary gland; a third from malignant polypus of the nose; and the fourth from carcinoma of the thoracic viscera. Professor Gibson* gives an instance of cancer of the breast in four sisters. The two eldest were operated on in North Carolina, and both died from a return of the malady. In the third case, the excision was performed by Dr. Gibson, and there was no relapse ten years after. In the fourth case, the operation was performed by this gentleman in 1835, with every prospect of success, but the final result has not transpired. Bayle gives a number of similar examples. Thus, of a family composed of five members, one had cancer in the breast, one in the face, and one in the stomach. In another family, the father died of cancer of the tongue, and a son of noli me tangere of the face. In a third, the malady existed in three members, respectively, in the womb, the breast, and the neck. Finally, in a fourth family, consisting of seven members, one died of cancer of the bladder, another of cancer of the breast, and a third of cancer of the brain.† Examples of a similar character are narrated by Sir Astley Cooper‡ and other writers. I shall have occasion to revert to this subject by and by.

§ 4. LATENCY OF MALIGNANT DISEASES.

Cancer is sometimes a latent disease; that is, after it has reached a certain point of development, it remains in a state of quiescence, apparently neither advancing nor receding. The period of dormancy varies, in different cases and under different circumstances, from several months to several years. In the case of an elderly lady, the wife of a Baptist clergyman of this State, and the mother of one child, the malady has continued stationary for upwards of twenty years; the tumour occupies the mammary gland, and possesses all the external characters of scirrhus, being exceedingly hard, and the seat of occasional sharp, lancinating pains. Many years ago, this lady consulted Professor Dudley, of Lexington, who unhesitatingly pronounced the disease cancerous, but advised against

* Institutes of Surgery, vol. i. p. 168, 1845.

† Dict. des Sciences Méd. art. Cancer, t. 3, 677. Paris, 1812.

‡ Lectures on Surgery, by Tyrrell, p. 263. Philadelphia, 1835.

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an operation. The general health has usually been good, and the local suffering has been all along very slight. She has recently passed the critical period, and still the tumour remains indolent.

Sir Benjamin Brodie* saw a case where the cancer remained quiescent for twenty-five years, and where the woman died at last, not from disease of the breast, but from effusion into the cavity of the chest. Dr. Babington, of London, knew an instance in which a schirrous tumour of the breast was stationary for twenty-four years; and Sir Astley Cooper† attended two females, in whom the period of latency of this affection amounted, respectively, to seventeen and twenty-two years. Mons. Leroy, in his *Memoir on Cancer*, read before the Royal Academy of Sciences of Paris, in 1843, refers to the case of a *réligieuse*, who laboured under scirrhus of the mamma for twenty-five years, and who at the end of that time was able to perform all her duties in the hospital. Mr. South‡ alludes to a similar case. Dr. Macfarlane, of Scotland,§ has seen instances of ten, fifteen, and twenty years of this kind of latency. Indeed, there is reason to believe that the occurrence is more frequent a good deal than is generally supposed. The fact is not without interest in its practical relations.

§ 5. CIRCUMSTANCES CONTRAINDICATING SURGICAL INTERFERENCE.

The following circumstances are generally enumerated by writers and teachers as contraindicating the removal of malignant tumours. A reference to the opinions of American, English, French, Dutch, and Italian authors, cited in this Report, will show the remarkable coincidence of views upon most of the topics comprised under this head.

No operation should be performed when the disease is congenital, or when it manifests itself soon after birth. Under such circumstances, a resort to the knife is almost certain to be followed by relapse, and that, too, in a very short time, owing to the fact, probably, that the system is saturated, as it were, with the cancerous poison. Cases of this kind are peculiarly virulent and intractable, resisting all attempts at cure, or frequently even at palliation, and rapidly tending to fatal termination. The occurrence of the disease

* Select Surgical Works, p. 222, Phila. 1847.
† Lectures on Surgery, by Tyrrell, p. 268, Phila. 1835.
‡ Chelius's Surgery, vol. iii. p. 509, Phila. 1847.
§ Amer. Journ. Med. Sciences, vol. xxiii. p. 221.

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in several members of the same family may also be regarded as contraindicating ablation, inasmuch as it is denotive of a constitutional proclivity to malignant action.

Interference should be avoided when the disease exists in several parts of the body, as, for instance, when it affects the mamma and the uterus, or the testicle and the eye. Although all these organs are accessible to the knife, yet a resort to it under such circumstances would be highly injudicious, inasmuch as it cannot possibly eventuate in any permanent good, but, on the contrary, be almost sure to hasten the patient's destruction. No surgeon, however reckless, would think of operating when the external disease is associated with carcinoma of an internal part. The coexistence of cancers in different organs or tissues, generally denotes a bad state of the system, prohibiting surgical interference.

Operation is never resorted to, at least not as a curative agent, when the malady has made great and rapid progress, or when it has broken through its original confines, and invaded the adjacent tissues. Thus, in cancer of the mamma, no surgeon who values his reputation, or has any regard for the welfare of his patient, thinks of interfering where there is great bulk of the tumour, or firm adhesion of the breast to the surrounding parts; where the skin is changed in structure, ulcerated, indurated, or dimpled; where there is retraction of the nipple, or enlargement of the axillary, subclavicular, or sternal lymphatic ganglions; where there is œdema, with numbness and loss of function in the corresponding limb; and finally, where, in addition to some of the symptoms just mentioned, the features exhibit all the evidences of a cancerous cachexy. The same circumstances guide the surgeon in carcinoma of the testicle, of the eye, lip, penis, and extremities. If the knife is ever employed when the malady has made such progress and such inroads, it is with a view solely to palliation, not to cure. Of the propriety of such a course, every surgeon must be his own judge.

When the disease advances very rapidly, as it not unfrequently does in encephaloid, breaking through its original boundaries, and leaping, as it were, suddenly into the surrounding tissues, it may be assumed, as a general rule, that ablation will be improper; or that, if had recourse to, a rapid repullulation will be the consequence. Rapid growth, constituting what some have denominated the acute form of malignant action, always implies a bad state of the constitution, and imperatively forbids surgical interference. There is another symptom which is equally portentous, but which has not, I think, en-

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gaged sufficient attention. I allude to the œdematous appearance of the parts immediately around the morbid deposit, or at a distance more or less remote from it. This condition, which is seldom absent in external carcinoma in its latter stages, is not unfrequently present, at an early period, in encephaloid and the hæmatoid variety of this affection, and always denotes the very worst state of things, both local and constitutional. The immediate cause of this symptom is obstruction of the lymphatic ganglions and vessels. My observation has taught me that nothing but mischief is to be expected from interference when the malady has attained this crisis.

I find that many of the authorities which I have examined, in the progress of my labours as Chairman of this Committee, speak discouragingly of ablation when the malady has supervened suddenly upon suppression of the menses prior to the usual period for the stoppage of this discharge. Even great irregularity of this function is considered by some as a sufficient reason for declining surgical interference. Flajani lays much stress upon these circumstances, especially the former; and Bayle and Cayol stated long ago that the period of the cessation of the menses and the age of confirmed virility are those in which the cancerous diathesis is apt to exist in its greatest intensity.* Neither my own experience, nor that of my friends, furnishes me with any facts illustrative of the truth or falsity of this opinion.

A quickened state of the pulse, occasioned by the local irritation, augurs unfavourably. Excision, performed under such circumstances, is nearly always followed, it is said, by speedy relapse; and it is, therefore, the duty of the surgeon to discountenance it. This point is particularly dwelled upon by several of the older writers, but I find no authority for it among our contemporaries and immediate predecessors.

Temperament is supposed to exercise some influence upon the result of excision, and may therefore be considered, perhaps, as contraindicating the operation. Dr. Macfarlane, of Glasgow, observed that the disease was more apt to reappear and to prove disastrous after extirpation in robust females of sanguine temperament than in nervous or lymphatic persons; and Sir Benjamin Brodie has found that hysterical women are especially unfit subjects for operation. My impression, however, is, in respect to the latter writer, that his remark has reference not so much to the greater liability of

* Dict. des Sciences Médicales, art. Cancer, t. 8, p. 673, Paris, 1812.

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this class of females to suffer from relapse as to their greater liability to suffer from the immediate effects of the knife. I think it is generally conceded that fat, nervous, and hysterical persons are very apt to perish from all operations attended with much shock or loss of blood, neither of which they are able to bear well.

Latent cancers should not be interfered with. Cases have occurred again and again, in which, from neglect of this precaution, the patient has lost his life, within a very short period after operation, from a return of the disease in its worst form. Dr. Macfarlane* refers to instances where persons, after having laboured under malignant disease for ten, fifteen, and twenty years, with hardly any suffering, were cut off in a few months by an operation. In such cases, the reproductive powers of the part, if not of the system generally, manifest an astonishing activity, and the consequence is that the malady soon accomplishes its work of destruction.

It is not necessary here to insist upon the propriety of refraining from operation when there is serious disease of an important internal organ. Such a complication could hardly fail to predispose to relapse, if not to the speedy destruction of the patient. Especially is it important that there be no organic affection of the heart and kidneys, the due performance of whose functions is so essential to the maintenance of a healthy circulation and the efficient and thorough depuration of the blood. The absolute necessity of avoiding capital operations, under such circumstances, has long been acknowledged by scientific men, but has not been sufficiently insisted upon in cases of malignant disease.

Another objection that has been occasionally alleged against this operation, is the danger which sometimes attends its performance. This danger, which may be either immediate or secondary, is unquestionably real, though there is no doubt that it has been much exaggerated. My own practice has furnished me with two cases of this sad occurrence; in both, the females were unusually fat; they lost each a considerable quantity of blood, perhaps a pint, during the operation, and in both, death was caused by erysipelas; in one, on the sixth, and in the other on the eighth day. Dr. Macfarlane,† of Glasgow, lost two patients out of thirty-two operated on for the cure of this affection; one died from erysipelas, and the other from

* American Journal of Medical Sciences, vol. xxiii. p. 221; also, Walshe on Cancer, 238, London, 1846.

† Amer. Journ. Med. Sciences, vol. xxiii. p. 221.

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pleurisy. Sir Astley Cooper* met with five deaths from this cause, in a practice extending through many years, and involving a large number of cases. Two of his patients perished from erysipelas, one from pleuro-pneumonia, one from the immense size of the wound, and one from advanced age. Mr. South† gives an instance which proved fatal from hemorrhage, almost before the completion of the excision; the breast was of great bulk, and the blood gushed out in torrents, so that a large quantity was lost before it could be controlled by the operator and his assistants. The late Mr. Abernethy‡ states that he has known a patient to die soon after an operation for the removal of a carcinomatous tumour of no great magnitude, merely in consequence of the shock imparted to the system. He has seen other cases, where the diseased state of the wounded parts seemed to have been the chief cause of the speedy death of the individual. Dr. Benedict,§ of Breslau, mentions two cases which proved fatal from exhaustion, out of ninety-eight operated on for carcinoma of the mammary gland. Sir B. C. Brodie states that he has lost patients from the immediate effects of the operation, and every surgeon has had the same misfortune. Not a few examples might be cited from the older writers, where death followed the extirpation of the mammary gland for the relief of this class of affections; but as this would subserve no useful purpose, I shall omit them.

Of thirty-nine persons operated on, for cancerous disease, at the Hôtel-Dieu, and in private practice, by the late M. Blandin, of Paris, seven died from the immediate effects of the excision.‖ In the Massachusetts General Hospital, at Boston, of seventy-five cases of cancer of the mamma five perished from the operation; two from erysipelas, and three from internal disease which existed at the time of the excision, and should have prevented its performance.¶

Thus it will be seen that excision itself is not often followed, except, perhaps, in Paris, by fatal results; not, indeed, nearly as often as some other capital operations, such, for example, as amputation, lithotomy, and the ligation of arteries. I am, therefore, induced to believe that it should not, in the slightest degree, enter into the con-

* Lectures on Surgery, by Tyrrell, p. 278, Phila. 1835.
† Chelius's Surgery, vol. iii. p. 541, Phila. 1847.
‡ Surgical Works, vol. ii. p. 125, Amer. edition.
§ Rust's Magazine, No. ii. vol. xliv.
‖ Broca, Mém. de l'Académie de Médecine, t. 16, p. 752, Paris, 1852.
¶ Dr. Parkman, Amer. Journ. Med. Sciences, vol. 16, p. 305; 1848.

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sideration of the case; or, in other words, that it should never of itself prevent the surgeon from using the knife. It is well-known that, in certain cases and in certain circumstances, the most trifling operation is sometimes followed by the speedy death of the patient, thus humbling the pride of the surgeon and deriding his prognosis As a general rule, it may be stated, I think, that fat, corpulent, and indolent women, with large breasts, are more apt to suffer in this way than any other class of persons. They are certainly ill able to bear the loss of blood and the shock of the operation; and they are also, according to my observation, very prone to attacks of erysipelas, often of a severe and obstinate character. With proper attention, however, during the operation, and a proper preparation of the system, the surgeon will rarely be obliged to witness the unfortunate result in question.

§ 6. REPRODUCTIVE TENDENCY OF MALIGNANT DISEASES AFTER OPERATION.

Of the reproductive tendency of carcinomatous diseases, after extirpation, or destruction by the actual or potential cautery, writers have made mention from the earliest periods of medical science to the present time. Hippocrates was fully aware of the fact, and entered his protest against all operative proceedings, under the conviction that, however early, or well executed, they could not possibly afford any permanent relief, or guard the patient against a return of his malady. Similar views have been advanced by nearly all succeeding writers. If a different sentiment has occasionally been expressed, as has happened in a few instances, it has been by men who have had a very imperfect knowledge of the disease, who have been poor observers, or who have wilfully concealed the truth, from interested and dishonest motives.

The period at which relapse occurs varies from a few weeks to a number of years. On an average, it may be stated to be from four to six months. Occasionally, it takes place within an almost incredibly short period. In one of my cases the malady returned in less than three weeks. The original disease was of eight months' standing, and was seated in the left mammary gland, which it involved nearly in its whole extent; the nipple was somewhat retracted, and there was a slight enlargement of one of the lymphatic ganglions, which was removed in the operation. The dissection was performed with much care, and every particle of the morbid structure was

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apparently cut away; the greater portion of the wound united by the first intention, but a part at the centre remained open, and became the starting-point of the new growth. The woman, who was about forty-six years of age, died three months after the operation, after having endured the most horrible torments.

Sometimes a relapse does not take place until the end of the first year; and in a few instances it is postponed to a later period, as the expiration of the second, third, and even fourth year. Broca states that about two-thirds of the relapses appear during the first six months after the operation, and at least three-fourths before the end of the first year.* I am not able to say how far this assertion is borne out by the experience of other surgeons; but it does seem to me to be more favourable than the results of the profession generally upon this subject would warrant.

All malignant diseases possess this tendency to relapse after ablation, but not in an equal degree. Encephaloid undoubtedly enjoys it to a far greater extent than scirrhus, and scirrhus much more than colloid. Melanosis, the black cancer of Alibert and Dupuytren, also relapses with great frequency and promptness, and may be placed as next to encephaloid in this respect. Again, it must be borne in mind that a genuine cancer is more certain to return than a cancroid affection, as well as more apt to prove rapidly fatal.

The reproductive tendency of malignant disease, especially of that form of it known under the name of encephaloid, after operation, may be illustrated by a reference to cases, of which I select the following as examples in which the fairest trial was given to this mode of treatment without any ultimate advantage.

The first case which I shall relate is mentioned by the late Mr. Allan,† of Edinburgh, and is one of the most remarkable on record. The patient, in 1805, at the age of twenty-three, observed upon his left hip a movable and elastic tumour, not larger than a hazel-nut. About five years afterwards, when it had attained the bulk of a child's head, it was removed by Mr. Newbigging; the wound healed kindly; and the man was apparently well. At the end of nine months, however, it began again to grow, and in seventeen months from the time of the first operation, it was cut out by Mr. Russell, the tumour being as big as two fists. The patient remained seemingly well for nine months, when the morbid growth returned, and

* Mém. de l'Académie Nationale de Méd. t. 16, p. 742.

† Pathological and Operative Surgery, vol. i. p. 264. Edinb. 1821.

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gradually increased until it acquired the size of a very large mamma; the skin was much inflamed, and a part of the tumour was ulcerated, protruding a dark-coloured sphacelating substance. Mr. Allan now removed it, along with the skin, leaving a raw surface, of the diameter of the crown of a hat, which was completely cicatrized in a month. The disease was now apparently eradicated, for the man enjoyed good health, and resumed his work. In seven months, however, it again returned, and growing more rapidly than at any former period, it attained in two months nearly the same volume as when last extirpated. Mr. John Bell was next consulted, and removed the tumour, now as big as the head of a child of eight years, along with a great portion of its integuments. The wound healed kindly, as before, and when it was reduced down to the size of a crown-piece, it was kept open as an issue, to invite a free discharge of matter. The health continued good till March, 1815, when the tumour showed itself a fifth time. In December, it had acquired an extraordinary volume, and was the seat of a large fungus, resembling a cauliflower in appearance. This, Mr. Allan destroyed by ligature in June, 1816, with partial recovery of the general health. A year subsequently, the tumour was of enormous dimensions, measuring three feet in circumference at the base; the discharge was very profuse, and the general health became so much impaired that the man died exhausted in January, 1818, thirteen years after the commencement of the malady, and nearly eight years from the time of the first operation. All the viscera were found to be perfectly sound, except the liver, the left lobe of which contained a few hard tubercles, about the size of barley-corns.

The next case has been furnished by M. Broca,* and is, perhaps, still more extraordinary. It came under the observation of the late Professor Blandin, the eminent Parisian surgeon. In this case, the patient had an encephaloid tumour, of the size of a turkey's egg, on the left side of the trachea, which, rapidly augmenting, threatened to destroy him in a few months. In 1844, when on the point of ulcerating, it was removed. The wound from the operation speedily healed. In five months, the disease returned, and a second operation was performed, in August, 1844. Five months passed without relapse, when the tumour reappeared, and the knife was again used in March, 1845. The man now remained well for a whole year,

* Anatomie Pathol. du Cancer, Mém. de l'Acad. Nationale de Médecine, t. 16, p. 756.

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when, rearing its head again, the morbid growth was extirpated in April, 1846. At the end of ten months it reappeared, leading to the necessity of a fifth operation, which was performed on the 23d of March, 1847. The parts were well in a few days; and for nine months the patient enjoyed an immunity from his tormentor. The sixth operation was performed on the 2d of February, 1849; but this time the wound united less promptly, and the cicatrization was not completed till the 23d of the month. In five months, the malady reappeared; the man came again to Paris, but finding that Blandin was dead, and being unwilling to intrust his case to any other surgeon, he went home to submit himself to his inevitable fate. He lingered till the autumn of 1850; the tumour, in the mean time, forming an enormous mass, extending from the parotid gland to the clavicle. The signs of constitutional infection existed in the highest degree.

It will be perceived from the history of this case, that, counting the intervals between the relapses and the operations of which the poor patient was the victim, he enjoyed nearly four years of health. This period might, doubtless, have been augmented, had the man not abandoned himself to despair upon finding that his surgeon was no longer alive. "Is this case not sufficient," asks M. Broca,* "in the existing state of the science, and in the absence of any specific remedies that may hereafter be discovered, to show that an operation for the removal of cancer ought always to be performed whenever it is possible to satisfy ourselves that there is no constitutional contamination?"

The following case, related by M. Jobert,† of Paris, illustrates this tendency to relapse when the cancer is situated near the eye. The patient, when first seen by this distinguished surgeon, in October, 1849, was forty-two years of age. Her health had been quite good till 1834, when she observed a small tumour under the right superciliary ridge, which, as it was the seat of severe pain, was removed in August of that year, when only the size of a grape-seed. The wound promptly cicatrized. Between November, 1834, and April, 1835, a similar tumour was developed at the same spot, and was excised along with the lachrymal gland by Cloquet. The wound was healed in a month, and for two years the parts remained well.

* *Op. cit.* p. 757.

† Gazette des Hôpitaux, 1849, No. 143; also Brit. and Foreign Medico-Chir. Rev. April, 1850, p. 545.

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At the end of this period, while the woman was apparently in perfect health, three little tumours were observed at the old cicatrice, and gradually involved the surrounding structures. She bore her sufferings for three years, when, in 1842, extirpation was performed by Lisfranc. The wound healed in twenty-six days. She continued quite well for one year, when another little swelling appeared just above the lachrymal caruncle, which was afterwards removed by a young surgeon. After a respite of two years, the malady recurred at the cicatrice, and finally involved the eye. Having borne her sufferings for four years, she at length, in April, 1849, fell into the hands of Jobert, who extirpated the entire eye. Cicatrization readily occurred, but in three months the disease showed itself again, not at the original site, but in the orbitar surface of the inferior lid. This, too, was extirpated in November, 1849, and the parts perfectly healed.

It will thus be seen that the poor woman, in this case, underwent not less than six operations; and that no effort was spared, as far as the knife is concerned, to relieve her from her persecuting malady. If, as the reporter observes, the resources of art were not as complete as might be desired, it cannot be doubted that they have secured to the patient a prolongation of her life, and several years of tolerable comfort. It is to be regretted that no mention is made as to the fact, whether these tumours were epithelial or cancerous. It is hardly presumable that they were of the latter character, inasmuch as the intervals between the relapses were, with the exception of one, unusually long, and inasmuch as the general health remained throughout unimpaired. It is equally to be regretted that we have no knowledge of the final result of the case.

The next case which I shall mention occurred in my own practice. The results have been already made known, in part, in the *Western Journal of Medicine and Surgery* for 1852. A man, aged 32, consulted me in April, 1851, for a tumour of the lower jaw, which he had first noticed three months ago; it was firm, elastic, free from pain, of a pale-red color, and attached to the gum and jaw, extending from the ramus to the first bicuspid tooth. Two operations had been already performed upon it, each being followed by rapid relapse. On the 27th of April I removed the parts, along with the corresponding portion of the jaw; and, early in September, he wrote me that the disease had returned. On the 24th of that month I operated upon him a second time, removing the whole of the new growth, which was about the size of a pullet's egg, and about three-quarters

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of an inch of the anterior extremity of the ramus of the bone, from which the diseased structure seemed to spring. On the 31st of August, 1852, I excised the ramus at the articulation, the disease having attacked its inferior extremity. The man remained well until the winter of 1853, when the disease broke out in front of the ear, and now forms a tumour of the size of a small fist. It is worthy of remark that the general health has been all along pretty good, and that the wound has always healed well after each operation. A farther account of this case will appear under the head of Cancer of the Jaw.

A most extraordinary instance of this tendency in cancer to relapse after excision, is mentioned by Professor Sidillot, of Strasburg.* It occurred in a woman, aged thirty-five, who had a cancer of the knee, which had been extirpated nine times. It was removed for the tenth time on the 15th of July, and, again returning about six weeks after, the thigh was amputated on the 6th of November. The limb remained sound, but the patient died, a year after the last operation, of cancer of the lungs.†

The subjoined case, sent me by Dr. Alexander Barclay, of Newburg, Orange County, New York, is one of the most remarkable on record.

John Nolty, aged 48, shoemaker, of a sanguineo-nervous temperament, applied to Dr. Barclay on the 23d of March, 1844, on account of a cancerous ulcer, involving two-thirds of the lower lip; its edges being everted, and its surface hard and irregular, accompanied with violent, lancinating pains. The disease had been first noticed about three years ago as a wart, the origin of which was ascribed to the use of a tobacco-pipe. There was no enlargement of the ganglions in the neck or under the chin. The parts were removed in the usual manner, and the wound healed by the first intention.

The man applied again to Dr. Barclay early in December, 1845, on account of an immovable bony tumour of the lower jaw, extending from near the symphysis to the angle. It seemed to involve only the outer plate of the bone, was exquisitely painful, and was accompanied by severe inflammation of the soft parts. It had commenced about six months previously, and had grown rapidly up to the present time. The whole of the morbid mass was now removed with the saw, chisel, and mallet, the operator leaving the internal plate and alveo-

* Recherches sur le Cancer, Obs. 30, p. 99.

† Bennett on Cancerous and Cancroid Growths, p. 260.

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lar process of the bone, as they appeared to be sound. The diseased soft structures were also excised; the hemorrhage was excessive; and, in about two weeks, the wound was completely cicatrized.

On the 26th of May, the patient again presented himself for the purpose of an operation, which was performed on the following day, and which consisted in the removal of the piece of bone left at the previous operation in December, 1845.

In eighteen months the disease reappeared in the remaining portion of the jaw-bone. Dr. Barclay declining all further surgical interference, the man consulted Dr. Blackman, of Newburg, who exsected the portion of the bone containing the rest of the molar teeth. The disease, again showing itself, was now removed by Professor Parker, of New York, the exsection embracing a portion of the ascending ramus of the jaw.

Twelve months after this the disease returned, and the man again solicited Dr. Barclay to perform an operation, which, however, he declined. The tumour increased with great rapidity, pressing inwards upon the tongue, so as to impede respiration and deglutition. He struggled hard for life, but became much emaciated, and died in June, 1851.

In this case not less than five separate operations were performed; that they had the effect of prolonging the patient's life, no one can doubt. Death took place about ten years after the first appearance of the malady, and a little upwards of seven years after the first operation.

I am indebted to Dr. Charles S. Tripler, U. S. Army, for the following history of a case of encephaloid of the thigh, remarkable for the length of time which elapsed between the operation and the patient's death.

An officer of the army, 22 years of age, of nervous temperament, and accustomed to much exercise on horseback, noticed, in 1841, a small tumour, not larger than a pea, at the upper and inner part of the thigh. In the spring of 1843, when first seen by Dr. Tripler, it was of the volume of a small hen's egg, somewhat painful, and steadily, though slowly, increasing in bulk. In June, 1844, when this gentleman again saw the case, the local symptoms were, in every respect, worse; and, although the general health was apparently good, yet there was some evidence of the cancerous cachexy. The tumour was now removed by the knife, and was found to weigh five ounces and a half; its structure being, to all appearance, encephaloid. It was situated beneath the femoral aponeurosis, upon the

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adductor muscle, and was partially overlapped by the gracilis and sartorius; portions of which, as they were implicated in the morbid mass, were necessarily excised. The wound did well, and the man soon rejoined his company. In September, 1845, while stationed at Corpus Christi, in Texas, he suffered with dysentery, which induced him to visit Pennsylvania, and quit the army. Recovering from this affection, he laboured for some time under diarrhœa, though, in other respects, his health was good. Late in the spring of 1847, the tumour reappeared, and increasing slowly, attained, by the ensuing autumn, the volume of a hen's egg. In the following summer, when it greatly augmented in bulk, extending eight inches down the thigh, and causing constant pain, he consulted the late Dr. George McClellan, of Philadelphia, who refused to operate upon him; assigning, as a reason, that he would not be able to withstand the shock and loss of blood attendant upon the undertaking.

From this time on the patient gradually sank, and died in June, 1849, five years after the extirpation of the original tumour. The secondary growth at this time, extended half way round the thigh, and was of great size, weighing, as was supposed, about eight pounds.

The following facts are added in further illustration of this interesting subject.

A female, from whom Sabatier removed a cancerous tumour of the breast, of large size, enjoyed excellent health for ten years, when she experienced a relapse. A second operation was performed, and the malady had not returned when the case was reported, some considerable time after. An officer, treated by the same distinguished surgeon, also for a carcinomatous affection of the mamma, had undergone three operations, and was in good health when the facts were published.* Lacombe, in his work on cancer,† narrates, with much minuteness, the case of a mantua-maker, aged 49, who was operated upon four times for a similar disease, and in whom no relapse had taken place at the end of five years. In a discussion on this subject, some years ago, before the Academy of Medicine of Paris, Professor Roux referred to the case of a man upon whom the celebrated Boyer had performed five successive operations for a carcinomatous tumour of the left shoulder, and in whom there was no recurrence of the morbid action three years after the last excision. He was also cognizant of the case of a lady, in whom there was no trace of cancer

* Médecine Operatoire, t. iii. p. 865. Paris, 1824.

† Proposition sur le Cancer, Paris, an. xiii.

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after six operations, performed, in the space of three years, by himself and another surgeon, for a malignant tumour of the mammary region.*

§ 7. GENERAL RULES RESPECTING THE MANNER OF CONDUCTING EXCISION OF MALIGNANT DISEASES.

When excision is determined upon, it is a matter of paramount importance that it should be performed in the most thorough and complete manner, in order that the parts may be effectually guarded against relapse. The slightest atom of the new tissue, the most minute cancer-cell, nay, possibly, the smallest particle of cancer-juice, may, if left behind, endanger a reproduction of the malady. Upon this subject there is such a universal agreement of opinion, that it would be folly to cite any authority in its support. The practice is sanctioned alike by the dictates of common sense and the results of general experience.

It does not fall within the province of this paper to give an account of the methods by which malignant tumours should be removed; but there are a few points to which I deem it my duty to advert, in a special manner, in connection with the subject, inasmuch as attention to them must exert an important influence upon the great question which I have been examining.

The first of these points, and one, as just stated, of paramount consequence, is, that every particle of the heterologous substance should be removed along with the tumour. To accomplish this important object, it is necessary that our incisions should always be carried through the healthy tissues at some distance from the morbid deposit. Should any part have escaped the knife in the first instance, it should be traced out immediately after the extirpation of the main mass, and be excised with the most scrupulous exactness. Free use should be made, in this stage of the operation, of the sponge and the finger; of the former, for the purpose of clearing away the blood, and of the latter, for the purpose of ascertaining the consistence of the parts which constitute the surface of the wound. The sight alone should never be trusted in a case of this kind, for appearances are much more deceptive than the sense of touch. Not a particle of substance that is in the least suspicious should be left behind. Skin,

* Bulletin de l'Académie de Médecine, Mars 26, 1844; also Compendium de Chirurgie Pratique, par Bérard and Denonvilliers, t. i. p. 695. Paris, 1840.

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muscles, glands, vessels, nerves, and bones should all be sacrificed, if necessary to the success of the operation. Nay, the very atmosphere of the disease should be destroyed; and with this view it would be well to adopt the practice of the celebrated Scarpa, of removing an amount of healthy substance equal or nearly equal to the abnormal.

Secondly. The operator should always endeavour to preserve as much of the common integuments as possible, in order to afford a complete covering to the surface of the wound. This rule is one of great importance, and should never be departed from. Another precept, of nearly equal consequence, but one which has not hitherto been sufficiently insisted upon, is to preserve as large a quantity as practicable of the subcutaneous cellular tissue, with a view of maintaining, unimpaired, the cutaneous circulation. Whenever this is much interrupted, as it necessarily must be by a very close dissection, there is additional danger of a speedy return of the abnormal action.

Thirdly. When only a portion of an organ is involved by the heteromorphous matter, the rule is to remove, not a part, but the whole of it. Thus, in cancer of the mammary gland, the practice invariably is to extirpate the entire organ, no matter how small a portion may be implicated in the disease. Upon this point surgeons have long since been agreed. When the disease is seated in an extremity, especially the distal portion, the proper operation is amputation, not excision.

Fourthly. In removing a malignant tumour we should always endeavour to avoid the loss of blood. This is a good rule, I conceive, even when the patient is tolerably plethoric; but its observance is especially important in lean and fat subjects, the latter of whom, in particular, generally bear the loss of this fluid very badly. I deem it a matter of great moment to guard against hemorrhage in every operation of this kind, not so much on account of the immediate recovery of the patient as on account of the danger of relapse, which, I feel constrained to believe, is frequently very much increased by this accident.

Fifthly, and lastly. It is a matter of great consequence, in reference to the question of relapse, that the whole of the wound left by the operation should be healed by the first intention. For this purpose, the parts should always be approximated as nicely as possible, not only at their edges, but also over the surface of the wound, that there may be no cavities or pouches for the lodgement of matter, but that the restorative process may proceed in the best and most rapid manner at every point. The best dressings, I conceive,

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are a light compress and a bandage, aided by adhesive strips, or collodion plaster. Sutures I consider as objectionable, because the track made by them occasionally serves as a point of departure to the new deposit, thereby promoting relapse. We have already dwelt upon the importance of saving a sufficient amount of integuments, that is, skin and cellular substance. Any deficiency of this kind must necessarily interfere with the adhesive process.

When a sufficiency of integument cannot be preserved, and the wound is obliged to heal by the granulating process, it is, I think, worthy of consideration whether the whole of the raw surface should not be effectually cauterized with the nitrate of silver, or with a stick of the Vienna paste, so as to form a superficial eschar. The practice certainly derives support from the beneficial effects which are said to follow the treatment of cancer by cauterization in the hands of the empirics, as well as in the hands of certain scientific practitioners, especially Canquoin.

Rhazes,* the Arabian, who flourished in the latter part of the ninth and the beginning of the tenth centuries, and who obtained the appellation of the *experienced*, recommends that all the carcinomatous structures should be retrenched with the greatest possible care; and he goes so far even as to enjoin that all enlarged veins, as well as a portion of the surrounding healthy tissues, should be removed at the same time. Then, as if to render assurance doubly sure, he cauterized the surface of the wound with the red iron, and immediately after used such means as were calculated to promote the speedy separation of the eschar. A similar practice was afterwards pursued by other surgeons, but with what effect has not transpired. I beg leave to recall the attention of the Association to the subject, in the hope that it may engage their consideration, with a view of determining whether this treatment, in some modified form, is not worthy of renewed trial. In a disease which has hitherto baffled all our attempts at cure, and which is so prone to return after operation, even when performed under the most favourable circumstances, every experiment having for its object the destruction of the new substance and the prevention of its recurrence ought to be regarded not only as justifiable, but highly proper.

Considerable diversity of opinion has existed among surgeons as to the time when the operation should be undertaken with the best prospect of ultimate success. The preponderance of professional senti-

* Continens, lib. xiii. cap. 2, fol. 256.

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ment, however, has always been in favour of early interference, on the ground that the longer the disease is permitted to remain the greater, all other things being equal, will be the risk of contamination. The advocates of this measure, indeed, never countenance a resort to the knife when there is positive evidence that the disease has invaded the adjacent parts, or the system at large. Some, it is true, employ it with a view of prolonging life, or alleviating suffering, but never with the hope of effecting a radical cure. Among those who advocate the propriety and importance of delay, no one holds a more conspicuous rank than the late Mr. Pearson, of England. "The early extirpation of cancer," says this distinguished writer,* "confers no peculiar security against the return of the complaint; on the contrary, if the removal of the morbid part were equally complete in two patients, one of whom has been afflicted seven months, and the other seven years, with a cancer, I should esteem the latter patient in less danger of a relapse than the former. My reason for an opinion, which to some people may appear singular, is this, that when the breast, for example, is affected by the cancer, distant parts of that gland may become the seat of the morbid alteration about the same period. These several diseased portions may not advance with an equal celerity, but while one portion has acquired a considerable bulk, the other altered parts may be scarcely objects of attention. Under such circumstances the most obviously morbid parts may be removed, but the disease, being only in progression, no man can be certain, without removing the whole breast, that he has not left some diseased fibres. If, however, the disease shall continue without increasing during several years, one may in general conclude that its boundaries are more accurately defined." Similar sentiments have been expressed and advocated by several recent authorities.

Without stopping to dispute the accuracy of the opinions of the advocates for delay, I may state, as the result of my reading, that multiplied and reiterated experience has shown that, in the great majority of cases, if not in nearly all, the disease, if removed at a late stage, is only aggravated, and runs a more rapid course than it would do if left to itself. The testimony of Dr. Macfarlane, already alluded to, to say nothing of that of other surgeons, is conclusive upon this subject. This gentleman, who enjoyed ample opportunities of investigating the question, states that he could adduce a number

* Practical Observations on Cancerous Complaints, p. 31.

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of instances in which patients, after having laboured under malignant disease for fifteen, and even twenty years, were cut off in a few months by operation. The complaint, under such circumstances, has a peculiar repullulating tendency, owing apparently to an increased functional activity of the capillary vessels, and also to an increased perversion of the nutritive process. The operation acts, figuratively speaking, like a blow or bruise, rousing the parts which had served to circumscribe the morbid growth from their slumber, and causing them to engage in the formation and deposition of new cancer-cells, with a force and rapidity previously unknown.

§ 8. TREATMENT AFTER OPERATION.

Another object to be attended to, after operation for the removal of the diseased structure, is the patient's diet. Of the propriety and importance of this injunction no one can entertain any doubt. The force of this remark will appear more evident when it is recollected that the progress of cancer has occasionally been stayed for months and even years by a regular and persistent system of starvation, barely allowing a sufficient quantity of food, and that of the most bland and unirritant character, to maintain the due play of the vital functions, without too great a reduction of the heart's action. Pouteau in France, Callisen in Denmark, and Fearon and Pearson in England, have particularly insisted upon this kind of diet, asserting that its employment is of more service in arresting the progress of cancer than any medicines that have ever been suggested for the purpose.

The kind of diet is, doubtless, a matter of no little moment. As a general rule, it may be stated, that meats, soups, and the coarser varieties of vegetables should be proscribed, on account of their heating and indigestible character. For the same reason, condiments, such as mustard and pepper, wine, spirits, and fermented liquors are to be eschewed. Among the more suitable articles may be mentioned stale bread, toast, and soda biscuit, hominy, rice, sweet and Irish potatoes, mush, macaroni, baked apples, figs, and ripe fruits. Not only should the food be perfectly simple and easy of digestion, but great care should be taken that it is always thoroughly masticated, and that the quantity at each meal is never so great as to crowd and oppress the stomach. As drinks, the best articles are water, lemonade, milk, and weak tea. Coffee, as being too stimulating, must be avoided.

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Of late years, the attention of the profession has been called to the importance of an almost exclusively milk diet after an operation of this kind. The first mention which I find of this subject is by Antoine Deidier,* of Paris, as far back as a century and a quarter ago. In a case of carcinoma of the breast, he attributes the happy issue mainly to a diet of milk, prescribed after the operation. Some years ago, Dr. Leonard Pierce,† of Sutton, Massachusetts, published the particulars of a most interesting case of cancer of the mamma, showing the beneficial effects of this simple mode of living. The patient, his own mother, first discovered, in September, 1825, when in her sixty-second year, a tumour about the size of a chestnut in her right breast. A course of iodine, both external and internal, having failed to do any good, and the tumour continuing to increase, at the same time that it was attended with lancinating pains, excision was performed on the 29th of May, 1826. The wound healed without any difficulty, and the health remained as good as usual until the following winter, when another tumour appeared just beneath the cicatrice, accompanied with similar pains. This was removed, without any previous treatment, on the 5th of April, 1827. The wound showed but little disposition to heal, and in a few weeks two other tumours appeared in the neighbourhood of the former ones, and rapidly increasing soon ulcerated, and finally formed a hemispherical growth somewhat elevated above the skin, having an exceedingly jagged and angry aspect, bleeding on the slightest injury, and constantly discharging a thin, watery, irritating fluid. The pain in the part, as well as in the axilla, was excessively severe, and almost uninterrupted; there was also severe pain in the uterus, and everything, in short, seemed to indicate that the case would soon terminate in death.

On the 13th of September, 1827, soon after a severe attack of dysentery, which had reduced the patient very much, the knife was again used. At that time the tumour was larger than the breast, whose place it occupied, and it had for more than two months past bled from one to three ounces daily. A large portion of integument was removed along with the tumour, the base of which was nearly four inches in diameter; the pectoral muscle was perfectly sound; the wound soon entirely healed, partly by adhesion and partly by granulation; and the pains in the axilla and uterus entirely subsided.

It is proper to observe—for this is the chief motive for adducing

* Dissertation sur la Nature et la Guérison des Tumeurs, Paris, 1725.

† American Journal of the Medical Sciences, vol. viii. p. 49, 1831.

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the case—that this lady, at the suggestion of the late Professor Smith, of New Haven, had subsisted for about two months before the last operation upon the most bland and simple diet, consisting exclusively of boiled green corn, seasoned with salt, and cooling drinks. She had also employed, three times a day, a pill composed of about three grains and a half of red oxide of iron, and one grain and a half of extract of conium. This prescription was laid aside after the operation, but she rigidly continued her corn diet, using the article fresh, or otherwise, according to the season of the year; sometimes she ate it in the form of hominy, with molasses, or she had it ground, and made into bread with water.

After having used this fare for about a year, she attempted the use of animal food, barely tasting it; but was obliged to abandon it at once, as it made her mouth and throat sore, and produced pains in the axilla, similar to those she had experienced in the cancer. The trial was several times renewed, at intervals of several months, but always with similar results. Exercise and mental emotions had the same effect, for a considerable time after the operation, as animal food, and she was obliged to avoid them. She occasionally ate fish sparingly. Potatoes agreed well with her. This diet was continued faithfully up to the time—how long afterwards, I cannot say—of the publication of the case, in March, 1831. All animal food, liquors, condiments, and heating drinks were studiously avoided. She never, however, deprived herself of the use of tea, but always took it cool, in small quantity. From the time she began to diet herself, the alvine discharges were thin and watery, and so continued as long as she confined herself to green corn; they were also hot and excoriating. It had the effect, moreover, of greatly diminishing her strength; but this afterwards improved very much when she commenced the use of ripe corn, bread, and other vegetables. Under this management, the health became as good as it had been before she was affected with cancer, which had not returned at the period above referred to, that is, about three years and a half from the time of the last operation.

Another most interesting and instructive case of malignant disease, exhibiting the beneficial effects of a restricted diet, occurred a few years ago in the person of an honoured member of this Association, and has been carefully reported by Dr. H. J. Bowditch, of Boston.* I allude to the case of the late Dr. Twitchell, of New

* Bost Med. and Surg. Journ. vol. xli. p. 494.

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Hampshire. This gentleman, whose grandmother died of cancer of the breast, and whose sister had scirrhus of the pylorus, observed, when nearly sixty years of age, a small, hard tumour, free from pain, and not larger than a grain of mustard, at the inner angle of the right eye. It was imbedded in the substance of the skin, and seemed little inclined to increase. In 1843, it had attained the volume of a pea, and had a tendency to form scabs, the removal of which, under the use of Janning's eye-salve, usually exposed a small lobulated surface, covered with a little purulent fluid. In 1845, the greater portion of the tumour was excised by Dr. Hayward, of Boston. For a short time, the wound seemed to do well; but, finally, refusing to heal, the part was removed again two months afterwards, and touched with nitrate of silver. Meanwhile, however, it had become the seat of a deep-seated and rather severe pain, radiating towards the brow and cheek, and less transitory than before.

The tumour continued to augment slightly, and in the spring of 1847 it exhibited a decidedly malignant aspect. It was an ulcer, about the size of the top of the finger, with hard, ragged, and elevated edges, and an irritating discharge, which at night caused a gluing of the lids. Dr. Twitchell now determined to diet himself most rigidly, and for this purpose he used, three times daily, from four to six ounces of cream or rich milk, and the same quantity of white or brown bread. This course was faithfully persevered in with the effect of a complete cure until 1850, when the case was published. The pains in the part were lessened almost immediately; the purulent discharge also soon diminished; and it became apparent, in a few months, that the disease was not augmenting; the cure gradually progressed, and in August, 1848, the ulcerated mass was entirely gone, leaving the angle of the eye perfectly natural, excepting a minute white cicatrice, about a line in diameter. It is worthy of remark that this rigid course of dieting exerted no pernicious influence upon the patient's general health; but that, on the contrary, he presented, at the end of two years, when the cure was completed, the picture of a hale, robust man.

In a case of osteo-sarcoma of the scapula, in a man about forty years of age, a tumour, as large as a pint-bowl, was reduced, by this simple kind of diet, to such an extent that there was, at the end of two years, merely a trifling thickness of the skin. The disease, when first seen by Dr. Twitchell, whose patient the man was, looked so unpromising that this gentleman declined extirpation, being, in some degree, deterred by the result of a similar case, where the in-

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dividual died, sometime after the operation, from carcinoma of some one of the internal organs.*

A case of cancer of the womb, in which the progress of the affection appears to have been retarded, if not arrested, for a long time, occurred, a number of years ago, in the practice of Dr. James McKeen, Professor of Obstetrics in the Medical School at Brunswick, Maine.† The woman, who was thirty-nine years of age, had been in the habit of living upon rich food and drinking strong coffee, and had suffered greatly from the disease referred to. Under the use of a simple farinaceous diet, and nothing but water for her drink, pursued for three years, she experienced so little pain and inconvenience that she thought herself at times cured. At the end of this period, at the solicitation of her friends, she lived for a few days upon strong food, such as she had not been accustomed to, and the consequence was a most severe attack of pain, followed by a foul discharge, and, in a few weeks, by her death. A *post-mortem* examination revealed the entire destruction of the neck of the uterus, with deep ulcers in the body of the organ, and apertures of communication with the bladder and rectum.

The late Professor McClellan,‡ of Philadelphia, refers to a case where a genuine carcinoma of the breast was kept in check for twenty years mainly by a course of rigid dieting. The tumour progressed rapidly for the first eight or ten months, until it had attained the size of a double fist, when it began to ulcerate near the centre, and to discharge ichorous and offensive matter. By detergent lotions, anodynes, and emollients, with great attention to cleanliness and the alvine evacuations, and a diet consisting almost exclusively of milk, bread, and vegetables, the disease was arrested, the pain alleviated, and the general health maintained in a good condition.

To prevent relapse after operation for malignant diseases, it has been suggested that the patient should be restricted almost exclusively to pure water. Ponteau,§ of Lyons, flattered himself that he had, by this treatment, effected several radical cures. His practice consisted in giving his patients each from five to six pints of ice-water in the twenty-four hours. At the end of three days, says he, the appetite usually ceases, and the individual is hardly incommoded by the privation, living often fifty and even sixty days with-

* Boston Med. and Surg. Journ. vol. xli. p. 498, 1849.

† Mussey's Report, Trans. Amer. Med. Association, vol. iii. p. 330, 1850.

‡ Principles and Practice of Surgery, p. 400, Phila. 1848.

§ Œuvres Posthumes, t. 1, p. 96, Paris, 1783.

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out a particle of food of any kind. Should the breath become offensive and the tongue coated, he recommends the exhibition of from two to three drachms of magnesia, in divided doses, in the morning. At the end of two months, this rigid abstinence is abandoned, and the patient gradually resumes his former mode of living, beginning with the yolk of an egg, beaten up in two tumblersful of cold water, then with creams and soups, and finally with solid food. Ponteau thinks that the use of ice-water is more in harmony with nature than ordinary water; that it is, in short, a better tonic, and that it is better calculated, so to speak, to deceive the appetite, thereby rendering this feeling more endurable.

Dr. William Lambe,* an English physician, embracing the views of Ponteau, recommends the substitution of distilled for ice-water, as a curative agent in this complaint; but it does not appear that he obtained any particular benefit from its employment. The *water practice*, as it may be termed, of these writers, has never become popular with the profession, and I have no facts to offer in support of its utility.

Valsalva, and, subsequently, Fearon, a surgeon of London, recommended the repeated abstraction of blood by leeches, as a means of curing cancer, and a similar mode of treatment has occasionally been pursued to prevent relapse after operation. It is difficult to perceive the *modus operandi* of such a remedy in such a case, or the principle upon which it could afford relief. Fearon, believing that all cancerous affections are of an inflammatory nature, employed it as an ordinary antiphlogistic.

It is hardly necessary, in a paper of this kind, to speak of the various internal remedies that have been, from time to time, proposed for the cure of cancer, and for the prevention of its relapse after extirpation. Suffice it to say that none of them have fulfilled the promises and hopes that practitioners have been led to expect from their exhibition, and that there is not, in all the long and variegated catalogue, a solitary one that can, in anywise or degree, be viewed in the light of a specific, or as a neutralizer of the morbid poison concerned in the production of malignant disease, in any of its diversified forms and characters. Whatever benefit may attend their employment flows solely and entirely from the good effects which they exert upon the digestive organs, and, through them, upon

* Report on the Effects of a Peculiar Regimen on Scirrhous Tumours and Cancerous Ulcers, London, 1809.

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the system at large, or, in other words, upon both solids and fluids. If there be any remedy calculated to prevent the formation of the cancer-cell, or to arrest its progress after it has been developed, we are ignorant of it. Human science and human skill may destroy, rudely and mechanically, a malignant tumour in an accessible part of the body; but they have not yet taught us, if, indeed, they ever can teach us, how to change and modify those circumstances which precede and attend its creation under the plastic powers of the capillary vessels of the affected structures. Possibly, the joint agency of animal chemistry, the microscope, and therapeutics, may some day enlighten us upon a subject which has hitherto eluded our investigations, conducted though they have been through a series of ages, as if to admonish us of the imperfections of our art, the imbecility of our nature, and the vanity of our inquiries.

Nearly twenty years ago, a French surgeon, M. Martinet (de la Creuse),* thought that he had discovered a method by which, in many cases, if not in all, a return of the disease after ablation might be effectually prevented. It consists in filling up the gap left by the removal of the morbid mass with a flap of sound skin, taken from the neighbourhood, and carefully adapted, by means of stitches and other means, to the raw edges of the wound, to insure their speedy reunion. The object of the procedure is to change the functions of the parts in such a manner as to restore their healthy nutrition, thereby destroying the tendency to cancerous action. When M. Martinet published his paper on the subject, in 1834, he had performed this species of anaplasty in four instances, and in every one with the most gratifying success. One of his patients was seen six years after the operation in perfect health; and two others remained without relapse at the end of three years. In the other case, death occurred at the expiration of two years and a half, in consequence of an affection unconnected with cancer. The operation has been performed by Sedillot,† Jobert de Lambelle, Dieffenbach, Zeis, Baroni, Rizzoli, and other surgeons; but with results so unsatisfactory as to hold out no inducements for repeating it. Zeis‡ has formed a very unfavourable estimate of the practice, and the experience of Baroni and Rizzoli,§ of Italy, proves that it does not exert the slightest influence upon the reproduction of the disease.

* Gaz. Méd. de Paris, No. 42, 1834.

† Annales de la Chirurgie, t. 15, p. 105, Paris, 1845.

‡ Handbuch der Plastich. Chirurgie, s. 367.

§ Bulletino delle Scienze Mediche, serie ii. vol. 7, p. 237.

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My experience with this operation is quite limited, having performed it only twice. In both cases the patient had epithelial cancer of the lower eyelid, and in both the disease returned after an interval of several months. In another case, of which I have the particulars, the patient, a gentleman, aged about fifty, had cancer of the lip, which, after having been removed several times, was finally treated by anaplasty. For awhile, the transplanted parts retained their healthy character, but at the end of a few months they gradually became hard and rigid, and exhibited all the evidences of cancerous disease. This patient has just died from the effects of his disease.

I find that many of the older surgeons, and, indeed, quite a number also of the modern, strongly insist upon the establishment of an *issue*, seton, or perpetual blister, as means of preventing a return of the disease after extirpation. In the time of Baron Boyer and Benjamin Bell, the practice seems to have been in very common vogue. Monteggia, of Milan, lays much stress upon it, as late as 1815.* He says that Desault, who neglected attention to this subject, and who permitted his patients to take care of their health as best they could, lost nearly every one from speedy relapse. Professor Chelius, of Heidelberg, is one of the latest writers who insist upon the importance of this mode of treatment. M. Rayer also thinks well of it, and fancies that reproduction is less frequent after operation, especially in cutaneous cancer, when an issue is made in each corresponding extremity.

Of the value of this practice, which consists in maintaining a free discharge in the neighbourhood of the original disease for several years, or even during the remainder of the patient's life, we have no positive information. Those who seem to have the greatest confidence in its efficacy have failed to adduce any facts in support of it. Their ideas respecting it are evidently of a vague and conjectural character. It is difficult to conceive how such an agent could prevent the formation of cancer-cells, and yet it might exercise such an influence. I submit it to the members of the Association, whether it would not be well to revive this mode of treatment, in connection with a well-regulated diet and strict attention to the bowels and secretions, after operation? The true value of it, at all events, can be determined only by future trials.

* Institutioni Chirurgiche, vol. vii. p. 213, Milano, 1815.

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PART II. GENERAL OBSERVATIONS ON CANCER, IN RELATION TO THE RESULTS OF SURGICAL OPERATIONS.

It will be the object of the Committee to arrange under this head such facts and observations as have been contributed by different surgeons, and gleaned from American and European writers. The information obtained from the two latter sources is comparatively meagre; but this circumstance is to be the less regretted, because ample opportunity will be afforded, in the section on carcinoma of the mammary gland, for an exhibition of the views and recommendations, not only of our own surgeons, but of the surgeons of different countries, in respect to the propriety or impropriety, utility or inutility of operation for the relief and cure of malignant diseases.

The most valuable information that has been furnished to the Committee, in relation to this department of the subject, is from the pen of Dr. J. M. Warren, of Boston, and from that of Professor Hamilton, of Buffalo. The paper of the former of these distinguished gentlemen, comprises: first, a history of twenty cases of cancer, as it appeared in different organs of the body; secondly, a general summary of the operations for cancer at the Massachusetts General Hospital, from 1822 to 1852; and, thirdly, a record of operations for cancer in the hospital and private practice of the author. The table of Professor Hamilton gives the particulars of fifty-nine cases of the disease, and has evidently been drawn up with much care. Both articles are worthy of attentive consideration and reflection.

§ 1. FACTS AND OBSERVATIONS CONTRIBUTED BY J. MASON WARREN, M.D., OF BOSTON.

The attempt to arrive at the results of surgical operations in malignant diseases is attended with many difficulties. Most patients thus afflicted come from a distance, to present themselves to a surgeon in the city, either in private or hospital practice; the operation is performed; the patient disappears, and is often completely lost sight of. This is true of a large majority of the cases which occur. The important question, therefore, seems to resolve itself into this—

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are there any cases known of patients operated on for cancer, who have remained well after the lapse of a number of years? If any such can be produced, it is certainly an important element in encouraging us to attack malignant growths by the knife.

A number of the cases, hereafter to be given, seem to lead to the conclusion that a cure by the knife is sometimes to be expected; others show that a removal of the tumour may be resorted to for the purpose of deferring the evil day; and, finally, a third class exhibit the operation simply as a palliative to smooth the pathway to the grave.

The accounts contained in the following pages may, for the sake of arrangement, be classified under these heads:—

1. Some operations for the removal of cancer, from which the patient still survives.

2. Others, in which the disease having been removed, has returned after a number of years, and again been subjected to the knife with relief.

3. Others, which are interesting from their peculiarity.

4. The statistics of cancer from the records of the Massachusetts General Hospital. A table of these, from the opening of the Institution to 1850, was prepared under the direction of Dr. Parkman, some years since, with great care, and kindly lent me by him. This has been completed under my own direction by Mr. Sawyer, the present surgical house-pupil. A general summary of these has been introduced in the present article.

5. To these have been added some cases of my own public and private practice within the last few years, chiefly of larger operations, no record having been preserved of many upon the lips and skin. My earlier notes are also not sufficiently detailed to afford any satisfactory results; and the patients, with the exception of the cases to be given, are mostly lost sight of.

The hospital statistics simply show the part most affected by cancer, and the immediate result, whether fatal or not.

CASE I. *Scirrhoma of the Left Breast. Excision. No Return at the End of Twelve Years.*—1840. Mrs. V., 50 years old, the mother of three children, has always enjoyed good health until the occurrence of the present disease. Her complexion and hair are both light. Early in life, she had a milk abscess in the left breast, which has left a slight induration. She knows of no hereditary disposition to cancer.

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About two years since, she began to have an uneasiness in the left breast, and soon discovered a slight hardness in the glandular structure. This has gradually increased till it now involves the whole breast; the nipple is retracted. She suffers much pain, and is very desirous of an operation. The glands in the axilla seem not to be affected. The breast itself is the seat of a large hard tumour, movable on the subjacent structures; the skin at different points is slightly retracted and adherent to the tumour beneath.

The disease was removed by two semilunar incisions, embracing the nipple and a portion of the adherent skin; the breast being large, required quite an extensive dissection. Dr. J. C. Warren, Dr. Cantaro, and other gentlemen assisted at the operation. The wound was brought together by sutures and adhesive straps, secured by compresses and a bandage. The subsequent prostration was considerable, but the recovery progressed well.

February, 1852. I have heard from this lady, who is now in England. She is in the enjoyment of good health, and at present, so far as I can ascertain, there are no signs of a recurrence of the disease.

CASE II. *Encephaloid Tumour of the Breast. Removal. No Return at the End of Six Years.*—January, 1846. Mrs. G., of South Boston, 69 years of age, a small, thin woman, and the mother of a number of children, has always had good health. So far as she knows there is no hereditary tendency to cancerous disease.

About eighteen months since a tumour appeared in the right breast, and grew with great rapidity; so as to involve the whole breast in a large tumour, of a pyramidal shape, quite tense and elastic, the summit occupied by a bright red spot, apparently on the point of ulceration. The tumour was quite movable on the subjacent parts.

The patient, having great dread of an ulcerated cancer, was very desirous to have an operation performed, and as the tumour was on the point of breaking through its bounds, when severe, if not dangerous hemorrhage would undoubtedly ensue, I consented. The friends, however, were first made aware how little was to be expected from the removal of a disease which had proceeded to such an advanced stage, and also of the fact that on the right arm there was a small, movable, melanotic tumour, the size of a hazel-nut.

The tumour was removed in the ordinary way, with as much rapidity as possible. On account of the great vascularity of the

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parts, every incision was followed by a violent gush of blood, leaving the patient seriously faint at its termination. The edges of the wound were approximated as nearly as the form would admit of, and moderate compression made by a roller around the chest.

On examination, the tumour was found to be a specimen of pure encephaloid, in weight about three pounds. It was shown at one of the meetings of the Society for Medical Improvement, and no doubt entertained as to its character.

The recovery of this lady was good. From all appearances at the time of the operation, her chance of living without a removal of the disease could hardly have been estimated beyond two or three months; but in 1848, I saw her in good health, and am informed at this date (Feb. 1852) that she is still living, and has never experienced any inconvenience from the operation or relapse of the disease.

Case III. *Excision of the Horizontal Part of the Lower Jaw for Malignant Disease.*—May 13. The patient was a healthy boy, fifteen years old, with no hereditary tendency to malignant disease, so far as could be ascertained. Two years and a half since, a small red fungous tumour made its appearance between the middle incisor teeth of the lower jaw. This tumour gradually increased, separating the teeth, and finally involved the whole depth of the bone. At the time of the operation the lip was much pressed outwards, and the cavity of the mouth so encroached upon as materially to embarrass the speech and prevent mastication.

The disease was removed by making an incision through the lip in the median line, and extending it as far as the os hyoides. The soft parts being dissected from the bone, this was partly sawed through by means of a metacarpal saw, the second bicuspid tooth on either side being removed. The incision through the bone was completed with cutting forceps.

Before separating the attachments between the jaw and the tongue, a ligature was passed through the mucous membrane at the base of that organ, in order to prevent retraction, an accident that Dr. W. had seen happen in an operation similar to the present, the patient being saved from suffocation by the surgeon thrusting his finger down the throat, hooking up the tongue, and securing it by a ligature.

In the present instance there was no disposition in the tongue to retract, but, for the sake of precaution, the ligature was passed

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through the wound, and formed one of the sutures which confined the edges of the lip when they were approximated.

The portions of the jaw removed show the whole bony structure expanded and enlarged. On the alveolar edge one bicuspid tooth remained on each side. On the left side two incisors. The right incisor teeth had disappeared. The right cuspidatus was found deeply buried in the jaw, lying diagonally across the root of the bicuspid, never having come to the surface. From the expansion of the jaw by the disease, nearly two inches existed between the bicuspid tooth of the right side and the incisor of the left.

A small portion of the expanded shell of the bone being cut away from the internal face of the jaw, exposed a fungoid mass filling the interior. This, on being submitted to microscopic examination, presented well-marked cancer-cells.

The disease was limited by the bone, the soft parts in the vicinity exhibiting no marks of disease.—*Records Bost. Soc. Med. Improvement.*

Case IV. *Scirrhus of the Left Breast. Excision. No Return of the Disease.*—August 19, 1848. This patient was an Irish widow, 40 years of age. About nine months previous to her admission to the Massachusetts General Hospital, she suddenly experienced a sharp darting pain in the left breast, near the nipple, and on applying her hand detected a small hard lump, the size of a pea, which gradually increased to that of a hen's egg. "Sharp stinging pains have troubled her for the last two months, since the nipple began to be retracted. She thinks she has nursed her children more from this breast than the other."

The breast was removed upon the day of her admission, and in rather more than six weeks she was discharged well.

March 16, 1852. This patient, who is a large powerful person, was seen by me to-day. She is now, and has been since her recovery from the effects of the operation, in the enjoyment of perfect health. The scar is about four inches long, quite soft, and healthy.

Case V. *Encephaloid Tumour in the Cheek. Removal. Recovery.*—A young gentleman from Halifax, aged 16 years, came under my care in January, for a tumour situated in the substance of the right cheek. One of a similar nature had been removed from the same situation two years before, but had speedily reappeared. When I saw this patient, the whole right side of his face looked larger than

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the other, the veins were everywhere distended, and, at first sight, a malignant disease of the antrum would have been suspected.

On careful examination, I discovered a lobulated tumour deeply seated in the substance of the cheek, just below the zygomatic arch, and apparently running up under that bone. It was quite hard, and moderately movable. A large vascular polypus also occupied the right nostril, and entirely obstructed the breathing through that side.

In consultation with Dr. J. C. Warren, it was determined to remove the tumour, which was done on January 28, in the presence of a number of medical gentlemen. Ether was exhibited by Dr. Morton, and in four minutes the patient was quite insensible. A transverse incision was then made along the lower edge of the zygoma, through the skin and muscular substance, which exposed an erectile vascular tissue, entirely enveloping the tumour, and intimately connected with the surrounding parts. In dividing this, in order to arrive at the body of the tumour, a hemorrhage commenced, which greatly obscured the dissection, and it was only by compressing the carotid artery that the operation could be continued.

The tumour was now discovered extending up under the zygomatic arch, but only attached in that direction by a loose cellular tissue; behind, it dipped down in the direction of the spheno-maxillary fissure. It was finally detached from these different connections, after a long dissection. The operation was suspended from time to time, in order to allow the repetition of the ether, which was thus three times repeated at the request of the patient, and after the first time with an almost instantaneous effect. He said subsequently that he had experienced no pain, and that his impressions were of an agreeable character. His call for the ether, he said, was partly for the pleasure of taking it, and not entirely on account of the relief it afforded him from suffering.

In consequence of the great hemorrhage from every part of the wound, it was finally found necessary to stop the bleeding by compression with sponges. No subsequent bleeding took place, and the patient recovered without any bad symptoms. One or two weeks elapsed, however, before the sponges could be extricated from the wound, so firmly were they embraced by the granulations, and at length removal was accomplished only by tearing them away piecemeal. The disease was of the encephaloid character, and entirely surrounded by erectile tissue, the division of which gave rise to the hemorrhage. With the exception of the actual cautery, the use of the sponges seemed to me the only means of stopping the flow of

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blood, and from the great difficulty here encountered in the removal of these from the wound, I think that in a similar case I should give the preference to the former.—Taken from the *Boston Medical and Surgical Journal*, 1847.

May, 1852. This patient I have seen within a month, and there had at that time been no recurrence of the tumour.

Case VI. *Scirrhoma of the Parotid. Removal. Recovery.*—A large, powerful, plethoric man, 34 years of age, consulted me for a tumour in the right parotid gland. A tumour had been removed from the same spot twenty years ago, which shortly afterwards reappeared in the form of a small, hard tubercle under the ear. For fifteen years, it remained stationary, and then began to increase. It is now about the size of a hen's egg, of a bluish colour, lobulated, and having a hard base surrounded by small cysts. The lobe of the ear is pushed upward by the tumour, which extends inward and apparently involves the lower half of the parotid gland.

As the patient was very desirous of an operation, I determined on its removal. It was a question whether it would not be advisable to apply first a ligature to the carotid. On examination, by a number of medical gentlemen, it was thought possible to remove it without having recourse to this means. Drs. J. C. Warren, George Parkman, S. Parkman, Briggs, and Mr. Slade, were present at the operation.

Ether was given him by Dr. Morton, who was desirous of seeing its effects on so powerful a patient. The pulse, before its exhibition, was 90. On commencing the inhalation, the eyes and face were much injected with blood, but as it was continued he gradually became pale, and in about the ordinary time was quite insensible. The tumour required a very slow and careful dissection; the base of it was found to have undergone osseous degeneration, and pressed upon the facial nerve as it crossed the styloid process. This was the cause of a paralysis of the side of the face, which was gradually taking place, a fact I omitted to state in the history of the case.

Towards the middle of the operation, the patient began to make strong muscular exertions, indicative of returning consciousness, and which somewhat interfered with the dissection. As soon as he complained of pain, a second dose of ether was administered, which took effect speedily, and prevented him from suffering for the remainder of the time. The operation occupied about half an hour; and, although it was severe, not the slightest constitutional trouble fol-

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lowed. In the course of a week, he was able to return home to Maine.—Taken from the same *Work*.

March, 1852. This patient has been heard from not long since as in good condition.

These two last-recorded cases were among the earliest operations performed under the influence of ether.

CASE VII. *Cancer of the Eyelid.*—April 25, 1839. A healthy, robust-looking sailor, 71 years old, noticed a small pimple at the inner corner of the left orbit, about four years ago. To this, others succeeded, which coalesced into one tumour, with a warty appearance on its surface, when he picked off the scab. There have been occasionally slight pains in it. At the present date, there is a small irregular wart-like tumour on the nasal aspect of the left orbit, covered by a thin dark-brown scab.

On April 28, patient being well etherized, the tumour, with a sufficient portion of the healthy surrounding structures, was excised, and the surface touched with potassa fusa. Two small arteries were tied, and the parts brought together by four sutures. Collodion was applied, forming a thin, delicate scab over the wound. In about three weeks he was discharged well, and remains so at this present time, March, 1852.

CASE VIII. *Scirrhus of the Breast.*—May, 1849. Mrs. M., 45 years of age, applied to me for advice on account of a large hard tumour of the breast, slightly adherent to the skin, and which had appeared between ten and twelve years previously. The patient was a powerful, fine-looking woman, the mother of a number of children, and until the appearance of the tumour had always enjoyed perfect health.

A small movable mass was first discovered implanted in the glandular part of the breast, and had gradually enlarged until the whole of that organ had been involved. The disease caused her much pain and inconvenience, though it had no decidedly bad influence upon her general health. She formerly had a milk abscess in this breast.

Under chloric ether, the whole breast was removed, a sufficient portion of the integuments being saved to allow the edges of the wound to be approximated.

The disease was undoubted scirrhus.

A day or two after the operation, a sudden hemorrhage took place from the wound, but it was checked before I arrived by the simple

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application of compresses wet with cold water over the dressings. Some suppuration ensued, union by the first intention being prevented by the coagula which had collected. In the course of three or four weeks, she was sufficiently well to return home, a long distance from Boston, the wound being then almost healed.

During the past week (March 16, 1852), having had occasion to operate for cancer of the breast, on a lady, a connection of the patient, I was informed that at the present time she enjoys good health, and has had no return of disease in the part operated on.

CASE IX. *Carcinomatous Tumour of Upper Jaw.*—April 3, 1850. A lady, 65 years of age, had always enjoyed good health until about six months since. At this period, she observed a small tumour between the middle incisor teeth of the upper jaw, which gradually loosened and fell out, shortly followed by the lateral incisors.

The tumour was, at the time of her appearance, of the size of a pigeon's egg, rather elastic, crackling, as if having a delicate shell of bone. The alveolar process was implicated in the disease, which impinged on the nasal cavities.

The patient being brought under the influence of ether, and the mouth secured in such a position, by means of wedges between the teeth, that the jaw could be easily worked upon, the tumour was removed by the knife and a powerful cutting-forceps. The lower part of the left maxillary sinus was exposed by the operation. The bleeding was profuse, but was checked without having recourse to the actual cautery, which had been kept in readiness, in case of necessity.

This patient had a good recovery, and is now (March, 1852) in the enjoyment of good health. The disease removed consisted of a shell of bone, containing in the centre a soft granular matter having the appearance of colloid.

CASE X. *Scirrhoma of the Left Breast. Removal. Reappearance in the Right Breast after twenty years. Removal. No Return since.*—Mrs. B., 50 years of age, of clear complexion, dark hair, considerable *embonpoint*, good colour, and having every appearance of fine health, consulted me in September, 1851, for a tumour of the right breast. The swelling had presented itself nine years previous, without any assignable cause; but it was not until the last year that it had given much trouble.

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On examination, there could be discovered a hard lump, occupying the central portion of the breast, quite firm, and having all the marks of scirrhus. The axillary glands were not enlarged.

The patient informed me that, twenty years previous, Dr. John C. Warren removed a scirrhous tumour from the left breast, which had been of slow growth and was the result of a blow.

The mammary gland and nipple were removed in the subsequent operation (1851), with the assistance of Drs. Minot and Abbott. The edges of the wound were approximated by means of two or three sutures and adhesive straps. At the first dressing, on the third day, the skin was found to have entirely healed by the first intention. On the fifth day, however, a small, painful spot was complained of by the patient just above the wound, and at this point a collection formed, to which it was necessary to give exit a day or two after. The extremities of the wound also gave way, and discharged a bloody serum. The whole wound then healed regularly and kindly, and the patient is now (Feb. 1852) in the enjoyment of perfect health.

This case is interesting from the fact of a removal of a scirrhous tumour from the breast, and no return of the disease for eleven years subsequently; a second tumour then appearing in the opposite breast, and nine years elapsing before it was sufficiently troublesome to require extirpation. Her aspect was that of a fine, handsome woman, and wholly unlike that of the victim of a malignant disease.

In regard to the suppuration which took place in the wound, I would remark that I have almost invariably observed that where union by the first intention is attempted, and succeeds, in persons of much adipose substance, the wound sooner or later gives way again from the suppuration in the subcutaneous tissue. In thin persons, a sound union may be expected; in fat subjects, hardly ever, according to my experience.

CASE XI. *Malignant Tumour arising from Periosteum of Left Tibia. Removal of the Limb. Health good at the end of fifteen months.*—Dec. 1850. A mechanic, 52 years of age, with general good health, and no hereditary tendency to cancer, consulted me, and stated that, about twenty-three years ago, he struck a heavy iron hook against the middle of the left shin-bone, in front. A swelling very soon appeared, much harder than usual, not discoloured, and very tender, which soon increased to the size of a small egg. And thus it remained for twenty years.

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In the summer of 1847, the tumour was accidentally struck by a hammer. At the time, the pain was excruciating, but subsided sufficiently in the course of a day to allow him to attend to his business, recurring occasionally for two or three months, mostly in the night. At the expiration of this period, he perceived the tumour to have gradually enlarged, having in eighteen months attained the size of an orange, with a proportionate increase of pain. It was very red; at the apex of a bright crimson, and the integument over it very thin and tense. The limb below was swollen and œdematous. Granulations, bleeding at every touch, arose from a slight abrasion. Such was its condition when the tumour and portions of the tibia, to which it was adherent, were removed by a surgeon.

In three months the wound healed soundly, and thus remained about a year, when there appeared, on the outside of the cicatrix, a swelling of the size of a cent, which followed precisely the course of the preceding tumour. At the time of removal of the limb, it had assumed the following appearance:—

"Six inches above the malleolus, on the outer aspect of the left tibia, and apparently adherent to it, was a lobulated tumour, eight inches and three quarters in circumference, two inches in height. It was somewhat constricted at its base by the integument, through which it had extruded, as if forced up from beneath. To the inside of its base was a hard tumour, in size and shape like a large almond." The apex of the large tumour had a sloughing aspect; the integument at the base, which was the seat of the greatest tenderness, was reddened.

The limb was amputated by the circular method, and upon a section of the tibia the tumour was found to have been between the periosteum and the bone. It was firm, white, and fibrous, of the size of a walnut, and from one side sprung the fungous growth described above.

Case XII. *Tumour of the Right Arm, requiring Amputation at the Shoulder-joint.*—1851. A man, tall and thin, about 30 years of age, had always enjoyed good health, until April, 1850, when he had a bloody discharge from the urethra, without pain. In June following, there appeared under the skin of the arm, near the insertion of the deltoid muscle, a small hard lump, which ultimately encircled the arm, raising up the brachial vessels and nerves. The motions of the arm were not much affected, but for a short time previous to his application the tumour had rapidly increased in size and in pain.

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On Feb. 15, 1851, the patient being etherized, an incision was made into the tumour, when it was unanimously pronounced by the medical gentlemen present to be a carcinomatous growth. The removal of the arm at the shoulder-joint, as had been previously determined upon, was therefore performed by an anterior and posterior flap. But little blood was lost, in consequence of the scientific compression of the subclavian artery by Dr. Williams.

"The tumour, on dissection, presented a lardaceous appearance, and, under the microscope, showed much fibrous tissue, in which, after much investigation, cancer-cells were distinguished."

This patient recovered rapidly, and at the present date, February, 1852, is, I believe, in good health.

CASE XIII. *Scirrhus of the Right Breast. Excision. Subsequent Tumour in the Thigh, of a questionable character.*—September, 1847. Mrs. J. W., forty-five years of age; quite tall, thin, skin somewhat dry and sallow, chest flat, with health generally delicate, the mother of two children, and no hereditary tendency to cancer, had a milk abscess in both breasts, leaving an induration there. Two years since, a tumour made its appearance in the inner part of right breast, which gradually became attached to the nipple, causing a retraction of that organ. At the time I saw the patient, the tumour was the size of a hen's egg, or perhaps larger; irregular, moderately movable, very hard. It gave her much pain, and she was extremely desirous of having it removed.

From the delicate appearance of this patient, and the irregular formation of the tumour, I had great fears as to the result of the operation, and so stated to her friends. As the inconvenience from it was so great, they determined to have the operation performed, even if it was done merely for the palliative effect. I therefore went into the country, some forty miles from Boston, for this purpose. The whole gland, with the nipple, and a portion of skin, was removed, and the edges of the wound approximated with adhesive straps. To my surprise and pleasure, I heard from this patient, some three years after, through her physician, who said that the wound had healed, and that she enjoyed good health. The sister of this patient, who was present at the operation, had a large tumour of the breast removed ten years before, which was of very rapid growth, and possessed all the appearance, according to her description, of a fungoid disease.

To-day, March 16, 1852, this lady applied to me on account of

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a hard movable tumour in the left thigh, placed over the course of the femoral vessels, an inch or more from Poupart's ligament. I asked permission to examine her breast. The cicatrix, four or five inches in length, was perfectly healthy, as were the skin and glands in the neighbourhood. Her health was good, and she had been once brought to bed of a living male child since the operation.

As to the nature of the tumour in the thigh, which is without pain, and as yet of but slight inconvenience to her, time only can determine. Her appearance is quite healthy, and presented a striking contrast to what it was previous to the removal of the cancer.

May 4. I removed the above tumour, and the patient is now (1853) well, and has lately been confined with a healthy child.

CASE XIV. *Cancer of the Breast. Removal. Reappearance at the end of ten years. Excision.*—1851. Mrs. ——, sixty-four years old, applied to me, in 1841, for a cancer of the right breast. At this time she was a widow, and her last child was an adult, after the weaning of which the milk had remained in her breast for three years. The whole mammary gland had been invaded by scirrhous disease, and the skin covering it eaten off by the applications of a cancer doctor, who had left her in this condition after informing her that she was cured.

I removed the whole breast, and the wound healed well by the second intention.

In January, 1851, I was requested to visit this patient out of town, as another tumour had made its appearance near the axilla, and on the same side of the chest with the previous operation. I found the lady engaged in her household affairs, quite lively, and not at all disposed to give way on account of the reappearance of the disease.

Since my previous attendance she had again entered the matrimonial state. She had experienced no inconvenience at the seat of her old disease until the last year, when a small, hard lump had sprung up on the edge of the pectoral muscle, which was now adherent to the skin and to the muscle.

With the assistance of Dr. B. Brown, who accompanied me for the purpose, I removed the disease, Drs. Gould and Sullivan, physicians of the place, being present.

The husband of this lady informed me in January, 1852, that she enjoyed perfect health, and had no return of the local affection. He

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also informed me that, previous to the last operation, she had been in the habit of remaining constipated for five or six days, and had sometimes gone sixteen days without an alvine evacuation, the food being rejected from the stomach once or twice in the twenty-four hours. During the past year, by careful attention to diet and regimen, assisted by some saline purgatives, she has been able entirely to overcome this torpidity of the digestive function.

CASE XV. *Scirrhoma of the Breast in a woman seventy-six years old. Removal. Return of the Disease in the Cicatrix four years subsequently. Again removed. Recovery.*—1847. Mrs. F——, aged seventy-six years, of florid complexion, quite fleshy, by occupation a nurse, entered the Massachusetts General Hospital in October, 1847, for a scirrhous tumour of the right breast. When quite a child, she said that a "boil" formed near the nipple of this breast and discharged itself, leaving a small indurated spot under the skin. Within the last twenty years, the present disease had been gradually increasing, having commenced in the above induration, and extending over the whole breast. The nipple had retracted, and a bloody serum issued from it. The lancinating pains were so severe as to induce her application at the hospital for relief.

I removed the whole breast, which was a large one, the incision being over nine inches in length, and she had a good recovery, being discharged well in about a month. The disease was genuine scirrhus.

In December, 1851, I was requested to see this patient by Dr. Francis Minot, at the Hospital for Aged Females, under his charge. She had perceived no return of the disease, and had been quite well until within a year, when a small indurated spot appeared in the centre of the cicatrix made by the former operation. I agreed with Dr. Minot in the propriety of removing this disease at once, and, the patient consenting, it was done by him a few days after. I have since understood that the wound healed well.

CASE XVI. *Cancer from a Blow, with Hereditary Predisposition.*—March, 1851. Mrs. R., sixty years of age, lost her husband a few months since. She has generally been healthy; her father lived to a good age; her mother had a cancer of the breast at the time of Mrs. R.'s birth, and afterwards died of cancer of the uterus. One of her sisters had cancer of the breast.

About six months since, she received a violent blow on the arm.

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and chest, occasioning inability to use the arm afterwards, the shoulder-joint being quite stiff, and almost immovable. Shortly after the reception of the blow she began to have pain in the right breast, and a small, hard tumour made its appearance there, which slowly increased, and became the seat of a burning and shooting pain.

On examination, I found a tumour the size of an egg, having all the physical characters of scirrhus, to occupy the axillary side of the nipple, the skin at this point being drawn in and attached to it. The stiffness of the shoulder-joint rendered it impossible to inspect the axilla satisfactorily. Her health had been much impaired by the long confinement incident to the injury of the arm, and by a chronic bronchial irritation, but the suffering from the disease was such that she was very anxious to have it removed.

Previous to the operation, this lady was placed fully under the influence of chloric ether. The arm then being firmly seized, the adhesions at the shoulder-joint were broken up, so as to impart quite a free motion to the limb; the injury having apparently been a fracture of the socket, and possibly of the head of the humerus. The axilla by this manipulation being fully exposed, it was found that the lymphatic glands in its border were enlarged, but the disease had not extended into the axillary fossa to any depth. The entire breast and the diseased glands were carefully dissected out, so that no perceptible disease was left.

The morbid tissue, examined microscopically and otherwise, had the distinct marks of scirrhus.

The wound healed well by the first intention, and the patient recovered a very fair use of the arm. Since the operation she has removed from the city, and when last heard from her health was failing, although there was no recurrence of the cancer.

This case presents a remarkable instance of hereditary predisposition, brought into activity at a late period of life, from an accidental cause.

Case XVII. *Testicle retained in the Groin. Removal. Subsequent Death of the Patient from Internal Disease, apparently malignant.*—Feb. 1851. The patient was a gentleman, 35 years of age. The right testicle had never descended into the scrotum, but had been retained in the inguinal canal. For many years this was no inconvenience to him, but lately it had been quite tender, giving him extreme pain on the slightest pressure, which extended upwards into

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the abdomen and down the thigh. As no treatment gave relief, I was requested to perform the operation of removal.

The patient was quite delicate in health and nervous, but with no apparent malignant disease beyond what has been stated. After etherization with chloric ether, the dissection was conducted with the utmost caution, for fear of infringing on the abdominal cavity. The testicle was removed with the tunica vaginalis, which closely embraced and in some places strongly adhered to it.

On cutting into the testicle after its removal, the structure of the organ was found to be completely disorganized; the upper part of its body presenting the aspect of an unripe apple; in the lower, was a cyst filled with a jelly-like material. Under the microscope, some nucleated cells were seen.

This gentleman recovered from the operation, but died suddenly a year afterwards, having suffered for some time previously with obscure symptoms of internal disease.

In two other cases of retention of the testicle in the groin, which I have witnessed, the organ was invaded by encephaloid disease.

Case XVIII. *Encephaloid Tumour of the Forearm. Amputation. Return in the Chest a year afterwards.*—A child was brought me in 18—, about four years old, having a large elastic tumour occupying nearly two-thirds of the forearm. The tumour was large above, and gradually tapering towards the wrist-joint. It was covered with large veins, and had a most picturesque appearance. The child seemed to be in the bloom of health; it was quite fat, and the cheeks possessed a most beautiful glow. The tumour was congenital, but had increased slowly since birth. The parents were from the country, healthy, and had other children living.

I pronounced the disease at once to be malignant, and advised immediate amputation, which was agreed to by the parents, and the limb removed above the elbow. The wound healed kindly.

A year or two afterwards I was requested to attend the autopsy of this child. I then learned that, till a short time before death, it had been in good health, when suddenly it was attacked with cough, difficulty of breathing, and other symptoms of pulmonic affection. The chest gradually projected, as if yielding to some substance forcing its way out from within, and the child died as if suffocated.

On inspection of the contents of the thorax, the pleuritic cavities seemed to be filled with a mass of encephaloid disease. The lungs were almost obliterated by compression against the spine.

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The amputated arm, and a most graphic drawing of the subject of the operation, made by Mr. Johnston, are still in my possession.

CASE XIX. *Cancerous Ulcer of the Integuments of the Chest.*—Feb. 1851. "Mr. M——, 64 years old, had a small pimple appear on the breast, to the inside of the left nipple, a number of years since. Within six months this has ulcerated, and has since resisted every effort made to heal it. At present, it is of the size of a dollar, of unhealthy aspect, with raised indurated edges. The pain is extreme, being hot and lancinating in its character. Occasionally, there is a sensation of living animals creeping in it. The discharge is quite fetid. This gentleman is of full habit, and suffered from palpitation and asthma."

On account of the extreme suffering I advised its removal, which was very carefully done, the patient being under the influence of chloric ether. Every part of the disease, including a portion of healthy skin, an inch in breadth, surrounding it, was removed. The wound healed well, and the patient returned home greatly relieved by the operation. He was directed to use a bread and milk diet; to take moderate exercise; and employ the other means naturally suggested for the preservation of health.

In July following, I regret to say that the patient was again brought to me, having two large encephaloid movable masses on the borders of the axillæ. I advised against an operation, and he died in the course of the year.

CASE XX. *Cancer of the Penis.*—May 12, 1847. For the peculiar hereditary predisposition, consult the *Remarks on Cancer of the Penis*, in the General Summary accompanying this paper.

"Some months previous to the above date, this man noticed a small sore on the glans penis, which soon healed up, leaving the prepuce adherent to the glans. In the effort at withdrawal, a raw surface was made on the glans, which gave origin to the present disease.

"On examination at the M. G. Hospital, the external appearance was healthy; but on withdrawal of the prepuce, the whole corona glandis had the aspect of a raw granulating surface, having on the top a cup-like depression, three or four lines in depth, and half an inch in diameter.

"On May 15, the patient being fully etherized, about two inches of the penis, embracing all the disease, was removed by one stroke of the amputating knife. Three ligatures only were required."

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In less than a month this patient was discharged well, but died in eight months afterwards.—*Hospital Books.*

From a review of the preceding cases, and from the general impression of the results of my own practice, I feel justified in drawing the following conclusions:—

1. That, in a certain number of cases, cancerous tumours, once removed, do not again return.

2. That in a certain other number, the patient, after immunity for a longer or shorter period, has a return of disease, requiring a second operation, which sometimes proves successful.

3. That, in a great proportion of cases, the disease, after removal, returns, either in the original wound, in its neighbourhood, or in some internal organ.

4. That, in consequence of the relief from pain during the operation, afforded by anæsthetic agents, and for reasons mentioned above, it is better to remove the tumour, provided the health be not much impaired and the disease yet remains local. For, in case of return, it generally reappears in a milder form, and the patient is saved from the loathsome character it assumes when allowed to proceed unrestrained to a fatal termination.

General Summary of the Operations for Cancer at the Massachusetts General Hospital, from its establishment, in 1822, *to January,* 1852.

Breast.	Amputation.	Removal.	Not treated.	Number.	Deaths.
Scirrhus,	21	60	5	86	Amputation, 2* Removed, 3†
Encephalus,	7	5	2	14	Amputation, 1‡ Removed, 1§
Doubtful,	1	6	1?	8	Amputation, 1
Fungus hæmatodes,	1			1	
Total,	30	71	8	109	8

One patient had five operations for return of disease in cicatrices, and last wound healed.

* One of erysipelas; the other in six months of internal disease with the wound unhealed.

† One of very extensive internal disease in ten days; health poor at time of operation. Another sank in four days from the same cause.

‡ Died in two months of extensive internal disease; health poor at time of operation.

§ Died several weeks after operation; health poor at time.

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Five had operations on breast twice for return of disease in cicatrix.

Four of those not treated were cases where the disease had returned in cicatrices of former operations.

Twelve were known to have had a return of disease in the cicatrix within a short time.

Tongue.—Removal 11, not treated 1; total 12. Wound healed 9, wound unhealed 2, death 1; total 12. Reappearance 1.

Lip.—Removal 9; all well. In one, the whole disease was not removed till a second operation.

Face and Cheek.—Removal 9, not treated 1; total 10. Wound healed 7, disease returned 2; total 9.

Penis.—The case of amputation of penis for cancer, reported in the *Surgical Records of the Massachusetts General Hospital*, vol. xxxviii. p. 234, died eight months afterwards. This man's father, grandfather, and great-grandfather, all died of cancer of penis.

There have been eight cases of cancer of penis in the wards of the Massachusetts General Hospital.

Congenital phymosis existed in two cases.

In one case, phymosis existed at time of entrance, but when it commenced is not stated.

Phymosis came on after disease commenced in one case.

In one case, it did not exist.

Nothing relating to the subject is stated in three cases.

Of four cases of cancer of the penis which I now call to mind, in private practice, two had congenital phymosis. The fact in regard to the other two I cannot at present remember, or discover the notes of the cases.

One case had the cancer limited to the prepuce, although it so nearly infringed on the gland that it was necessary to attack it at that point.

The operation was performed in May, 1849. I heard from the patient one or two years after, and there was no return of the disease at that time.

Amputation.—Healed 6, unknown 1; total 7. Known to have returned in the groin 2.

Testis.—Removal 5. One died in four weeks, of great internal disease; rest did well.

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In Groin.—Removal 2, not treated 1; total 3. Wound healed 1, unhealed 1;* total 2.

Antrum.—Removal of disease 2,† both unhealed, removal of upper jaw 3; total 5. Well 2; death 1, of erysipelas, in nine days.

In Neck.—Removal 5. Well 3, much relieved 1, died 1; total 5.

In Axilla.—After removal of breasts 3. Unknown 1, recovered 1, death, wound unhealed 1; total 3.

Arm.—Amputations 3, did well, not treated 1; total 4.

Leg.—Amputation of thigh 5, 2 cured, 3 died;‡ amputation of leg 2, both cured; total 7.

On scalp. Removed 1; well.

From dura mater. Cut off; then cauterized; wound healed 1.

Walls of abdomen above umbilicus. Removed 1; died in 17 days of great internal disease; operation performed to relieve him of great pain of tumour.

Labia and nymphæ. Removed 1; well.

Eye. Organ extirpated 1; well.

Thigh (Gelatiniform?) Dissected out 1; well.

Of parotid. Dissected and cauterized 1; well.

Total number of cases, 186; of which 12 were not treated.

* Following cancer penis.

† In one, the disease returned in glands of neck before healing.

‡ 1 died in two weeks; 1 from cancer in groin.

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Recorded Operations for Cancer in the Hospital

	No.	Sex.	Age.	Date.	Situation.	Variety.	Duration.
Private	1	F.	50	1840	Breast	Scirrhus	2 years
"	2	M.	41	1844	Right nipple	"	10 years
"	3	F.	68	1846	Right breast	Encephaloid	18 months
"	4	F.		"	Jaw	"	
"	5	F.	42	"	Right breast	Scirrhus	2 years
Hospital	6	F.	28	"	"	"	5 months
"	7	F.	76	"	"	"	20 years
Private	8	F.	45	"	"	"	2½ years
"	9	M.	50	"	Testicle	Encephaloid	
"	10	F.	52	"	Right breast	Scirrhus	2½ years
"	11	M.	64	"	Ala nasi	"	
Hospital	12	F.	28	"	Parotid	"	8 years
"	13	F.	39	"	Breast	"	9 months
"	14	M.	67	"	Penis	"	4 months
"	15	F.	58	"	Submaxillary gland	"	5 months
"	16	F.	36	"	Upper jaw	Encephaloid	3 months
"	17	F.	48	"	Breast	Scirrhus	2½ years
"	18	M.	38	"	Lip	"	2 years
"	19	F.	41	1848	Breast	"	5 months
"	20	F.	46	"	"	Encephaloid	11 months
Private	21	F.	48	"	Left breast	Scirrhus	1 year
"	22	F.	40	"	Navel	"	1 year
"	23	F.	60	"	Integuments of chest	"	15 years
"	24	M.	50	"	Mouth	"	9 years
Hospital	25	F.	42	"	Left breast	"	1 year
Private	26	F.	40	"	Right breast	"	10 years
"	27	F.	35	"	Left breast	"	6 months
Hospital	28	M.	54	"	Parotid	"	9 months
Private	29	F.	33	1849	Right breast	"	2 years
"	30	M.	40	"	Palm of hand	Fungoid	1 year
"	31	M.	7	"	Ear	"	4 months
Hospital	32	F.	52	"	Breast	Scirrhus	2 years
"	33	M.	27	"	Femur	Encephaloid	9 months
"	34	M.	54	"	Face	"	10 months
"	35	M.	71	"	Eyelid	Scirrhus	4 years
"	36	F.	37	"	Breast	"	2 years
"	37	F.	52	"	"	"	2 years
"	38	F.	32	"	"	"	6 months
"	39	M.	15	"	Lower jaw	Encephaloid	2½ years
"	40	M.	52	"	Tibia	Scirrhus	23 years
"	41	F.	24	"	Cicatrix on thigh	Fungoid	22 years
Private	42	F.	50	1850	Right breast	Scirrhus	2 years
"	43	M.	64	"	Integuments of chest	"	Many y'rs
"	44	F.	60	"	Border Axilla	"	4 months
"	45	F.	56	"	Right breast	"	4 years
"	46	F.	60	"	Upper jaw	Encephaloid	6 months
"	47	M.		"	Lip	Scirrhus	
Hospital	48	F.	24	"	Side	Melanosis	8 months
Private	49	F.	45	"	Right breast	Scirrhus	2 years

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and Private Practice of Dr. J. MASON WARREN.

Operation.	Immediate result.	REMARKS.
Excision	Well	Now well, 1852. (See Case I.)
"	"	Partially removed by a cancer doctoress, with caustic, six years previous.
"	"	Now living, March, 1852.
"	"	
"	"	Seen March 10, 1852. (See Case XIII.)
"	"	Breast had always been sore after nursing.
"	"	Seen December, 1851; well. Since had small lump in cicatrix. (See Case XV.)
"	"	
"	"	Returned in groin. Died in the course of a year.
"	"	
"	"	Ala had been destroyed; was restored from cheek by autoplasty.
Removed	"	Hereditary malignant tendency. One removal of tumour four years previous; another, two years previous.
"	"	
Amputation	"	(See Case XX.)
Removed	"	
Extirpation	Relieved	Reappeared in a month, with wound unhealed, and large, hard glands under angle of jaw.
Excision	Well	Seen six months after; return of disease in cicatrix, ulceration, scirrhous glands in axilla; cough.
"	"	
"	"	
Caustic applied	Not relieved	Excision six months previous; return two months previous; applied too late for a second operation.
Excision	Well	Nipple retracted, and bloody discharge previous to operation.
"	"	Died six months or a year after of cancer of rectum.
"	"	
"	"	
"	"	Now well, March, 1852.
"	"	
"	"	Returned three weeks after. Died.
"	"	Had cancer of face removed by me six years previous. Died since.
"	"	Now well, March, 1852.
"	"	
"	"	Returned. Died.
"	Much relieved	
Amputation	Relieved	Died some months after with pulmonary symptoms.
Excision	"	
"	Well	
"	"	
"	"	
"	Death	From accidental erysipelas and peritonitis.
"	Well	(See Case III.)
Amputation	"	(See Case XI.)
"	Dead	Very weak before operation; sank rapidly; no trace of malignant deposit on autopsy.
Excision	Well	Returned in axilla. Died one year after.
"	"	Encephaloid in both axillæ one year after. Dead.
"	"	Died one year after with typhoid symptoms.
"	"	Remained well two years. Died of internal disease.
"	"	Now well, 1852.
"	"	
"	"	
"	"	Well until 1852; then return in axilla.

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Recorded Operations for

	No.	Sex.	Age.	Date.	Situation.	Variety.	Duration.
Private	50	M.	30	1850	Thigh	Encephaloid	2½ years
"	51	M.	68	"	Lip	Scirrhus	1 year
"	52	F.	68	"	Right breast	Scirrhus and encephaloid	1 year
"	53	F.	60	"	Ala nasi	Scirrhus	
"	54	F.	38	"	Right breast	"	1 year
"	55	M.	30	1851	Arm	Encephaloid	6 months
"	56	F.	54	"	Left breast	Scirrhus	14 months
"	57	M.	54	"	Ala nasi	"	14 months
"	58	F.	64	1st 1841 2d 1851	Right breast	"	1 year
"	59	M.	35	1851	Testicle	Encephaloid	
"	60	F.	49	"	Left breast	Scirrhus	2 years
"	61	M.	35	"	Ankle	Encephaloid	3 years
"	62	F.	60	"	Left breast	Scirrhus	6 months
"	63	F.	60	"	Right breast	"	6 months
Hospital	64	F.	62	"	Neck	Encephaloid	20 years
Private	65	M.	84	"	Parotid	Fungoid	6 months
"	66	F.	60	"	Right breast	Scirrhus	2 years
"	67	F.	52	"	Left breast	"	6 months
"	68	F.	50	"	"	Encephaloid	6 months
"	69	F.	35	"	Right breast	Scirrhus	9 months
"	70	F.	50	"	"	"	9 years
"	71	M.	48	"	Lower lip	"	3 years
"	72	M.	39	1852	Back	"	3 months
"	73	M.	70	"	Face	"	1 year
"	74	F.	44	"	Right breast	"	4 years
Hospital	75	F.	50	"	"	"	1 year
"	76	M.	55	"	Parotid	"	25 years

No. 33 had been in the hospital three months at a previous date, for disease of the knee, and was discharged relieved. After the operation, some pulmonary disease developed itself, and when the stump had partially healed, he was transferred to the Medical Department. His disease was thought to have been phthisis, but no autopsy was allowed.

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Cancer—Continued.

Operation.	Immediate result.	REMARKS.
Excision	Recovered	Returned; reported amputation of thigh by quack; death on spot.
"	Well	Now well, 1852.
"	Recovered	Operation by request of friends. Disease returned, and she died.
"	"	I removed cancer from her nose five years previous. Returned past year.
"	Died	Erysipelas.
Amputation	Recovered	Amputation at shoulder-joint. (See Case XII.)
Excision	Well	
"	"	
"	"	First incision 1841; return 1850; second excision 1851. (See Case XIV.)
"	Recovered	Died the year after with anomalous symptoms. (See Case XII.)
"	Died	From erysipelas.
Amputation of thigh	Recovered	Died with pulmonary symptoms some months after.
Excision	Well	Milk abscess thirty years previous. Now well, 1852.
"	Recovered	Died after of internal disease. (See Case XVI.)
"	Well	Now well. 1852.
"	"	Returned; bled freely from tumour before operation; much relieved by operation.
"	"	Now well, 1852.
"	"	Believed to be well, 1852.
"	"	Now well, 1852.
"	"	Now well, 1852.
"	"	Had a cancer removed from left breast twenty-five years ago. (See Case X.)
"	Recovered	Had been three or four times operated on. Returned 1852.
"	Well	Now well, 1852.
"	"	
"	"	
"	"	Had been operated on by cancer doctor during seven or eight months.
Removed	Doing well	

BREAST.

	Scirrhus.	Enceph.	Total.
Right*	19	1	20
Left	7	2	9†
Not given	8		8
Total	34	3	37

Deaths 8, only 3 of which were immediate.

	Scirrhus.	Enceph.	Total.
Parotid‡	2	1	3
Face	1	1	2
Ear		1	1
Lip	4		4
Eyelid	1		1
Nose	2		2
Mouth	1		1
Upper jaw		2	2
Lower jaw	2	1	3
Neck		1	1
Back	1		1

	Scirrhus.	Enceph.	Total.
Axilla	1		1
Arm		1	1
Palm of hand		1	1
Integuments of chest	2		2
Penis	1		1
Testicle		2	2
Navel	1		1
Femur		2	2
Tibia	1		1
Ankle		1	1

* 1 case of right breast, of scirrhous and encephaloid character, is included in the deaths.

† Disease is known to have returned in 7.

‡ Melanosis of the side was operated on *once*, and a cicatrix (enceph.).

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§ 2. TABLE OF RESULTS OF SURGICAL

By FRANK H. HAMILTON, M. D.,

CARCINOMA.

CARCINOMA OF

No.	Original seat of disease.	Age when it commenced.	Age when operated on.	Sex.	Temperament.	Residence.	Occupation.	Cause.
1	Mamma.	38	45	F.	Nervous.	Wayne Co., N. Y.		Been married twice, but never conceived.
2	Mamma.	40	47	F.	Nervous.	Erie Co., N.Y.		Married several years but never conceived.
3	Mamma.	41	43	F.	Lymphatic.	Monroe Co., N. Y.	Seamstress.	Married, and has children.
4	Mamma.	49	54	F.	Bilious.	Cayuga Co., N. Y.		Mother had cancer.
5	Mamma.	35 or 40	48	F.	Nervo-bilious.	Wyoming Co., N. Y.		
6	Mamma.	35	39	F.		Seneca Co., N. Y.		
7	Mamma.	45	65	F.	Sanguineous.	Windsor, Vt.		
8	Mamma.	59	61	F.		Niagara Co., N. Y.		
9	Mamma.		43	F.	Nervous.	Cayuga Co., N. Y.		
10	Mamma.	25	30	F.	Sanguineous.	New York.		Not married.
11	Mamma.	22	25	F.	Lymphatic.	Cattaraugus Co., N. Y.		
12	Mamma.	49	56	M.		New York.		Has unusually large breasts.
13	Testis.	31	42	M.	Nervous.	Erie Co., N.Y.	Merchant.	Mechanical injury.
14	Lymphatic gland under angle of jaw.	20	21	M.	Lymphatic.	Canada West.		
15	Lymphatic gland, or subcutaneous cellular texture in parotideal region.	19	22	F.	Lymphatic.	Erie Co., N.Y.	Housemaid.	Mechanical injury.

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OPERATIONS IN MALIGNANT DISEASES.

Professor of Surgery in the University of Buffalo.

GLANDULAR STRUCTURES.

Progress: state of part and of patient.	Mode of operation.	How long remained cured.	How soon returned.	How soon died.	Examination of product.
Commenced on outer verge of gland; whole breast involved and part of the pectoral muscle; small ulcerations at two points; health tolerable.	Knife; removed the whole gland and part of pect. m.		Immediately.	6 months of same disease.	
Involves two-thirds of gland; not ulcerated; health good.	Knife.	6 years.			Heteromorphous; some portions hard and caseous; other portions composed of large cells, containing variously-coloured matter.
Involves most of the breast; not ulcerated; health good.	Knife.	2 years.			Heteromorphous; irregular and hard.
Involves whole breast; not ulcerated; health tolerable.	Knife.	2 years.			Consistence of a raw potato.
Involves half of breast; not ulcerated.	Knife.		1 year.	2 years of same disease.	Structure quite hard; white and yellowish-white.
Involves most of breast; health poor; coughs; not ulcerated.	Knife.	3 years.		4 years of same disease.	
Whole gland involved; not ulcerated; two glands in axilla enlarged; healthy.	Knife; removed axillary gland also.	Never heard of her since.			Hard; crispy; some small cysts.
Size of a hen's egg; nipple much retracted; not ulcerated.	Knife.	2 years.			
Half of breast involved; not ulcerated; health tolerable.	Knife.	10 years.			Hard.
Size of a small hen's egg; irregular; hard; not ulcerated; near axillary verge of gland; healthy.	Knife; removed the tumour only	8 years.			Structure firm; section crispy. (I am not certain that it was scirrhus, but think it was.)
Size of a small hen's egg; not ulcerated.	Caustic by an empiric.		Immediately.	6 months.	
Size of a large nut; one inch below the nipple; not ulcerated.	Caustic by a surgeon.		Immediately, & 1 year after it was very large.		
Testis slightly enlarged; indurated; lobular; painful; not ulcerated; chord slightly enlarged; health bad.	Knife.	6 months.			Firm; cuts like cartilage; an albuminous fluid easily expressed.
One inch in diameter; hard; skin adherent; painful; not ulcerated.	Knife.				Structure hard; opaque; cut leaves a glossy surface.
Involved the whole parotideal region; hard; knobby; painful; of late growing rapidly; health good.	Knife.	2 years.			Structure hard; colour grayish-white; glossy.

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CARCINOMA OF

No.	Original seat of disease.	Age when it commenced.	Age when operated on.	Sex.	Temperament.	Residence.	Occupation.	Cause.
16	Under lip.	52	60 64 64	M.	Sanguineous.	Wyoming Co., N. Y.	Farmer.	
17	Under lip.	41	42 43	M.	Sanguineous.	Simcoe, C. W.	Farmer.	Originated from a small sore which remained after a fever.
18	Under lip.	40	40 56 57	M.	Sanguineous.	Chatauque Co., N. Y.	Farmer.	Smokes with a short pipe.
19	Under lip.	53	55	M.		Erie Co., N.Y.	Labourer.	
20	Under lip.	53	55 55 56	M.	Nervo-sanguineous.	Erie Co., N.Y.	Gardener.	
21	Commissure of lips.	36	38	M.	Sanguineous.	Monroe Co., N. Y.	Merchant.	
22	Side of tongue.	52	53	M.		Niagara Co., N. Y.	Labourer.	
23	End of tongue.	44	46	F.	Nervous.	Erie Co., N.Y.		
24	Nares.	28	52	M.	Bilious.	Erie Co., N.Y.	Farmer.	

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MUCOUS STRUCTURES.

Progress: state of part and of patient	Mode of operation.	How long remained cured.	How soon returned.	How soon died.	Examination of product.
Began as an ulcer, covered with a hard crust; the ulcer was destroyed by caustic; four years after this, it involved the whole right side of the lip, and I cut it out and closed the wound by anoplasty; it was removed by caustic; health generally good.	Caustic.		2 months.		
	Knife with anoplasty.		4 months.		
	Caustic.		Immediately.		
When the ulcer had remained a year, caustic was applied. The ulcer returned, and after 18 months was cut out. Again it returned, involving now the submaxillary gland, and after two years the gland also was cut away; the wound never healed, and the bone is at last diseased also.	Caustic.		Immediately.		
	Knife.		1 month.		
	Knife.		Immediately.		
Began as a wart; destroyed by actual cautery; returned, and when it had remained as an ulcer 16 years, potential cauteries were applied; the ulcer now spread rapidly, and I cut it out.	Actual cautery.		1 year.		
	Potential cautery.		1 month.		
	Knife.	5 years.			
Began as a small hard tumour, and when about the size of a hazel-nut it was cauterized; now it is a large, irregular, malignant ulcer.	Caustic.		Immediately.		
Began as a vesicle; when it opened there was left an ulcer with a hard base; caustic was applied; it returned, and the knife was used; again it returned, and caustic was used; health not good.	Caustic.		Immediately.		
	Knife.		2 months		
	Caustic.	1 year.			
Began as a fissure in the skin; became an indurated ulcer; caustic was applied; it became worse; and I removed it with a knife, and closed it by anoplasty; health bad.	Caustic.		Immediately.		
	Knife and anoplasty.				
Began as a fissure in side of tongue; gradually enlarged until an empiric applied caustic; then became much larger; now ulcer is two inches in diameter; base of edges indurated; painful, &c.	Caustic.		Immediately.		
Began as a small hard lump; when I operated it was open, and the ulcer about one inch in diameter; health bad.	Knife.		1 year.	2 years after, of same disease.	
Commenced as an ulcer in nose; right ala destroyed, and inferior turbinated bone gone; health poor.	Knife and anoplasty.	6 months.		2 years, of same disease.	

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CARCINOMA OF MUCOUS

No.	Original seat of disease.	Age when it commenced.	Age when operated on.	Sex.	Temperament,	Residence.	Occupation.	Cause.
25	Rectum.	33	31	M.		Erie Co., N.Y.	Mechanic.	Piles.
26	Rectum.	43	44	M.	Nervous.	Monroe Co., N. Y.	Labourer.	
27	Glans penis.	59	60	M.	Nervous.	Niagara Co., N. Y.	Labourer.	Mechanical injury.
28	Glans penis.	53	55	M.	Nervous.	Erie Co., N.Y.	Nurse.	Congenital phymosis.
29	Glans penis.	44	45	M.	Nervous.	Cattaraugus Co., N. Y.	Labourer.	

CARCINOMA OF

30	Skin of finger.	36	49 50	M.	Nervo-sanguineous.	Chemung Co,, N. Y.	Farmer.	Wart.
31	Skin below outer angle of eyelids.	45	62	M.	Sanguineous.	Genesee Co., N. Y.	Brick-mason.	
32	Skin of nose.	40	45	M.	Sanguineous.	Woodstock, Vt.		
33	Skin of face below eyelid.	45	45	M.	Melancholic.	Cayuga Co., N. Y.		
34	Skin of thumb.	75	77	M.	Sanguineous.	Niagara Co., N. Y.		Hereditary.
35	Skin on back of hand.	57	58 59 60	M.		Monroe Co., N. Y.		A brother, two years older, died of an ulcerated tumour outside of neck.
36	Skin over parotid region.			M.	Sanguineous.	Seneca Co., N. Y.	Farmer.	
37	Skin of face below outer angle of eye.	50	62	M.		Erie Co., N.Y.	Mechanic.	

CARCINOMA OF

38	Subcutaneous cellular texture of face, or lymphatic glands.	42	48 49	M.	Nervous.	Erie Co., N.Y.		

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STRUCTURES—*Continued.*

Progress: state of part and of patient.	Mode of operation.	How long remained cured.	How soon returned.	How soon died.	Examination of product.
Commenced as bleeding piles, and about four months before he was operated on it assumed a cancerous character; the caustic was applied by an empiric, and it immediately extended upward beyond the reach of the knife.	Caustic.		Immediately.	4 months.	
Commenced as a "lump;" when operated upon it was an inch in diameter; irregular, hard, painful, ulcerated; health bad.	Knife.	2 years.			Structure hard, firm; cut surface smooth and glossy.
Bruised his penis about a year since; whole glans and a part of prepuce involved; ulcerated; health bad.	Knife; amputation.	1 year.			Structure opaque and hard; also soft and granular, or vegetating.
Whole glans involved, and part of prepuce; health tolerable.	Knife; amputation.	2 years.			Structure chiefly spongy and granular, being covered with minute vegetations.
Glans and part of body involved; ulcerated; very feeble.	Knife; amputation.	6 months.			Structure hard; fibro-cartilaginous; warty.
INTEGUMENTARY STRUCTURES.					
During 14 years the wart presented nothing unusual; he then tied it off, and the disease within one year involved the whole finger with its bones; health good.	Ligature.		Immediately.	He died finally of erysipelas.	Bone carious; soft parts heteromorphous.
	Knife; amputation.	10 years.			
Commenced as a "ringworm;" caustic was applied a short time since; now a large open ulcer.	Caustic.		Immediately.		
Began as a small crust; in five years it had destroyed half an inch of end of nose; healthy.	Knife.	1 year.			
Half an inch in diameter.	Knife.	2 years.			
Began as a wart; twice fell off and healed spontaneously; now one inch in diameter; indurated ulceration.	Caustic.	2 years.			
When first removed was like a wart; when removed by caustic was the size of a two-shilling piece.	Ligature.		Immediately.		Structure when amputation was made hard at points; also soft and pulpy.
	Caustic.		Immediately.		
	Amputation		Immediately.		
Began as a small crust; now one inch in diameter; health good; involves lobe of ear.	Knife and anoplasty.		Immediately.	6 months.	
Began as a crust; now three inches in diameter; malar bone affected; health poor.	Knife, chisel, and anoplasty.	6 months.			
CELLULAR TEXTURE.					
Began as a small movable tumour under ramus of jaw near its middle; when it had existed six years, an empiric applied a caustic; the wound healed slowly, and the bone became involved; health at time of second operation bad.	Caustic.		Immediately.		Structure of bone soft, spongy, and cerebriform.
	Knife and saw. I removed more than half of inf. max.			1 month.	Soft parts indurated and heteromorphous.

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CARCINOMA

No.	Original seat of disease.	Age when it commenced.	Age when operated on.	Sex.	Temperament.	Residence.	Occupation.	Cause.
39	Humerus.	41	41	M.	Bilious.	Erie Co., N.Y.	Labourer.	

COLLOID.

40	Glands, or cellular structure of groin.	45	48	M.	Bilious.	Erie Co., N.Y.	Tavern-keeper.	Tumour.
41	Subcutaneous cellular texture of labia.		25	F.	Phlegmatic.	Chatauque Co., N. Y.		
42	Inner surface of uterus.	42 or 43	44	F.	Nervous.	Chatauque Co., N. Y.		

ENCEPHALOID.

43	Mucous membrane of nares.	4	5	F.	Nervous.	Orleans Co., N. Y.		She has a scrofulous appearance.
44	Upper eyelid.	39		M.		Ohio.	Physician.	
45	Upper eyelid.	41	45	M.	Sanguineous.	Erie Co., N.Y.	Steamboat-runner.	
46	Skin over parotid region.	31	45	M.	Bilious.	Monroe Co., N. Y.		
47	Cellular structure under tendo-achillis.	55	56	M.	Sanguineous.	Orleans Co., N. Y.	Merchant.	Probably a sprain.
48	Subcutaneous cellular structure near ankle-joint.	20	22	F.	Nervous.	Cayuga Co., N. Y.		

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OF BONE.

Progress: state of part and of patient.	Mode of operation.	How long remained cured.	How soon returned.	How soon died.	Examination of product.
Involved whole of upper arm and part of shoulder; ulcerated; bone necrosed; health bad.	Knife; amputation at shoulder-joint.		Never healed.		Structure opaque; white; solid; heteromorphous.
Tumour grew slowly at first, but rapidly during the last year; now about ten inches in diameter; health impaired.	Knife.	1 year.			Tumour weighed 8¼ pounds; structure gelatiniform; colour various shades of green and yellow, the green predominating.
Tumour large and pyriform; has a firm, elastic feel; been growing slowly.	Knife.		6 months.		Tumour weighed near two pounds; structure fibrous and partly gelatiniform; colour yellowish and greenish.
Had leucorrhœa six years; irregular menstruation five years; health very poor; has a large polypus projecting from vulva; pyriform, firm, elastic.	Ligature. Ligature.		3 months. Immediately.	4 months.	First tumour weighed 1½ pounds; structure caseous, fibrous, and gelatiniform.
Commenced in Feb., 184–, as a polypus; four months after it was removed by the forceps; in one year it again filled the nose, and dislocated the right eye; health poor.	Evulsion.		Immediately.		
Began as a small bright-red tumour on under side of upper lid; cut it out four times; last time it did not heal; whole upper lid now involved; tumour size of a hen's egg; from the first appearance of the tumour until he died was not more than three years.	Knife. Knife. Knife. Knife.		Soon. Soon. Soon. Immediately.	 1 year.	
Large tumour, with an open bleeding surface; health poor.	Knife.		Immediately.	6 months.	Structure entirely heteromorphous; in part cerebriform, and in part firm, opaque, and occasionally spongy.
Tumour six or eight inches in diameter; ulcerated and bleeding; health poor.	Ligation of common carotid and caustic.		Immediately.	6 months.	
In one year it had attained the size of two fists; did not ulcerate; health poor.	Knife; amputation.		8 weeks in stump.	5 months.	Structure cerebriform; muscles softened and disorganized.
Involved most of outside of ankle-joint.	Knife.		Immediately.	1 month.	Structure nearly resembling a mammary gland, but when it returned it became spongoid.

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ENCEPHALOID—

No.	Original seat of disease.	Age when it commenced.	Age when operated on.	Sex.	Temperament.	Residence.	Occupation.	Cause.
49	Alveoli of superior maxilla.	30	34	F.	Nervous.	Niagara Co., N. Y.		
50	Alveoli of inferior maxilla.	34	37 40	M.		Herkimer Co., N. Y.	Farmer.	
51	Superior maxilla, or malar bone.	8	4	M.		Seneca Co., N. Y.		
52	Probably the humerus near the shoulder-joint.	40		M.	Bilious.	Erie Co., N.Y.	Labourer.	
53	Probably the radius and ulna near the wrist.	38	35 36 40	M.	Sanguineous.	Cayuga Co., N. Y.	Merchant.	Fracture.
54	Tibia.	40	40	M.		Cayuga Co., N. Y.	Labourer.	

MELANOSIS.

55	Subcutaneous cellular texture and skin.	Congenital.	16 17	M.	Sanguineo-bilious.	Erie Co., N.Y.	Labourer.	Complexion of family very dark; at birth he had melanotic tubercles on back of neck.
56	Skin of shoulder.		25	M.	Phlegmatic.	Livingston Co., N. Y.	Merchant.	Scrofulous.
57	Lymphatic gland of neck.	34	41	M.	Sanguineous.	Erie Co., N.Y.	Farmer.	Mechanical injury.
58	Outer margin of iris.	22	34 45 46	M.		Livingston Co., N. Y.	Labourer.	
59	Lymphatic glands of groin.	20	22 28 28	F.				

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Continued.

Progress: state of part and of patient.	Mode of operation.	How long remained cured.	How soon returned.	How soon died.	Examination of product.
Has been cut out twice; the third time it had attained the size of a hazel-nut; it was very painful and tender; I removed a large portion of sound bone; opening into the antrum.	Knife. Knife. Knife and chisel.		1 year. 1 year.		Structure spongoid.
Was first cut away with a knife; returning, I removed it, when about the size of a pullet's egg, with a chisel and knife; health good.	Knife. Chisel and knife.		2 years.		Structure at second operation bony and spongy, or erectile.
Began six months before the operation was made; then occupied most of the parotid region; not ulcerated; health bad.	Knife and chisel.		Immediately.	9 weeks.	Structure cerebriform, cystic and bony; bone carious.
Involved most of upper arm; open and sloughing; health bad.	Knife; amputation at shoulder-joint by Dr. Winnie.			1 year.	
Began one year after a fracture of radius and ulna, near the seat of fracture; in two years the tumour involved most of palmar surface of forearm; health poor.	Knife. Caustic. Knife.	10 years.	1 year. 2 years.		Structure, the first time it was removed, was cerebriform and spongy.
Size of a large orange; ulcerated; bleeding; growing rapidly; bone carious.	Knife; amputation above the knee.	2 years.			Structure spongoid, pulpy, and bony; muscles of back of leg soft; a small tumour in ham.
The tubercles on back of neck began to increase about a year since; I cut them out; they soon returned, and an empiric employed caustic; he died soon after.	Knife. Caustic.		2 months. Immediately.	6 months.	Soft; nearly gelatiniform; colour dark-brown.
Tumour been growing several years; now size of an orange; health good.	Knife.	2 years.			Structure firm, fibrous; colour mottled, black and white.
Bruised his shoulder seven years ago; always been tender; grew mostly within a month; size of a hickory-nut.	Knife.	3 years.			Structure firm; cut smooth and glossy; colour dark-blue.
First seen at junction of cornea with sclerotica; when size of a hazel-nut, was cut off twelve years after it commenced; in eleven years it had attained same size, and was cut off again; in one year more it was larger than before, and I extirpated the eye & lachrymal-gland; health good.	Knife. Knife. Knife; extirpation of eye, &c.	4 years.	1 year. 1 month.		Structure almost entirely a black pigment, inclosed within a thin cyst, and intercepted by a few vascular bands.
It was first cut out when but two or three glands were involved; then when six or more were involved; the last operation was made since I saw her, and she died soon after.	Knife. Knife. Knife.			6 months.	

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§ 3. FACTS AND OBSERVATIONS CONTRIBUTED BY DIFFERENT SURGEONS AND GLEANED FROM AMERICAN AND EUROPEAN AUTHORS.

In a letter which I received, in March, 1852, from Professor Paul F. Eve, of Georgia, this gentleman informs me that he had, within the last eighteen months, operated upon twelve cases of cancerous tumours, in every one of which the disease had returned. He thinks that surgery promises very little in this affection. "It might do more," he says, "could patients be brought to submit to the knife at the earliest possible period after the disease is known to be cancerous." Dr. Eve recognizes the distinction between cancerous and cancroid affections, and insists upon a bread and milk diet after extirpation, though none of his patients, it would seem, have ever fully carried it out.

Of the various forms of soft cancer, Dr. Eve has never met with an instance, whether operated on or not, in which the disease did not end fatally, and that, too, more speedily than in scirrhus. His experience confirms the opinion that the younger the patient is the more rapid will be the malady, and the sooner will it occasion death.

Dr. Eve gives a case of cancer of the skin, for which he removed a large portion of the right cheek, with a part of the ear; the patient was a man aged seventy-five, and there was no repullulation at the end of ten years.

Dr. George W. Norris, of Philadelphia, speaks discouragingly of operations for the removal of malignant tumours. In his letter to me, dated November, 1852, he says: "But few, I take it, at this day, who have had an opportunity of following their patients, will pretend to say that these growths do not almost invariably return." Dr. Norris has carefully examined the records of the Pennsylvania Hospital, of which he is one of the surgeons, with regard to the subject under consideration; but he finds that they furnish no useful results of any kind. Indeed, it would seem that, for many years past, all the surgeons who have been connected with that institution have been so averse to the employment of the knife in malignant disease that but very few such cases have been admitted into its wards for operation.

The testimony of Professor Pope, of St. Louis, is of the same import. "As a result," he remarks, "of my operations in cases of true cancer which I have been able to follow, I can say with Broca that "it never forgives." In all, without exception, the disease has

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returned within from five weeks to twelve months after extirpation, and every patient has, in consequence, died sooner or later from its effects. The very reverse has been the case with the carefully removed cancroid affections, in whatever part of the body situated."*

Professor Hamilton, of Buffalo, who has paid much attention to the subject of malignant diseases, and whose interesting table accompanies this report, thinks himself authorized to conclude, first, that in glandular scirrhous carcinoma an operation should be encouraged at almost every period prior to the commencement of ulceration; the earlier, however, it is performed, the better will be the prospect of success. Secondly, that the knife should be used with reluctance after the tumour has resumed the character of an open sore. Thirdly, that the whole gland should always be removed, however limited the disease. Fourthly, that in tegumentary cancer, even when the ulceration is pretty extensive, excision, especially with the aid of anaplasty, will frequently effect a radical cure. And, fifthly, that in no case are caustics preferable to the knife.

In May, 1850, Professor Warren, of Boston, presented to this Association, at their meeting in Cincinnati, an account of eight cases of malignant disease, which he had relieved, if not permanently cured, by excision.† Of these cases, four were scirrhous, and the remainder cephalomatous. Those belonging to the first category all involved the mammary gland, and were so well marked in their character, both as it respects the symptoms and the morbid structure, as to leave no reasonable doubt in the mind of the operator as to their carcinomatous nature. The ages of the patients were, respectively, fifty-five, fifty-four, forty, and fifty-five.

The tumour, in the first case, had been discovered two years previously, and was, at the time of the operation, of the size of an egg, very hard, and movable on the pectoral muscle, without any discoloration of the skin. The entire breast was removed, along with the nipple, the condition of which is not mentioned. The patient had enjoyed good health up to the time of the operation, and had been the mother of a number of children. She finally died, at the age of eighty-eight, of peripneumonia, at the end of thirty-three years after the operation, and without any return of her disease.

In the second case, the patient, an unmarried woman, was fifty-four years of age, of very large stature, athletic, and of florid complexion.

* Letter to the Chairman of the Committee, July, 1851.

† Transactions, vol. iii. p. 895, 1850.

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The tumour is said to have been a genuine scirrhus, and was attended with enlargement of the axillary ganglions. She survived the operation more than twenty years, without any relapse, and at length died of disease of the heart with effusion into the thorax.

In the third case, the patient, aged forty, lived nearly nineteen years after the extirpation, when she died of some other malady. The tumour, which had a scirrhous hardness, was of the size of an egg, painful, but not tender, and movable on the pectoral muscle. The operation included only the outer half of the breast, the nipple, with the remainder of the organ, being left intact.

A lady, fifty-five years of age, had a tumour in the right breast, about the size of an egg, hard, irregular in form, somewhat tender, and occasionally painful. It had been caused, as was supposed, by a fall on the part the previous summer. The tumour was fixed, but the gland itself was movable on the pectoral muscle. The affected mass, together with a portion of the mamma, was removed with the knife, and the woman died twenty-two years afterwards, without any return of cancerous disease.

In the other division, also embracing four cases, the malady was of a cephalomatous character, and occupied various regions of the body.

In the first case, the patient, a healthy man, aged about thirty, had been affected a number of years with a tumour, of the size of an orange, situated on the right side of the neck in the region of the parotid gland. It was of a red colour, firm and elastic to the touch, and ulcerated on the surface. The wound healed in about a month, and the patient, when last seen by Dr. Warren, ten years after the operation, was perfectly well.

A stout, healthy man, aged thirty, of good habits, had a red, soft, bleeding tumour in the left nostril, emitting a most offensive smell, and encroaching upon the roof of the mouth and the corresponding nasal cavity. It had existed about four months, and had been the seat of frequent, and, at times, copious hemorrhages. The morbid mass was excised along with the floor of the orbit and the left side of the palate. The man continued well at the end of nine years after the operation. A portion of the tumour, examined after having been sometime in alcohol, exhibited caudate and fusiform cells, similar to those observed in encephaloid.

The third case was that of a boy, aged five years. The tumour, of the form and size of a lemon, was situated in the groin, and ex-

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hibited a distinct cerebriform texture. The operation was performed in 1843, and the boy was perfectly well in 1850.

In the fourth case, the tumour involved the dura mater, and was of well-marked encephaloid character. At the time of the operation, in 1847, it had burst through the skin, and was about the volume of an egg, of a red colour, without pain, and easily disposed to bleed. A probe penetrated the substance of the tumour to the depth of three inches. The hemorrhage was great, but was suppressed by several ligatures and the actual cautery. Several months elapsed before the wound was entirely closed; but it had remained well at the time the case was reported in 1850. The patient was a female, twenty-two years of age.

I have thus given an outline of these cases, because, first, they have been reported by a gentleman of great experience and intelligence; and, secondly, because their result is in direct contradiction to that of the generality of surgeons of the present day. The account of them is, in many respects, defective, and hence much of their value is necessarily lost. Thus, in at least two of the cases, constituting the first group, no mention is made of the state of the nipple or skin, and in none is there any satisfactory description of the morbid structure. The details of the other cases are given in a more satisfactory manner, though not as minutely as could have been desired. In one of these, a section of the tumour was examined microscopically, but as it had been immersed for a considerable length of time in alcohol, no legitimate deduction can be drawn from the appearance of the caudate and fusiform bodies revealed by the instrument. It is to be regretted, moreover, that Professor Warren has not informed us of the proportion which the above cases bear to his other operations for the removal of malignant diseases.

The late Dr. George McClellan, of Philadelphia, was in the habit of frequently operating in carcinoma, scirrhus, or scirrho-cancer, as he terms the hard variety of this form of malignant disease. He was opposed, however, contrary to the advice of many writers, to a resort to the knife on the first appearance of suspicious tumours, believing that the vascular excitement which accompanies them in their early stage is unpropitious to a favourable result, if it does not, indeed, always favour the speedy return of the complaint. He considered it of great importance, in every case, to wait until the constitution is enabled, in some measure, to check the progress of the growth, and set up a barrier, as it were, around its surface, thereby

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preventing its further extension. The older, in fact, the tumour is, the more favourable, he thought, the chance of a permanent cure.

There is reason to believe that this distinguished surgeon occasionally permitted himself to operate against his judgment, simply to prevent patients from falling into the hands of charlatans and cancer doctors. Speaking of the fact that very few patients ever remain contented with palliative treatment, he observes: "If we do not recommend the knife, or some more acceptable means of ridding them of the existing tumour, they will be sure to go to the rude and unskilful hands of some advertising pretender, and undergo all the hazards of empiricism. The moment, therefore, we find ourselves clear of the complications of engorgement and vascular irritation, provided other circumstances be favourable to an attempt at the radical cure, it will generally become necessary to decide upon the appropriate course. In the majority of such cases, there can be no question about the propriety of recommending the operation, which, if skilfully performed, will leave the most easily cicatrizable wound, and inflict the least agony upon the sufferer."* Where, from timidity and other causes, the patient was unwilling to submit to excision, Dr. McClellan occasionally employed caustics. "Sometimes even large tumours," he says, "will slough away entire in this manner, and leave a clean, healthy ulcer, which will afterwards close by a sound cicatrization."† It is to be regretted that Dr. McClellan has not favoured the profession with the results of his experience in this mode of removing cancerous growths.

In regard to encephaloid or medullary cancer, Dr. McClellan considers it as an essentially acute disease, the progress of which is usually so rapid as to hurry the unfortunate patient to a speedy exit from his suffering. It is only very rarely that the affection pursues a chronic course, or that its progress can be checked by remedial measures. In the former case, all attempts at eradicating the tumour by an operation, while it is rapidly extending, invariably fail of accomplishing the desired object. The disease will invariably return even before the wound left by the knife has cicatrized, thus hastening the fatal issue without hardly the benefit of a reprieve. The proper plan, under such circumstances, is to treat the case antiphlogistically, in order to enable nature to set up a landmark against the further extension of the malady. In this manner, the morbid action may

* Principles and Practice of Surgery, p. 401, Phila. 1848.

† *Op. cit.* p. 402.

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sometimes be rendered dormant for a considerable length of time. "Now, if we can seize such an emergency, and perform a justifiable operation, it is possible, in this class of cases, as well as in those which were originally chronic from the start, to rid the patient of his uncomfortable and dangerous companion for the remainder of his life. The same rules of procedure, however, should always be regarded that we have laid down under the head of operations for the removal of scirrhus. We must be able to extirpate *the whole of the existing disease by a single operation*, or it will be useless to make the attempt. The system must be free from any such constitutional derangement, or internal development of organic disease, as contraindicates an operation upon hard cancer."*

Dr. McClellan teaches that melanotic tumours return even more frequently after extirpation than encephaloid; and he is of opinion also that they occur more extensively in remote parts, and secondarily among the internal organs.† I recollect a memorable instance of this disease, which was under the care of this distinguished surgeon while I was his private pupil in 1827, and which strikingly illustrates its relapsing tendency after excision. The patient, aged about thirty-five years, a stove-manufacturer by occupation, had a large number of tumours, immediately beneath the skin, scattered all over the abdomen, from above the ensiform cartilage to the pubes, of a dark bluish colour, movable, of a hard consistence, and of the volume of a pea to that of an almond. When first noticed, several years previously, they were of the size and appearance of small shot. The general health of the man during several months that I watched him was excellent, and he was never disabled from attending to his business, except when laid up after operation. The course which Dr. McClellan pursued was to extirpate three or four of the larger tumours at a time, and then to wait until the resulting wounds were healed, when he would resume the attack. Thus he continued until he became perfectly satisfied that further excision would be unavailing; for as fast as he removed one deposit another would spring up in its place, while, meantime, numerous other tumours formed beneath the surface around. The patient finally died from the effects of the disease, which no doubt had seized upon the internal organs. The particulars of this, and of several other interesting cases of melanosis, exhibiting this tendency in the disease to return after extirpation, are mentioned in Dr. McClellan's work on *Surgery*, edited by his son.

* *Op. cit.* pp. 411–415.

† *Op. cit.* p. 420.

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My colleague, Dr. Palmer, Professor of Anatomy in the University of Louisville, has politely communicated to me a case of melanosis, remarkable for its latent character, and for its relapsing tendency after operation. A lady, aged sixty-three, consulted him in the summer of 1851, in relation to a tumour on the top of her head, which was giving her some pain. It was about the size of a chestnut, firm in its consistence, of a rounded form, and of a decidedly black colour. It was first noticed, about twenty years ago, as a small black spot, which remained stationary until two years since, when it began to increase in size; it had never been painful, and troubled her only by being in the way of her comb in dressing her hair. Her health was as good as usual, she having for years been subject to occasional attacks of indisposition. Professor Palmer did not see her again until the following winter, when he was requested to visit her for an attack of facial neuralgia. The tumour was now double the size as at the first examination five months ago. The neuralgia was somewhat protracted; but as soon as the woman had sufficiently recovered, the tumour was removed with the knife. On being submitted to the microscope, its malignant character was found to be very apparent. In a short time, and before the wound was healed, a black substance, about the size of a pea, shot up among the granulations, and extended several lines beneath the healthy skin. This being excised, the wound closed without any farther manifestation of disease in its immediate vicinity. But before it had entirely cicatrized, other black spots began to show themselves on different parts of the head. There were not less than from ten to fifteen, and they all appeared within four weeks after the operation. At first, they were not elevated above the skin, but in a short time they became prominent; and in six weeks from their first manifestation they had attained the size of a large pea. The tumour was removed in February, and about the middle of April some disturbance of the general health was first observed. The woman began to look sallow, and complained of debility, loss of appetite, and indigestion. From this time on she steadily declined, and in June it was evident that internal disease was rapidly undermining her existence. Severe pain in the back and abdomen kept her almost constantly miserable; the frequent occurrence of nausea and vomiting added to her sufferings. Emaciation, loss of strength, and the cancerous cachexy became very apparent. About this time, Professor Palmer first noticed an unusual prominence of the abdomen, accompanied by an unnatural hardness, which was not limited to any particular point, but extended over the

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greater portion of the walls of the cavity. By the first of September, the emaciation was extreme, and the dusky hue of the countenance was very striking.

The tumours on the head had changed but little from the description given above. Death occurred on the 13th of September; and, upon examination, the great omentum was found loaded with cancerous matter. In several places, it was nearly as thick as a man's hand. There were not only numerous lumps of melanosis, but there was also a large quantity of encephaloid intermingled with it. In fact, the soft cancer was altogether the most abundant, the black deposit being scattered about in isolated patches, in the midst of the larger mass of encephaloid. The mesentery was found to be much in the same condition. In the gastro-hepatic omentum, the melanosis was more abundant than the other form. The liver exhibited a very striking appearance. The black and the white deposits were scattered about in patches, and intermingled with a groundwork having the normal colour and structure of the organ. Near the splenic fissure were a few rounded black masses, connected with the gastro-splenic omentum. The walls of the intestines were healthy. No other parts of the body were examined.

The following case of apparently malignant disease came under the observation of Professor Mussey, and was reported, in part, many years ago, in the eleventh volume of the *New England Journal of Medicine and Surgery*, published at Boston. The patient was Dr. G. Heaton, of Berlin, Vermont, and was fifty-one years of age at the time of the operation, in February, 1822. The tumour, when first noticed, scarcely three months previously, was about the size of a filbert; but, at the period referred to, it was as large as a common fist, and had a pulpy feel at its most prominent part, where the skin was also thinner and more tense than elsewhere. As the morbid mass was deeply imbedded in the neck, it was deemed best to tie the primitive carotid artery, when it was carefully removed along with the sterno-mastoid muscle, which was lodged in its substance, and much changed in its appearance. The wound healed kindly, and the patient was directed to take daily, for some weeks, from fifteen to twenty grains of the phosphate of iron. The texture of portions of the tumour was as perfectly encephaloid as it could possibly be.

In a letter which I have had the honour to receive from Professor Mussey, dated March 22, 1853, he says: "I have this evening received a note from Dr. John Winslow, of Berlin, Vermont, the residence of the late Dr. Heaton, in which he assures me that that

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gentleman never had any recurrence of the disease for which I operated upon him, and that he died of consumption in January, 1850, aged seventy-nine years."

Sir Everard Home* informs us that, in the year 1773, it was the opinion of the most eminent practitioners in London, that extirpation should be performed in all cases in which the surgeon was capable of removing the whole of the diseased structure. This advice was given with a full knowledge that contamination of the system sometimes precedes the local disorder; yet, as the evidence of the existence of this contamination is not always clear, it was thought right to afford the patient the only chance that the power of art could offer. "The want of success," continues Sir Everard, "attending the greater part of the operations performed upon this principle, has established a point which could not have been otherwise ascertained. It has produced complete proof that contamination does generally precede the absolute alteration in structure; and, in too many instances, to a much greater extent than it was reasonable to imagine. This fact, which has only been lately established, should be made universally known, as being essentially necessary to guide us in our judgment respecting the propriety of performing an operation; and I am ready to confess that in all cases where the disease has arrived at that stage in which it has acquired the power of contamination, I should be inclined, from the experience I have had, to doubt the success of the operation, and therefore could not venture to press it upon the mind of any patient. But, if the patient should desire the operation, I would not refuse to perform it; since, whatever my own doubts may be, I am not able to bring proof that the operation will always be unsuccessful."

The following remarks of Sir Everard are too pertinent to the subject before us to be omitted: "The foregoing observations are very strongly confirmed by what happens to almost every practitioner in surgery, who, in the early part of his practice, advises the operation, and performs it with very sanguine hopes of success, in cases in which, at a more advanced period of his life, from the experience he had acquired, he would not have proposed it. This happened to Mr. Hunter, and has happened to myself and to those practitioners of whose judgment I have the highest opinion."†

Dr. William Nisbet,‡ of Edinburgh, expresses himself inimical to

* Observations on Cancer, p. 171, London, 1805. † *Op. cit.* p. 176.

‡ An Inquiry into the History, Nature, Causes, and Treatment of Cancer, Edinb. 1795.

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extirpation. The use of the knife, he observes, is never to be trusted as a certain resource. "It frequently hastens on the ravages of the malady."

Mr. Pearson,* on the other hand, declares that the greater number of patients have no return of the disease after the parts affected have been carefully dissected away. He strongly recommends the operation, and insists upon union by the first intention as calculated to prevent relapse. It may be added that this eminent surgeon had full confidence in the statements of Alexander Hill, respecting the result of his experience, referred to under the head of cancer of the mammary gland.

Mr. Fearon, of England, who published the third edition of his work on cancer, at London, in 1790,† strongly urges the necessity and advantage of operation in this disease. He declares that it is the only remedy, and advises its employment at an early period of the complaint, as most likely to conduce to a favourable issue. Nevertheless, he does not think that the operation ought to be entirely prohibited even when there is ulceration of the skin, firm adhesion of the tumour, or extensive enlargement of the neighbouring ganglions. His experience seems to have been almost as favourable as that of Mr. Nooth, alluded to in another part of this report. Fearon strongly insists upon healing the wound by the first intention, to the neglect of which he ascribes much of Monro's bad success.‡

The late Mr. John Abernethy,§ whose writings were formerly so popular in the United States, remarks, in speaking of malignant tumours, that, however numerous and momentous the deterring reasons may be, yet they should not prevent us from operating in many cases. He is clearly of opinion that the knife should be resorted to whenever it is perfectly certain that the whole of the morbid structures can be removed, provided always that the constitution is not so disordered, or diseased, as to render such interference nugatory. He directs that great attention should be paid to the diagnosis of malignant tumours, in order that, having ascertained their true character, an operation may be undertaken at the earliest possible moment, while the complaint occupies the smallest compass, and there is no contamination of the system at large. Resorted to

* Treatise on Cancer, pp. 5–82–83.

† A Treatise on Cancer, pp. 81–82, London, 1790.

‡ See the article on Cancer of the Mammary Gland.

§ Surgical and Physiological works, vol. ii. pp. 126–27, Philad. 1825.

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under these circumstances, he has known life prolonged for five, six, or more years; and when, after that lapse of time, the malady reappeared in the cicatrice, death seemed to have been the result rather of constitutional than of local suffering. Mr. Abernethy occasionally operated after ulceration had set in, for the purpose merely of diverting the disease, as it were, from its natural course, and thereby lessening the pain and discharge consequent upon an open state of the disorder.

The views of the late Baron Boyer,* of Paris, upon this subject are very decisive. Regarding cancer, in common with Hippocrates, Celsus, Monro, and numerous other writers, as the product of an internal cause, he considers it as always liable to return after extirpation, whatever may be the age of the patient, the state of the tumour, and the condition of the system. These remarks are particularly applicable to carcinoma of the mammary glands, which he had excised in many instances, hardly a solitary one of which escaped relapse, although the wound almost invariably healed in a very short time. In a small number of his cases death occurred before the completion of the process of cicatrization, in consequence of the supervention of acute disease, the symptoms of which appeared to be influenced, in a remarkable degree, by the state of the nervous system. He rejects the celebrated maxim of Celsus, that it is better in doubtful cases to employ an uncertain remedy than to abandon the patient to a certain death. In commenting upon this injunction, he very justly observes that it would be much safer, as a general rule, to refrain from the use of the knife altogether whenever it is evident that we can achieve no good by it.

Mr. Syme,† Professor of Clinical Surgery in the University of Edinburgh, asserts that the only proceeding that deserves at all to be considered, as a remedy for carcinoma, is removal of the morbid structure. He insists upon early and complete ablation before there is any serious implication of the lymphatic ganglions, or contamination of the general system. In speaking of medullary sarcoma, the encephaloid disease of Laennec and other authors, he states that the disease, after excision, is very often followed by relapse. The chance of permanent relief seems, according to this surgeon, to be most favourable when the complaint is seated in the testicle and bones, and most hopeless when it invades the eyeball. In melanosis,

* Traité des Maladies Chirurgicales, t. ii. pp. 712–715, cinq. edition, Paris, 1845.

† Principles of Surgery, p. 73, 8d ed. Edinb. 1842.

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as the tendency to extension and general involvement is less marked, the prospect of benefit from removal is not so unfavourable as in scirrhus and encephaloid.*

In speaking of encephaloid, the late Mr. Liston observes that, if removed early, it may be arrested; but that if the operation be long delayed a tumour of a similar nature, and more extensive, will almost invariably be produced. In several instances where he extirpated this form of tumour both from the mamma and testis, the malady did not return; but in all other cases the result was unfavourable.

Mr. Liston considers fungus hæmatodes as distinct from encephaloid; and while he asserts that the latter is occasionally permanently curable by the knife, he maintains that the former is always reproduced after excision, and that in a more frightful aspect, both in extent and malignity.†

Carcinomatous disease, according to this author, generally returns after ablation either in the original integuments, in the form of tubercles or buds, in the cicatrice, or in the lymphatic ganglions; very frequently all these parts are affected.‡

The late Professor Samuel Cooper, of London, whose *First Lines of Surgery* were at one time so extensively employed as a text-book in the medical schools of this country, considers that, although encephaloid has a much greater disposition to relapse, owing to its constitutional character, than scirrhus, experience occasionally brings forward a case forming an exception to this statement. As a proof of the truth of this remark, he refers to a case of fungus hæmatodes of the testis in a man, who recovered, and continued well a long time afterwards; and he adds that "we sometimes hear of the same disease in the eyes, breast, and limbs, being effectually extirpated by operation, without being followed by any return of the complaint. Unfortunately, he continues, the contrary more generally happens, so that the prognosis should be qualified by a reference to this prominent fact."§

Referring to melanotic tumours, Mr. Cooper remarks that the only chance of benefit depends upon the early removal of the disease by operation. "An eye, affected with melanosis, has been extirpated without any relapse for several years; and so have tumours of the skin and cellular tissue. Melanotic formations under the

* Elements of Surgery, by Gross, p. 145, Philad. 1846.

† *Op. cit.* p. 152.

‡ *Op. cit.* p. 150.

§ *Op. cit.* by Dr. Parker, vol. i. p. 460.

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tails of horses have likewise been frequently cut away with perfect success. These facts prove that melanosis in some situations has more chance of effectual relief by operation than medullary cancer."*

The late Mr. John Burns,† Regius Professor of Surgery in the University of Glasgow, thinks that there can be no hesitation in the mind of any man, as to the propriety of urging an operation where the tumour is recent, and unaccompanied by enlargement of the lymphatic ganglions. When these bodies are diseased, he considers that there is always much danger of relapse, the probability of which is still further increased by the open state of the cancer.

Professor Burns is one of those who are inclined to recommend excision as a mere palliative measure. Experience had taught him that whenever we can remove all the parts apparently contaminated, we not merely succeed in procuring a considerable period of health, but, when a relapse does take place, death is less miserable than where no operation is performed.

The late Dr. Graefe, of Berlin, affirms that he has often extirpated *fungus hæmatodes* without any relapse having followed. He saw some of his patients in perfect health at the end of sixteen, eighteen, and even twenty years. It is proper to observe, in connection with the results of the experience of this distinguished surgeon, that most of the German pathologists did not, until recently, draw any just distinction between fungus hæmatodes and encephaloid; or, more correctly speaking, that they, in common with the French, generally employed this phrase to designate a class of tumours possessing none of the characteristic properties of malignant diseases. The probability is that the fortunate cases alluded to by Graefe were nothing but common vascular growths.

In February, 1843, Mons. Leroy D'Etiolles,‡ of Paris, published an interesting statistical account of cancer among the inhabitants of France, showing the frequency and mortality of the malady at different periods of life, in both sexes, and in different classes of society. The most valuable part of the inquiry, and that which has a direct bearing upon the objects of this communication, was to ascertain the effects of ablation upon the longevity and welfare of the patients after the operation. Of 1172 individuals in whom the malady was permitted to pursue its own course, 18 lived

* *Op. cit.* vol. i. p. 468.

† Principles of Surgery, vol. i. p. 356, London, 1838.

‡ Journal des Connaiss. Med., Paris, May, 1843.

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more than thirty years after the first appearance of the cancer; while out of 801, subjected to operation, either by the knife or caustics, 4 only attained that period. Of those who lived from twenty to thirty years, 14 were operated on, and 34 were not operated on. Again, of those whose lives were prolonged from six to twenty years, there were 88 who underwent excision, and 228 whose tumours were not removed. Thus, it would clearly appear, that, so far as the prolongation of life is concerned, the result of this inquiry is opposed to the operation. The statistics upon which the above statements are based, were furnished to Leroy by one hundred and seventy-four surgeons, residing in different parts of France, and many of whom, it may be supposed, are not very accurate or competent observers. Nevertheless, the probability is that they are, in the main, correct; and if this circumstance be admitted, they exhibit operative interference, whether by the knife or caustic, in a most unfavourable aspect, no matter how early in the disease it may be employed.

Leroy himself maintains, as a result of the facts furnished him by his professional brethren, the following conclusions: 1. That extirpation does not arrest the progress of cancer; 2. Extirpation should not be adopted as a general method, except in cancer of the lip and of the skin; and 3. Cancer of other parts should not be interfered with, except when hemorrhage from ulceration threatens speedy death.*

Mons. Flaubert,† a distinguished surgeon of Rouen, states that most of the patients upon whom he has operated for cancer have had a return of the disease, either at the original site or its vicinity, or in some remote part. At present, he never resorts to the knife without much repugnance, and always declines interference when the case is complicated with enlargement of the lymphatic ganglions, rheumatic pains, or pains in the kidneys. He is convinced, notwithstanding some cases in which there was no relapse, that the human species would be greatly benefited if ablation were almost entirely proscribed.

G. L. Bayle, author of the celebrated *Traité des Maladies Cancéreuses*, published at Paris in 1833, considers that, although the prognosis is usually unfavourable, yet that the number of cases of success on record is such as to justify an occasional resort to

* New York Journ. of Medicine, vol. i. p. 420, 1843.

† Amer. Journ. Med. Sciences, n. s. vol. vii. p. 455; also, Gazette Méd. de Paris, March, 1843.

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the knife. Mons. Amussat believes that the operation may occasionally save life, though he does not seem to have any confidence in it as a general rule. Professor Roux, while he is in favour of extirpation as a general proposition, admits that it is not unfrequently followed by relapse and death. He has been in the habit of teaching that cancerous tumours are sometimes removable by local and general treatment.* Delpech was satisfied that every operation performed for the removal of carcinoma is succeeded, sooner or later, by a reproduction of the malady. So thoroughly, indeed, was he impressed with this conviction that he considered all attempts at the destruction of the morbid growth, whether by mechanical or chemical means, as a mere chimera.†

Velpeau,‡ whose writings are so well known in this country, lays it down as a principle, to destroy cancerous tumours as speedily and as effectually as possible, before they have made any serious inroads upon the surrounding parts, or contaminated the constitution. He is inclined to regard them, primarily, as of a purely local character. He insists, in conformity with this view, that the only hope of curing the disease is by its early destruction by mechanical or chemical means. The longer it is permitted to remain at its original site, the greater will be the danger that it will contaminate the system by the introduction of "new morbific germs." All carcinomatous tumours are, according to this writer, liable to repullulate, though not in the same degree, or with the same rapidity or frequency. The most malignant are the melanotic, then the encephaloid, next the scirrhous, and finally, as lowest in the scale, the colloid.

One of the most recent authorities upon this subject, in France, is Mons. Vidal (de Cassis), whose work, entitled *Traité de Pathologie Externe et de Médecine Operatoire*, is the universal text-book of Parisian students. In speaking of cancer, a term under which he includes scirrhus, encephaloid, and colloid, he declares his conviction that it is a constitutional malady, and therefore regards all attempts to remove a tumour of this kind as futile and irrational. Nevertheless, he considers a resort to the knife as proper in certain circumstances; as, for example, when the morbid mass mechanically impedes the functions of an important organ, as the larynx, trachea, or pneumogastric nerve; or where the disease increases with so much

* Bulletin de l'Académie Roy. de Médécine, t. 9, 1843–44.

† Dict. des Sciences Méd. t. 8, p. 681, 1812.

‡ New Elements of Operative Surgery, by Townsend & Mott, vol. iii. p. 111, New York, 1847.

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rapidity as to threaten the speedy destruction of life. In the latter case, he thinks that the malady may be deprived, at least in some degree, of its energy, by changing the theatre of its action, so that, although it may return, the patient would be likely to live longer than if no operation had been performed. Furthermore, he is of opinion that ablation would be proper where the patient suffers excessive agony, on the ground that, even if a relapse should occur, the second attack would be milder than the first, and thus lead to a less cruel death. In illustration of this view, he refers to the case of a woman, aged forty years, from whose leg he removed a very painful cancer, consequent, apparently, upon a former application of the actual cautery, and who had survived two years with hardly any suffering, although the disease had shown itself on the head soon after the operation.*

Another very respectable French authority is Mons. Nélaton,† whose work is also extensively used as a text-book, and which exhibits an excellent outline of the existing state of surgery as it is taught and practised in Paris. This writer thinks that no operation should be performed: first, when there is no certainty that we can remove every portion of the affected parts; secondly, when the tumour coexists with cancer in some internal and inacessible viscus; thirdly, when there is full evidence of the presence of the carcinomatous cachexy; fourthly, when the malady affects certain organs, as, for instance, the eye, experience having shown that, in this case, it always returns soon after extirpation, and progresses with frightful rapidity; and, fifthly, when the disease is extremely chronic in its march, as occasionally happens in old persons. After stating that some surgeons also oppose the operation when there is more than one tumour, when it has followed the use of the knife, when it has advanced with great rapidity, or, finally, when it is ulcerated, he remarks that, although these are certainly unfavourable cases, yet he does not consider them as positive contraindications. "We see, for example," says he, "permanent cures after a second, and even a third operation; nay, we may add," he continues, "that even if we were sure that there would be a relapse, we ought still to operate in certain cases; for this relapse may be delayed for a longer or shorter time, and when it does show itself, we may by a second operation postpone the fatal issue.

* *Op. cit.* t. i. p. 494, Troisieme edition, Paris, 1851.

† Elémens de Pathologie Chirurgicale, t. i. p. 870, Paris, 1844.

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Mr. Skey,* one of the surgeons of St. Bartholomew's Hospital, London, seems to think the removal of malignant tumours, in many cases, a matter of serious doubt, on account of the liability of the disease to return. He asserts that operations for malignant affections of the female breast become less and less frequent in proportion to the age and experience of the surgeon. He rejects, as unfit for such a proceeding, all cases of tumours marked by rapid growth, large size, and indefinite outline, by considerable enlargement of the axillary ganglions, by the presence of ulcerative action, and by constitutional disturbance.

Professor Pirrie,† of Aberdeen, Scotland, regards removal by excision as the advisable proceeding. The operation should be resorted to early, before the establishment of lymphatic invasion, and should be performed effectually, the surrounding tissues being raised to a considerable extent along with the tumour. Operative interference, after the occurrence of lymphatic disease, would only bring discredit upon surgery by subjecting the patient to useless suffering, followed by a return of the tumour, and probably, also, by a more rapidly fatal termination of the case.

Mr. Fergusson,‡ Professor of Surgery in King's College, London, the latest English authority, while he admits with every unprejudiced observer that the removal of malignant growths by the knife is very liable to be followed by relapse, nevertheless enjoins that an operation should always be resorted to, provided all the morbid structure can be embraced in our incisions. Excision, in his judgment, affords the only chance of security, and hence he considers it the duty of the practitioner to urge the patient to submit to it. The operation should be performed as early as possible, before the complaint has made much progress. When the skin is seriously involved, when the lymphatic ganglions are numerously or considerably enlarged, and when the general health is much impaired, or the system tainted by the morbid action, it would be unwise to interfere.§

The following are the conclusions at which Dr. Walshe has arrived, after a careful study of the whole subject of cancer, in his celebrated work:‖ "Here, then, may be closed the inquiry into the efficacy of excision as a therapeutical measure. From the facts, figures, and

* Operative Surgery, p. 388, Phila. 1851.

† Principles and Practice of Surgery, by Dr. Sargent, p. 771, Phila. 1852.

‡ System of Practical Surgery, p. 140, Phila. 1853.

§ *Op. cit.*, p. 186.

‖ On the Nature and Treatment of Cancer, p. 244, London, 1846.

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inferences now brought forward, the conclusion is inevitable and imperative, that extirpation of cancerous growths with the knife can neither be regarded as a means of curing cancer, nor of prolonging the existence of persons afflicted with the disease. The accumulated experience of intervening ages, dispassionately scrutinized and fairly interpreted, pronounces the very verdict that was upwards of two thousand years ago rendered by Hippocrates in his memorable aphorism. The lapse of centuries of civilization has done no more, in respect to the question of operation, than to furnish elements for demonstrating what the observant genius of one man had, in an era of comparative barbarism, so acutely divined!"

Professor Bennett* believes that excision ought to be had recourse to, both as a curative and as a palliative remedy. "Undoubtedly, the earlier it is performed the more it is likely to be attended with permanently good results. But even in advanced cases it should be tried with hopes of success; and so long as it continues to *return in the same place*, the excision should be repeated." . . . "The practical rule, then, which pathology and experience unite in causing us to adopt, seems to be this: that so long as a cancer remains fixed in a part which is capable of being removed, and the strength of the patient is not much reduced, so long is the surgeon warranted to interfere. If this applies to cancerous, it does so with tenfold force to cancroid growths, which everything that we know warrants us in asserting are much less fatal and malignant." We have already seen that, in the opinion of Dr. Bennett, cancerous tumours are for some time purely local in their character.

Mons. Broca, the latest systematic writer on cancer, believes that excision should always be resorted to, both in primary and relapsed cancer, provided the general system remains free from infection. The sources of this infection will thus be removed, and its occurrence delayed.† This advice is given by Mons. Broca, in despite, as it were, of the discouraging results of operation, of which no one seems to be more fully aware than himself. In his *Thesis*, published a few years ago, of which an abstract is to be found in the *St. Louis Medical and Surgical Journal*, for September, 1851, from the pen of Professor Pope, he announces his conviction, first, that, in genuine cancer, the disease always returns, sooner or later, after extirpation, either in or around the cicatrice, in the corresponding lympha-

* On Cancerous and Cancroid Growths, p. 245, Edinb. 1849.

† British and Foreign Med.-Chir. Review, for January, 1853, p. 116; New York ed.

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tic ganglions, or, more rarely, in some other part of the body; and, secondly, that the relapsed tumour always progresses, both locally and generally, more rapidly than the original one. After a second operation, repullulation is more prompt than after a first. He has never known reproduction to be postponed longer than two years, while in the great majority of instances he has found it to happen in six months.

PART III.—OF CANCER OF PARTICULAR ORGANS, IN RELATION TO THE RESULTS OF SURGICAL OPERATIONS.

In drawing up this report, I have deemed it my duty to speak of carcinoma as it occurs in particular parts of the body, with a view of exhibiting the results of operations performed for its relief or permanent cure. Such a course was absolutely indispensable, as it would have been impossible without it to determine whether the disease is more amenable to this mode of treatment in some structures, or situations, than in others. A careful perusal of this portion of the labours of the Committee will show that there is no difference in this respect, but that true cancer is equally incurable, no matter how early or how thoroughly it may be removed, in whatever organ or tissue it is located. It will be perceived that a very large part of this branch of the subject is devoted to carcinoma of the breast; a circumstance which will not appear singular, when it is remembered that this organ, from its prominent position, and its remarkable liability to this disease, has been, from time immemorial, the seat of constant operation.

SECT. I.—CANCER OF THE BREAST.

Cancer of this organ occurs chiefly under two varieties of form, the scirrhous and encephaloid. Of these, the first is doubtless by far the more frequent, especially as a primary affection. Colloid and melanosis are exceedingly uncommon, and have hardly received any notice as distinct maladies of this gland.

§ 1. VIEWS OF HIPPOCRATES, CELSUS, AND OTHER ANCIENT AUTHORS.

It is not a little surprising that the excision of cancerous tumours should have been interdicted by Hippocrates, who practised at an

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age and in a country in which complaints of this kind might naturally be supposed to have been uncommon. The fact that he did clearly shows that his knowledge of the disease and of the results of operative interference must have been much more extensive than surgeons seem willing to admit. The precise language of Hippocrates, as rendered by Dr. Francis Adams, in his learned edition of the genuine work of the *Father of Medicine*, is as follows: "It is better not to apply any treatment in cases of occult cancer; for if treated, the patients die quickly; but if not treated, they hold out for a long time."*

In the time of Celsus, practitioners were in the habit of removing cancerous tumours, especially those of the mamma and face, with the knife, the actual cautery, and escharotics. None of these methods, however, as this writer explicitly declares, were attended with any benefit, since the disease nearly always returned, even after the formation of a cicatrice, and conducted the patient more rapidly to a fatal issue than if no operation had been performed. Notwithstanding this, Celsus was in favour of a resort to these measures in the incipient stage of the malady; but, in its more aggravated forms, he thought the physician should content himself with soothing and palliative remedies.†

Leonidas,‡ of Alexandria, was the first to recommend indiscriminate amputation of the breast for cancer. The only case which, in his judgment, contra-indicates the operation, is where the whole organ is involved in the disease, and where it is so firmly adherent to the walls of the chest as to render it impossible to detach it without compromising the life of the patient. We have no account of the results of his experience; nor, indeed, does it seem certain that he himself ever performed the operation which he so strongly recommends.

Antyllus,§ it would seem, once amputated a cancerous breast; but, although the woman recovered from the immediate effects of the operation, she was attacked some time after with carcinoma in the other gland, which caused her death.

* The Genuine Works of Hippocrates, translated from the Greek, by Francis Adams, LL. D. vol. ii. 758, London, 1842.

† Lib. 5, cap. 18, § 2, p. 186; Sprengel, Histoire de la Médecine, t. vii. p. 422, Paris, 1832. It is due to this writer to state that I have derived much aid from him, in respect to the authorities cited in this part of my report.

‡ Ætius, Tetrab. c. 45, p. 611; c. 50, p. 618. Sprengel, *op. cit.* p. 423.

§ Rhazes, Continens. lib. 13, c. 2, fol. 257.

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Paul,* of Ægineta, was fully aware of the repullulating tendency of cancer of the breast. He expresses himself an advocate of the operation of Galen, and also recommends a method of his own, which, however, he rarely, if ever, practised in genuine cancer of this organ.

Rhazes,† who, as has been already stated, flourished in the latter part of the ninth and the commencement of the tenth centuries, thought that, although cancer might occasionally, in its incipiency, yield to internal remedies, yet that when it had advanced to ulceration the only means of safety for the patient was extirpation. He advised that the operation should be executed with the greatest possible care; and, in order to guard against relapse, he enjoined that the knife should be carried not only through the healthy textures, but that it should include all the enlarged veins in the neighbourhood. The mamma having been thus removed, the surface of the wound was immediately burnt with the redhot iron, when means were resorted to for promoting the speedy detachment of the slough. Rhazes does not inform his readers of the result of this kind of practice.

Albucasis,‡ the countryman of Rhazes, and who practised a hundred years later than that distinguished physician, doubted the propriety of operative interference in all cancerous tumours, asserting that he never cured or saw cured, a solitary instance of that implacable malady by this or any other remedy.

Abu'l Kasem,§ another Arabian, recommends a resort to the knife, but explicitly states that he has no confidence in it, because he had never seen a single cure achieved by it, either in his own hands or in those of other practitioners.

From the twelfth until near the middle of the eighteenth century, little success seems to have attended the removal of the mammary gland on account of carcinomatous disease. If a permanent cure occasionally followed, it was regarded as an exception to a general law, which deterred many practitioners from intermeddling with this complaint in any of its stages. A relapse was but too sure to take place, whatever method of procedure was employed, whether the knife, the ligature, the actual cautery, or escharotics.

* Opera, lib. 6, cap. 45, p. 192.

† Continens. lib. 13, cap. 2, fol. 256. Sprengel, *op. cit.* t. viii. p. 425.

‡ Miller's Principles of Surgery, p. 29, Am. ed. 1845.

§ Chirurgia, lib. 1, § 50, p. 96; Sprengel, *op cit.* t. vii. p. 427.

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John Tagault,* who published a treatise on surgery about the middle of the sixteenth century, absolutely repudiates all operative interference in cancer of the mamma. Amatus Lusitanus† was also averse both to extirpation and the actual cautery. He cites a great number of cases where, by palliative treatment, the cancers were kept in check for a long time, but which became speedily covered with frightful ulcers, and occasioned death when they were treated with the hot iron, or caustic applications. Ehrenfried Hagedorne‡ saw several cases of relapse after operation; a circumstance which made such an impression upon his contemporaries that many were deterred from having recourse to it. De la Motte§ had no confidence in the operation. Triller,|| Professor of Surgery at Wittenberg, was in favour of interfering at a very early stage of the disease; but discouraged the operation in cases of long-standing, being satisfied, from ample experience, that relapse under such circumstances is inevitable.

Fabricius Hildanus,¶ on the other hand, was in favour of amputation, and he refers to several examples in which the procedure was crowned with complete success. Nicholas Tulp** shows that extirpation is the only means by which the patient can be saved. Francis Ellain and Denys Guérin, two writers on cancer at the commencement of the seventeenth century, declare that the malady is incurable by any other remedy than excision. J. B. Denis affirms that he saw a surgeon, named Raulin, amputate several cancerous breasts with success. D. Van de Sterre and John Muys relate several successful examples. Dionis expresses himself favourable to the operation; and Ledran thinks that it affords the only resource in this disease. In 1739, C. N. le Cat†† wrote a prize memoir in which he attempted to establish, as a general proposition, the propriety of amputating in all cases of cancer, even when apparently of the most desperate character. He considered that the bad success of the operation was dependent, mainly, upon a want of care in approximating the edges of the wound, thus favouring suppuration

* Instit. Chirurg. lib. 1, cap. 15, 1555.
† Curat. Med. cent. i. p. 62—cent. iii. p. 245.
‡ Obs. et Hist. Med. Pract. Frankof et Lips. 1698
§ Traité Complete de Chirurgie, t. i. ob. 112, p. 5.
|| De Nociva Cancri Inveter. Extirp. Viteb. 1752.
¶ Cent. 1, obs. 89, p. 69; Sprengel, *op. cit.* t. viii. p. 436.
** Obs. Med. lib. 1, cap. 33, p. 97.
†† Haller, Biblioth. Chir. vol. ii. p. 176.

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instead of union by the first intention. He also insisted upon the importance of saving as much integument as possible. Repullulation was to be apprehended only when the cancer adhered to the sternum. John Louis Petit* was in the habit of operating for cancer of the mammary gland; and he was one of the first, if, indeed, not the first, to include in his incisions all enlarged axillary ganglions; a practice adopted by all modern surgeons.

§ 2. VIEWS OF AMERICAN AUTHORS AND PRACTITIONERS.

The late Professor Dorsey,† of Philadelphia, who wrote the first systematic work on surgery that this country has produced, expresses himself as an advocate for an early resort to the knife, before the cancerous poison is absolutely formed, and the neighbouring parts are contaminated. The operation is most likely to eventuate favourably when the morbid growth can be completely eradicated; when the malady arises from accident, and not spontaneously; when the general health is good; and when cancer is tardy in its progress, and does not involve the circumjacent structures.

Professor Gibson says: "Under favourable circumstances, the operation very frequently succeeds perfectly, and the patient never has the slightest return of the complaint; it must not be concealed, however, that it often returns very unexpectedly, and advances with wonderful rapidity, and in a way plainly to show that the knife has not only not eradicated the disease, but hastened its progress." He avers, positively, that whenever the lymphatic ganglions are much enlarged, and of stony hardness, the operation never succeeds.‡

Dr. Gibson does not agree with some modern surgeons that extirpation of the mamma is never successful under any circumstances. He refers, as a proof of the truth of his statement, to the case of a lady in Maryland, upon whom he had operated thirty years ago, and who was still alive at the time he wrote his work. He has also met with several other examples, where ten, fifteen, and twenty years have elapsed without a return of the malady.§

The late Dr. Physick, it would seem, occasionally extirpated the mammary gland when there was enlargement of the lymphatic ganglions, although he was opposed to the practice as a general rule.

* Traité des Maladies Chirurgicales, t. i. p. 223.

† Elements of Surgery, vol. i. p. 393, 1813.

‡ Institutes of Surgery, vol. i. p. 176, seventh edition, Phila. 1845.

§ *Op. cit.* vol. i. p. 177.

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In some of his cases, the malady returned after the operation; but in a few, he had the satisfaction to believe that life had been prolonged by it, his patients having remained free from all disease for several years.* In one of his cases the tumour was ulcerated, and yet the woman enjoyed an immunity from the disease for nine years after its extirpation. At the end of this time, several hard lumps showed themselves at the cicatrice, and death occurred in a few months after.†

The views of the late Dr. Joseph Parrish, of Philadelphia, upon this subject, are entitled to great respect, both because he was a close observer, and because he was in the habit of recording what he saw with scrupulous fidelity. After considerable experience and observation, he confesses that he was much discouraged as to the final success of all operations in cancerous diseases of the mammary gland.‡ He never resorted to the knife in any case where the affection had invaded the axillary ganglions or fixed itself upon the parts beneath the breast, except at the particular request of the patient, with a full explanation of his conviction of the probable inefficiency of the remedy. When the organ alone was involved, he advised the operation, but even here he regarded it as affording only a doubtful prospect of escape; he was never disappointed in finding the disease return, and finally carrying off his patient. He repeatedly saw the cicatrice removed by a second operation; but never with final success. He considered cancer to be essentially a constitutional affection, and doubted whether it ever arose purely from external injury. He had never witnessed any cases of cure from low diet, depletion, and an avoidance of exciting causes. He had been led to believe that many of the cases of cure reported in medical journals had not been examples of cancerous, but of ordinary maladies, rendered suspicious by their alarming appearance, or by the obstinacy with which they resisted various therapeutic remedies.

There was one form of cancerous disease of the breast which Dr. Parrish regarded as particularly malignant, and to which, it would seem, his attention was first directed by the late Dr. Physick. This was a peculiar granulated condition of the surface of the affected gland, imparting to the finger the sensation as if it were filled with small shot. Dr. Physick considered this as the most dangerous form

* Dorsey's Elements of Surgery, vol. i. p. 393, Phila. 1813.

† Mussey, Transactions Amer. Med. Association, vol. iii. p. 336.

‡ North American Med. and Surg. Journ. vol. vi. p. 300, Philadelphia, 1828.

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of the malady, in which he never knew an operation to prove ultimately successful. The experience of Dr. Parrish fully coincided with that of the "Father of American Surgery." "I once operated," says he, "upon such a case in the Pennsylvania Hospital. The disease was confined altogether to the breast, and, as far as could be ascertained by the sight or the touch, was entirely removed. The wound healed kindly, and for a short time the patient appeared comfortable; but in a few months the complaint returned, and soon terminated fatally."*

The late Dr. J. Kearney Rodgers, of New York, one of the ablest and most enlightened surgeons our country has ever produced, and who died only as it were yesterday, was induced, at an early period of his professional career, both from personal examination and from correspondence and intercourse with many of the most distinguished men at home and abroad, to take strong and decided ground against operative interference in cases of cancer of the mamma. In a conversation which I had with him on the subject, only a few months before his lamented death, he expressed himself in the most unqualified manner, declaring that his experience, even under the most propitious circumstances, was adverse to ablation, and condemning, in the most pointed manner, the reckless conduct of the so-called knives-men, both in this country and in Europe. His conviction, based upon an extensive and well-observed practice of thirty years, was clear and positive that excision was useless even as a means of prolonging life, while, as a curative agent, it had utterly failed in every instance that had fallen under his eye. He had his doubts even whether in genuine cancer of this organ, the operation was ever justifiable in any case.

The results of the operations of Professor B. W. Dudley, of Lexington, have been eminently discouraging; for it would seem that he never removed a scirrhous breast without a recurrence of the disease. He states that he has witnessed the healing of a carcinomatous ulcer, as large as a man's hand, within a month, under the influence of a fervid imagination, when all of a sudden, just as the last point was expected to cicatrize, the force of the imagination gave way, the ulcer reopened, and the patient died in a few weeks. He has checked the morbid action for several years by severe abstinence, but has never seen it cured.†

* *Op. cit.* vol. vi. p. 295.

† Transactions Amer. Med. Association, vol. iii. p. 331.

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The late Dr. Twitchell, of New Hampshire, extirpated the mammary gland in a good many instances, on account of scirrhous disease. In almost all there was a reproduction of malignant action in some other part of the system, carrying off the patient. In a few cases only, where a most rigid diet, excluding oils and sugar, was perseveringly observed, was there an immunity from relapse.*

Dr. W. L. Atlee, of Philadelphia, has met with an instance in which he operated in March, 1843, and where there was no return of the malady at the end of seven years. The patient was eighty-six years of age; the skin was corrugated, ulcerated, and attached to the tumour; the axillary ganglions were unaffected, and the whole breast was removed.†

Dr. Usher Parsons, of Providence, R. I., in his learned *Boylston Prize Dissertation on Cancer of the Breast*, published at Boston in 1839, has this language: "To the patient, then, and to himself, the surgeon must hold up the following propositions: 1. That the extirpation of a scirrhous tumour, whether indolent or painful, large or small, recent or of long-standing, is no positive security against its reappearance, and that the same is true in respect to cancerous degenerations of other tumours and ulcers. 2. That the danger of a return is greatly increased when the disease has been of long-standing, or of rapid progress in its development, or, if ulcerated, and especially if it has affected the axillary glands, or adheres to the subjacent muscles. 3. That there is but little hope of preventing a return by operating after the constitution exhibits marks of cancerous cachexy."

Dr. Parsons says that, in this country, so far as his inquiries had extended at the time he published his essay, the proportion of relapses, after operation, was at least two-thirds. Three years previously, he had stated the proportion of final cures at more than one-half. Farther examination, however, had satisfied him that this estimate was too high. It is to be regretted that Dr. Parsons has not accompanied his paper with the data upon which he has founded this singularly exaggerated statement respecting an operation, the final issue of which is so generally unfavourable as to have become a reproach to the profession.

Professor Knight, of New-Haven,‡ has frequently extirpated the breast for scirrhous disease. He thinks himself authorized to state

* Mussey's Report, Trans. American Med. Association, vol. iii. p. 334.

† *Op. cit.* vol. iii. p. 334.

‡ *Op. cit.* vol. iii. p. 335.

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that, in all cases in which there was lymphatic invasion, or enlargement of the axillary ganglions, there was a recurrence of the carcinomatous action, either in some of the neighbouring structures or in some internal organ, thereby destroying life. In those cases, on the contrary, in which the malady was of comparatively slow growth, and without any involvement of the circumjacent parts, the operation was occasionally successful, in the proportion, probably, of about one in four or five. In a few instances, a tumour appeared in the cicatrice, or in its immediate neighbourhood, and was removed with a favourable result. In one case he extirpated four such lumps, which had appeared successively at intervals of about a year, from an old lady, who survived the second operation eighteen years, and died at the age of ninety-five.

Dr. Knight is of opinion that in many of the cases of scirrhus of the mamma, in which the malady returned after extirpation, the operation had the effect of prolonging life for a considerable period. This is not true, however, of soft cancer, the encephaloid, cerebriform, or fungoid disease of authors, in which there was invariably a relapse, augmenting the patient's suffering, and causing death sooner than if no operation had been performed.

The experience of Professor Mussey, in cancerous diseases of the mammary gland, is anything but encouraging as it respects operation; for in all his cases, except two, in which he has been able to ascertain the results, there has been a return of the morbid action in some part of the system within four years, and in most of them within twelve months. In the two exceptional cases, there was no involvement of the nipple, areola, skin, or axillary ganglions; and the tumour, although hard, sometimes painful, and occupying the whole organ, was not adherent at its base. One of the patients was forty years of age, and was entirely well ten years after the operation; the other was under thirty, and in good health eight years after. Dr. Mussey observes, with reference to these cases, that if they were malignant, they were obviously not the most so. "He has but little to boast of from his own experience in this operation."*

Professor Eve, of the Nashville University, informs me that the results of his operations upon cancerous breasts have been eminently unfavourable; for he does not know of a solitary instance in which the malady did not return and terminate fatally, where the diagnosis was unequivocal.

* *Op. cit.* p. 333.

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Professor Pope, of St. Louis, in a letter to me, dated July, 1851, states that he had removed the female breast in a dozen instances for supposed malignant affections. Some of his patients had died, some had had no return of the malady, and of the rest he was unable to learn the history after they had left his hands.

Dr. Traill Green, of Easton, Pennsylvania, formerly Professor of Chemistry at Washington College, in that State, has communicated to me the following interesting case of cancer of the mamma. The patient was fifty years of age, of sanguine temperament, and still menstruating; the tumour, for which no cause could be assigned, was five inches in length by four in breadth, irregular on the surface, free from adhesions to the subjacent parts, and unaccompanied with retraction of the nipple, disease of the axillary ganglions, or change of structure in the skin. The general health was perfect. The whole gland was removed with the knife, together with an elliptical portion of the skin, about an inch and a half in width. Four months afterwards, the cancer returned in the integuments and was again excised. Another relapse took place in two months; and, notwithstanding the use of constitutional remedies and the application of caustics to the parts, the woman expired twenty-two months and a half after the first operation. The morbid products were examined by the microscope, and exhibited the characteristic cancer-structure.

Dr. Green states that he once knew a female who had a small tumour in one of her mammæ, which another practitioner, who saw the case with him, supposed to be carcinomatous, but which gradually disappeared without any particular medication. The individual is now living and in good health, beyond the menstrual period. In another case, a surgeon endeavoured to persuade a woman to permit him to remove her breast, which he represented to be in a cancerous condition. She declined the operation, and is now in excellent health, the gland secreting good milk for an infant born not long ago. Had these cases been subjected to operation, they would have been pronounced as successful cures.

In November, 1843, Dr. Joshua B. Flint, of Louisville, removed a fungous tumour from the right breast of a lady, aged twenty-five years; the disease having existed several months, and exhibited all the characteristic features of that heteroclite deposit. In consequence of a puncture made a week previously, a growth, half as large as a fist, protruded through the skin, and was the seat of frequent and exhausting hemorrhages, threatening a rapidly fatal termination. The integuments being so extensively involved, a portion

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of the wound had to heal by granulations, which several times assumed a suspicious appearance; but the parts finally cicatrized under the use of tonics and simple dressings, and a complete recovery followed, the patient remaining well in April, 1850, and having, in the mean time, given birth to two healthy children. It is stated that the morbid product exhibited, under the microscope, the characteristic structure of fungus hæmatodes.*

The annexed tables have been obligingly prepared for me by my friend Dr. J. Rowan Pirtle, of this city. The first comprises eighteen cases of scirrhus, collected from various sources, and the second twenty-nine cases, transferred from M. Canquoin's *Treatise on Cancer*.

* Transactions of the Amer. Med. Association, vol. iii. p. 337, 1850.

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I. *Table of Malignant Diseases of the Mammary Gland.*

No.	Age.	Married.	Temperament.	Residence.	Cause.	Duration.	Progress and symptoms.	Mode of treatment.	Result.	Structure.	Surgeon.	Page and name of work.	Date.	When last seen.
1	50	1		London	Spontaneous	4 y'rs	Slow; axillary glands enlarged.	Excision			Morgan	187 Lancet	April, 1826	
2	44	1		"	"	1 y'r	Slow; not painful.	"		Scirrhous	B. Cooper	91 "	1826	
3	60	1		"	Blow	1 y'r	Painful; ulcerated.	"		"	Wardrop	622 "	Jan. 1827	3d day.
4	40	1		"		2 y'rs	Slow; painful; nipple retracted; axilla involved.	"		"	B. Cooper	881 "	Sept. 1827	
5	49	1		"		7 y'rs	Slow; ulcerated and painful.	"		"	Wardrop	286 "	June, 1827	4th day.
6	68	1	Sanguine	"	Blow	2 y'rs	Painful and slow; ulcerated.	"		Hard	Morgan	800 "	Aug. 1827	10th day.
7	56	1	Nervo-bilious.	"		1 y'r	Slow and very painful; axilla involved.	"	Apparent cure	Scirrhous	L. Ray	282 "	Sept. 1828	2 months.
8	45	1	Sanguine	"		18 mo's	Painful.	"				735 "	July, 1828	
9	46			"		2 y'rs		"		"		765 "	Sept. 1829	
10	42	1	Nervo-bilious.	"		5 mo's	Painful.	"		"	Morgan	573 "	July, 1827	
11	47	1		"		8 mo's	Nipple retracted; painful.	"	Fatal	"	Scott	30 "	March, 1832	Died 8d d'y.
12	42	1		"		2 y'rs	Slow; axilla involved.	"		"	Stanley	286 "	June, 1832	
13	45	1	Nervous	"		9 mo's	Painful; rapid; axilla involved.	"		"	Lawrence	444 "	Dec. 1832	
14	47	1	Nervous	"	Blow	2 yr's	Slow; rapid; latterly painful; nipple retracted.	"	Apparent cure	"	Lynn	421 "	Sept. 1833	2 weeks.
15	44	1		"		2 yr's	Slow; ulcerated.	"	"	"	Martinet	154 "	March, 1830	2 years.
16	29	1		"	Blow	3 yr's	Slow; hectic fever.	"		"	Martinet	155 "		3 years.
17	48			"		1 y'r	Slow.	"		"	Lloyd	794 "	Sept. 1835	
18	41	1	Nervous	"		1 y'r	Slow.	"		"	Luke	374 "	1849	

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II. *Table of Malignant Diseases of the Mammary Gland.*—Cases translated from the work of Dr. A. CANQUOIN, of Paris.

No.	Age.	Married.	Single.	Temperament.	Residence.	Cause.	Duration.	Progress and symptoms.	Mode of treatment.	Result.	Structure.	Page and name of work.	Surgeon.	Date.	When last seen.
1	58	1		Sanguine.	Paris.	Spontaneous	1 year	Gradual; lancinating pains.	Vienna paste and chloride of zinc.	Cure	Scirrhous; fibro-cartilaginous; size of the feet.	123 Traitement du Cancer, par le Docteur Canquoin.	Canquoin	Nov. 1837	March, 1838.
2	56	1			France.		3 years	Reappeared after excision and ulcerated; system involved.	Vienna paste and chloride of zinc; tonics internally.	"	Five inches in diameter.	126 Canquoin.	"	June, 1835	Died in next y'r.
3	46	1			Paris.	Blow	1 year	Slow.	Internal; local.	Resolution	Hard; size of egg.	211 "	"	March, 1835	1 year.
4	30		1		"	Blow	1½ year	Slow and painful.	Internal, saturnine, cataplasms, &c.	"	Size of turkey egg.	212 "	"	Oct. 1835	
5	36	1			"	Spontaneous	2½ y'rs	Gradual and painful; axilla tender; both mammæ.	Emollient and other applications & tonics internally.	"	Both tumours size of a goose egg.	213 "	"	Sept. 1837	
6	38		1	Nervo-sanguine.	"	Blow	8 years	Painful; system undermined.	Internal and emollient.	Partial resolution.		219 "	"	Sept. 1837	
7	30	1			"		2 years	Painful; axilla involved as well as mammæ of both sides.		Resolution	Firm.	220 "	"	Nov. 1837	4 mo's.
8	41	1			"		9 years	Painful; slow.	Cataplasms, &c.	"	Very hard.	222 "	"	Oct. 1836	5 mo's.
9	50	1			"		13 y'rs	Both mammæ.	Customary treatment.	"	Fibro-cartilaginous; scirrhous.	225 "	"	Feb. 1837	
10	43	1			"		3 years	No pain; slow.	Customary treatment.	"	Hard; size of walnut	226 "	"	July, 1837	
11	39	1		Nervo-sanguine.	"		6 mo's	Painful; axilla tender.	Customary treatment.	"	Size of pigeon's egg.	227 "	"	April, 1837	
12	60	1		Lymphatico-sanguine.	"		1 year	Slow; lancinating pain.	Vienna paste.	Failure	Scirrhous.	229	"	August, 1837	
13	71	1		Sanguine.	"		6 years	Slow; painful; ulcerated.	Chloride of zinc.	Cure	Scirrhous.	234 "	"	March, 1836	2 years.
14	27		1	Lymphatic.	"		4 years	Ulcerated; painful; axilla involved.	Vienna paste and chloride of zinc.	"	Scirrhous.	236 "	"	July, 1836	1½ year.
15	63	1		Sanguine.	"		3 years	Painful; ulcerated; strength depreciated.	Cataplasms, Vienna paste, chlor. zinc, nitrate silver.	"	Hard.	238 "		Oct. 1836	16 mo's.
16	42	1		Bilioso-sanguine.	"	Blow	3 years	Painful; ulcerated; adherent to pectoral muscle.	Vienna paste, chloride of zinc, &c.; internals.	"	Scirrhous.	241 "	"	March, 1837	1 year.
17	48	1			"		3 years	Painful; axilla involved in swelling.	Vienna paste and internal treatment.	"	Encysted, scirrhous and encephaloid.	244 "	"	Feb. 1838	
18	41	1		Lymphatico-sanguine.	"	Blow	10 mo's	Painful, and skin inflamed.	Vienna paste, &c.	"	Very hard.	246 "	"	Jan. 1837	
19	68	1			"		6 years	Ulcerated; axillary ganglia swollen.	Caustic.	"		248 "	"	Dec. 1836	1 year.
20	39	1		Nervo-sanguine.	France.		4 years	Reappeared after amputation by Lisfranc; adhering.	Vienna paste, chloride of zinc, &c.	Partial cure	Scirrhous.	251 "	"	March, 1837	
21	36	1		Sanguine.	Paris.		2 years	Ulcerated; painful.	Cataplasms, &c.	Resolution	Scirrhous.	255 "	"	May, 1837	
22	45	1			France.			Reappeared after amputation; ulcerated; painful.	Emollient, &c.	Failure	Scirrhous.	258 "	"	March, 1836	
23	45	1			"				Vienna paste.	"		259 "	"	March, 1836	
24	56	1		Sanguine.	Paris.		10 yr's	Slow; axilla swollen; adherent to muscle; painful.	Vienna paste.	Cure	Fibro-cartilaginous.	260 "	"	Sept. 1837	1 year.
25	60	1		Sanguine.	"			Ulcerated; painful.	Vienna paste.	Par'l cure	Scirrhous.	263 "	"	August, 1836	
26	36	1		Sanguine.	"				Caustic.	"	Fibro-cartilaginous.	269 "	"	April, 1836	
27	48	1			"		2 years	Adherent and ulcerated.	Vienna paste.	Cure		271 "	"	April, 1837	
28	56	1			"		2 years	Reappeared after excision.	Vienna paste; chlor. zinc.	"	Scirrhous.	272 "	"	June, 1837	1 year.
29		1			"			Reappeared after excision.	Vienna paste.	"	Scirrhous.	"	"	1835	3 years.

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§ 3. VIEWS OF ENGLISH AUTHORS AND PRACTITIONERS.

Dr. Alexander Monro,* Primus, Professor of Anatomy in the University of Edinburgh, in 1742, asked the question, whether cancerous tumours, when they cannot be resolved, ought to be extirpated, or be treated merely palliatively? The subject had evidently engaged much of his attention, and he was anxious to obtain from his professional brethren, in every part of Europe, an expression of their views respecting it. At the time he wrote, he had witnessed nearly sixty cases of cancer, in which the tumour had been removed with the knife, and of these four only remained free from the disease at the end of two years. Of these "lucky people," as he quaintly styles them, three had occult cancer of the breast, and the other had an ulcerated cancer of the lip. He observed that, when the disease relapsed, it was more violent and more rapid in its progress than it was in those persons who had not undergone any operation. It is proper to state that, in Dr. Monro's time, the wound made in the operation, instead of being united by the first intention, was allowed to heal, if it would heal at all, by the granulating process. It would seem that this gentleman was in the habit, in his own cases, of directing that the sore, when reduced to the size of the palm of the hand, should be kept open as an issue during the remainder of life!†

Dr. Monro was of the opinion that it was proper to operate in ulcerated cancer, attended with rapid wasting of the patient, as the only means of prolonging life. He also thought that in an occult cancer, occurring in a young healthy person, as a consequence of a bruise or hurt, the hope of preventing a relapse should induce the surgeon to have recourse to the knife.

Diametrically opposed to the result of the experience of the Edinburgh professor is the observation of the late Mr. James Hill, an eminent surgeon of Dumfries, Scotland, and the author of a work entitled *Cases in Surgery, particularly of Cancers, and Disorders of the Head from External Violence*, issued at Edinburgh in 1772. At the time of the publication of this work, Mr. Hill had extirpated, from different parts of the body, not less than eighty-eight cancers, all of which, except four, were in an ulcerated condition. He regarded the disease, in every instance, as of a genuine

* Edinburgh Medical Essays, vol. v. p. 421, 1742.

† Johnson's Practical Essay on Cancer, p. 110, Phila. edition, 1811.

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character, and he assures the public that all the patients but two recovered from the effects of the operation.

Mr. Hill groups his cases under two heads; the first embracing forty-five, and the second thirty-three cases. Of the former, only one proved unsuccessful, meaning, I presume, that the patient died from the immediate effects of the operation. In three of the cases, the disease afterwards appeared in different parts of the body; and in another, the woman was threatened with some tumours at a distance from the original cancer; but not till three years after the excision, when she died of fever. All the rest of the patients continued well as long as they lived, or up to the time of the publication of Mr. Hill's work. One survived eleven, and another thirty years.

Of the thirty-three cases forming the second group, one lived only four months, and in five others the cancer broke out again. Mr. Hill endeavours to account for the greater fatality of these cases by stating that his previous extraordinary success induced patients to flock to him from all parts of the country, compelling him, often against his inclination and judgment, to operate when there was but little probability of success.

Up to April, 1764, after a practice of thirty years, he had cured sixty-three patients, of whom thirty-nine were alive and sound at that date. In twenty-eight of this number, the operation had been performed more than two years; in the rest, less than two years. The remaining twenty-five, which complete the eighty-eight, were operated upon after the year 1764, and of these, twenty-two were well at least two years.

In July, 1770, when he had eighty-eight cases, two were not cured; in nine, the disease broke out afresh, and in one a relapse was threatened; forming less than a seventh part of the entire number. At that time, forty patients were alive and sound, whose cancers had been extirpated above two years.

The results of the operations of the late Mr. Nooth,* of England, seem to have been still more favourable than those of Mr. Hill. His experience, based upon an extensive practice of thirty years, justifies him, he says, in declaring that only one in thirty of his patients ever had any return of the disease, in any form whatever, after extirpation of the mammary gland. The number of cases reported by him amounts to one hundred and forty-six, and of this five only were unsuccessful!

* Observations on the Treatment of Scirrhous Tumours of the Breast, p. 37, second edition, 1806.

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Mr. Benjamin Bell,* of Edinburgh, who published his celebrated *System of Surgery* towards the latter part of the last century, considers that cancer of the breast, on its first appearance, is, perhaps, in every instance, merely a local affection; that the carcinomatous diathesis is produced, not by any original disease of the constitution, but by absorption of the poison from the parts primarily implicated; and that, therefore, every such malady should be immediately removed by amputation, whenever this is practicable. This, he thinks, ought to be an established maxim in the treatment of all cases of cancer wherever it may be situated, but particularly when it occurs in the mammary gland. "When practitioners, therefore," says he, "have an opportunity of amputating cancerous or scirrhous breasts early, they ought always to embrace it. It often happens, however, from an improper delicacy in patients, as well as from other causes, that practitioners are not consulted till the disease is far advanced. But although the advantages to be derived from the operation will in general be in proportion to the previous duration of the disease, yet, on all occasions, even in very advanced stages of cancer, it is right to advise it, provided the parts affected can be completely removed." It may be added that Mr. Bell expresses full confidence in the statements of Mr. Hill, of Dumfries, in regard to the results of his operations for malignant disease of the breast.

Mr. James Latta,† an Edinburgh surgeon, and the author of *A Practical System of Surgery*, published at Edinburgh in 1795, was a decided advocate of early excision in cancerous disease of the mamma. Like his countryman, Mr. Benjamin Bell, he thought that the complaint was often of a local nature, and that, therefore, a radical cure might reasonably be expected, especially when there is no lymphatic invasion, ulceration, or adhesion of the tumour to the circumjacent parts, or great impairment of the general health; in which case, no operative interference whatever would be justifiable.

Sir Everard Home relates‡ the particulars of twenty-two cases of cancer of the breast, of which eight were permitted to run their course unmolested. Of the remaining fourteen that were operated on, only five proved completely successful.

The late Mr. Robert Allan, of Edinburgh, the author of an ex-

* Vol. ii. p. 487-40, third edition, Edinburgh, 1787.

† Vol. ii. p. 510.

‡ Observations on Cancer, London, 1805.

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cellent system of surgery in its day,* states that, upon taking a review of the cases of cancer in which he was concerned, either as the operator or as the assistant of Mr. John Bell, and which, at the time he wrote his work, amounted to a considerable number, he finds that in the spontaneous form of the disease, occurring in the female breast about the forty-fifth year, at the cessation of the menses, there was a return of the affection, either at the original site or in some other part, in at least two-thirds of the patients, although the knife was employed before the lesion had made much progress. This result, so unfavourable, led him to adopt the opinion of Monro, already cited, respecting the futility of operative interference. Mr. Allan thinks that the prospect is generally better, all other things being equal, in cancer of local than in cancer of constitutional origin. "In the former," says he, "the malady is slow in its progress, and, if removed early, there is usually no reproduction; whereas, in the latter, it is rapid in its march, and often reappears after excision."

The character of the late Mr. Henry Park, of Liverpool, as an able, experienced, and scientific surgeon, is well known to the American profession. Few English practitioners have stood higher in their day in the estimation of their brethren; and hence a brief reference to his opinion upon the subject before us will be particularly appropriate on this occasion. His views of the excision of carcinomatous tumours deserve to be recorded as the result of an enlarged experience and the most careful observation. In the *Memoir of his Life*, by his daughter,† it is explicitly stated that he had no confidence in the cure of cancer by the use of the knife. After extirpation of the mammary gland, he had so frequently seen the disease show itself in the other breast that he was led to doubt whether recovery ever takes place in the genuine form of the complaint. It seems that he had met with no less than eight cases of carcinomatous breasts, condemned as incurable, except by operation, by the surgeons of London, Edinburgh, and Dublin, which all recovered under palliative treatment, without amputation. They had been originally under his care, and afterwards returned to him to receive farther advice and attention. The disease was, of course, not carcinomatous in any instance; at all events, it was not so regarded by Mr. Park.

About fifteen years ago, namely, in 1838, a very able and learned paper on the treatment of carcinoma of the mamma was published

* A System of Pathological and Operative Surgery, vol. i. p. 244, Edinb. 1821.

† Transactions of the Provincial Medical and Surgical Association, vol. vii. p. 479.

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by Dr. John Macfarlane, of the Royal Infirmary of Glasgow, from which it appears that, of thirty-two cases of this kind operated upon by this distinguished surgeon, the cure was not permanent in a single one. The ages of his patients varied from forty-two to fifty-nine; twenty-three were married, and had children; the rest were unmarried. In twenty-two the entire gland was affected; and in ten the disease existed as a tubercle. In twenty, the axillary ganglions were involved, though not extensively, and were removed in the operation. In the other twelve, no derangement could be detected in these structures. In nine cases, the cancer returned in the integuments of the chest, or in the axilla, at a period varying from six weeks to three months after the excision; in thirteen cases, from three to nine months; in four, from nine to twelve months; in three, within two years; and in one, near the end of the third year. In two of the cases, the operation proved fatal; in one, from pleurisy; and in the other from erysipelas.*

Besides these examples, Dr. Macfarlane ascertained, on inquiry among his friends, the results of eighty-six additional cases, in which this organ was extirpated for well-marked carcinoma, and in not one of these was the cure permanent. In the majority, the operation was performed at an early period, and under, apparently, the most favourable circumstances; the affected parts were thoroughly removed, and in many there was no distinct evidence of constitutional deterioration; and yet in every one the disease returned, both externally and internally, and proved fatal.

There is no writer in the English language whose works have been received with more confidence and respect by the physicians and surgeons of the United States than those of Sir Benjamin C. Brodie. They bear, throughout, the impress of an honest mind, great judgment, rare originality, and uncommon practical acumen; and his opinions are, therefore, entitled to much weight, especially upon the subject under consideration, which has evidently engaged much of his attention.

The cases in which Mr. Brodie† thinks an operation for the removal of a cancerous breast proper, are, first, where there is no disease of the skin; secondly, where the nipple is not retracted; thirdly, where there is no adhesion of the organ to the surrounding

* London Medical Gazette, June 2, 1838; Amer. Journ. Med. Sciences, vol. xxiii. p. 219.

† Select Surgical Works, p. 214, Phila. 1847.

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parts; fourthly, where there is no enlargement of the axillary ganglions; fifthly, where the patient is not very far advanced in life; and, lastly, where there is no sign of internal mischief. Sir Benjamin states that he does not intend to say that in these excepted cases there will be a permanent cure; but he declares that this will happen occasionally, and he adds that the chance of the occurrence is sufficient to warrant a resort to the knife. He recollects several persons from whom, under the above circumstances, he removed the mammary gland, and who are now alive and well many years after the operation, but who would have died long since had the disease been permitted to pursue its own course.

He is also of opinion that an operation is proper under other circumstances. A hard scirrhous tumour, for example, occasionally forms on the surface of the breast, but unconnected with its structure, except at one narrow corner. He has operated successfully in cases of this description, without extirpating the gland itself. Again, a scirrhous tumour sometimes originates in the nipple, subverting its structure, without seriously involving the mamma itself. In one case of this kind, in which the whole breast, although healthy, was excised, the woman was alive and well many years afterwards. In another instance, that of a large elderly female, with a very large breast, the affected nipple alone was removed with chloride of zinc and caustic paste, and the patient was found to be doing well three or four years afterwards.

Mr. Brodie states that he has occasionally operated merely with a view of prolonging life, and relieving the patient from present suffering. He refers to several cases in which he happily accomplished this object.

He thinks the following circumstances as particularly unfavourable to an operation: retraction of the nipple; ill-defined state of the mamma; a contaminated condition of the skin, as indicated by its brawny, anserine, tuberculated, dimpled, or indurated appearance; implication of the lymphatic ganglions; ulceration of the tumour, or its firm attachment to the integuments, muscles, or ribs; the coexistence of cancer in other parts of the body; and deterioration of the general health.

The late Mr. Liston,* in cancer of the mamma, recommends recourse to the knife as soon as the malignant nature of the tumour is recognized, before it has made much progress, and before it has

* Elements of Surgery, by Gross, pp. 412–13, Phila. 1846.

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contracted extensive adhesions, or contaminated the lymphatics. In the latter event, he considers operative interference scarcely justifiable. When enlarged glands are perceptible above the clavicle, or in the intercostal spaces, the practitioner who advises intermeddling with the original tumour must, he says, be grossly ignorant, or very unprincipled. Notwithstanding Mr. Liston encourages a resort to the knife in the early stage of the malady, he distinctly declares that permanent riddance from mammary carcinoma is scarcely to be expected by operation, or any other means. Neither does he consider ablation of medullary and bloody tumours of this gland as more successful in its result; though he adds that he has certainly witnessed permanent cures under unpromising circumstances, as when the tumours were large, of long duration, and even ulcerated.

Mr. Herbert Mayo,* formerly surgeon to the Middlesex Hospital, London, whose writings are well known in this country, states that in amputation of the breast, under the most favourable circumstances, that is, at the earliest possible period after the disease has declared itself, and before there is any invasion of the surrounding structures, the malady will return in ninety-nine cases out of every hundred, either at the cicatrice, or in the adjacent ganglions. When the skin is extensively implicated, the gland firmly adherent, the tumour ulcerated, the lymphatics much engorged, and the general health seriously deranged, the patient would not derive sufficient benefit from the operation to compensate him for the suffering attending it. The time at which the malady recurs after removal of scirrhus ranges from six months to two years, or even longer. The changes which supervene in secondary scirrhus, whether originating at the cicatrice, or elsewhere, are generally mild, compared with the former. The constitution, however, sinks more rapidly on the return of the malady, being by that time more completely undermined than at the first invasion.

Mr. Syme,† of Edinburgh, in speaking of carcinoma of the mammary gland, remarks that it would appear, on the whole, that the prospect of permanent recovery is not so hopeless as it has been represented, provided the operation is performed only in proper cases, and in an efficient manner. Similar diseases, he continues, are removed from other parts of the body, as the lip, with almost

* Outlines of Pathology, p. 573, London, 1836.

† Principles of Surgery, p. 296, Edinb. 1842.

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invariable success; but no surgeon thinks of cutting out a cancer of the lip if there be an affection of the glands under the chin.

Mr. Travers,* of London, whose name is so well known in this country by his various writings, especially his treatise on *Constitutional Irritation*, believes that cancer of the mammary gland is "never curable by excision. He thinks that he is safe in saying that of twenty cases which would, twenty years ago, have been operated on by verdict of a consultation, not five, or not more, would now be subjected to it by the consent of the surgeons." Generally, when life is prolonged by ablation, without reappearance of external disease, death finally results from visceral invasion. Mr. Travers can recall to mind instances, exceptional ones, of course, of genuine carcinoma, where extirpation was followed by a survival of ten, fifteen, or even twenty years.

Sir Astley Cooper† states that the disease in a large proportion of cases of excision of scirrhous breast returns; but not so frequently as formerly, especially if the patient, immediately after recovering from the operation, is subjected to an alterative course of medicine. He considers that, in the present state of our knowledge, extirpation furnishes the only hope of preventing the malady from proving destructive, with the exception of those cases which occur in advanced age, in which it makes little inroad on the constitution, and little progress in the parts. Although the patient may not ultimately survive, yet it may be said that when the complaint does return the operation, by preventing ulceration, preserves the individual from a most painful and offensive state. "On these accounts," says he, "I recommend the patient to submit to it. Hope is revived, and the only chance for life is given."

In regard to medullary cancer of the breast, Sir Astley Cooper‡ distinctly states that it is less liable to return after extirpation than the scirrhous form of the affection, and he therefore advises an early resort to the knife. The constitution, as after the operation in hard cancer, requires an alterative treatment, to counteract the tendency to relapse.

Mr. Bransby B. Cooper§ is an advocate for extirpation of the scirrhous breast in the earlier and milder forms of the affection; but

* Delafield's Biographical Sketch of J. K. Rodgers, M. D. p. 22, New York, 1852.

† Lectures on Surgery, by Tyrrell, p. 273, Phila. 1835.

‡ *Op. cit.* p. 279.

§ Lectures on the Principles and Practice of Surgery, p. 565, Phila. 1852.

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in the more active or advanced cases, the operation, he thinks, is rarely successful, even as a means of prolonging life. Under such circumstances, indeed, he is of opinion that the operation itself is sufficient to create a further development of the malady, and that the matter which is deposited under the attempted restorative inflammation is cacoplastic instead of healthy; thus leading, as a necessary consequence, to a propagation of the morbid action to other parts of the body, quickly followed by a fatal termination. Mr. Cooper alludes to the case of a lady who had a scirrhous tumour in one of her breasts, and who remained quite well for ten years after the operation, when the complaint returned in the cicatrice, and ultimately destroyed her life. In another case, the disease had not reappeared at the end of eight years, though the morbid mass had in this, as in the former instance, every characteristic of the true scirrhous tubercle.

Although this surgeon is averse, as a general rule, to any operative interference when the tumour is in a state of ulceration, yet he seems disposed occasionally, under such circumstances, to recommend a resort to the knife, with a view merely of relieving the excessive pains, and checking the exhausting discharges.

In respect to soft cancer of the mammary gland, Mr. Cooper states that all remedies, even extirpation, are unavailing. He is, however, of opinion that, as the operation removes a great source of constitutional irritation, it ought not to be entirely repudiated, and the patients abandoned to their fate. Soft cancer, he says, requires earlier extirpation than any other form of malignant disease.*

Mr. South, of London, the able editor of *Chelius's Surgery*,† cautions practitioners against advising patients to submit to an operation for a scirrhous tumour of the breast under any circumstances, but especially when it is attended with ulceration or involvement of the neighbouring ganglions. In no instance can a cure be promised; nor can it be affirmed that the person's condition will not be made worse by it. He does not think, as has often been alleged, that an operation will "put off the evil day," or retard the ulcerative process; for he has known many examples to the contrary. The only thing, indeed, which it can do is to afford temporary palliation, when the patient, as is not very frequently the case, is harassed by severe, shooting, stabbing pain, rendering life almost insupportable. In short, he declares that extirpation of a scirrhous breast is, without

* *Op. cit.* p. 573.

† Vol. iii. p. 540, Phila. 1847.

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doubt, the most unsatisfactory proceeding in the whole course of surgical practice.

Mr. Birkett, the author of an excellent work on the *Diseases of the Breast and their Treatment*, published in London in 1850, is rather favourably disposed to operation in mammary cancer, especially in its first stage of development, before the tumour has acquired any considerable bulk, while the adjacent structures are still sound, and the general health continues unimpaired. The chance of success diminishes as the disease advances, and in what Mr. Birkett calls the fourth stage of the complaint, characterized by ulceration and other severe symptoms, he considers operative interference inadmissible, even as a means of prolonging life.

§ 4. VIEWS OF FRENCH AUTHORS.

Under this head, I shall content myself with citing the views and recommendations of the leading systematic writers of France during the last fifty years. Of these, one of the most respectable and influential is Baron Boyer, whose celebrated *Traité des Maladies Chirurgicales* was for a long time the principal authority upon the subject in that country, and which will always remain as a monument of the learning and science of that great and excellent surgeon.

The result of the experience of this author* upon this subject strikingly coincides with that of Monro, given in a preceding page. Of upwards of one hundred cases of supposed cancerous tumours of the mammary gland and other parts of the body, operated upon by him, there were only four or five in which the cure was radical; in all the others the disease recurred after a longer or shorter period, and proved fatal. He is of opinion that ablation is frequently useless and even detrimental, and that, in many of the cases in which it is said to have been followed by permanent relief, the affection was in reality not cancerous, but strictly benign in its character.

The circumstances which, in the judgment of Boyer, justify a resort to the knife, with a prospect of a favourable issue, are, first, the recent standing, small size, entire mobility, and indolent character of the tumour; secondly, where the disease can be traced to some external cause, as a blow, or contusion; thirdly, where the axillary ganglions are not enlarged; and, fourthly, where the patient is young,

* Traité des Maladies Chirurgicales, t. v. p. 585. Cinquième edition, Paris, 1846.

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perfectly healthy, menstruates regularly, and is free from all hereditary taint. The chance of success is much more uncertain when the tumour is old, bulky, and painful; when it arises without any evident cause; when it appears at or before the critical period; and when it is attended with marked derangement of the uterine functions. The case is hopeless when the tumour involves the whole or the greater portion of the breast; when it is flattened and of stony hardness; when it is ulcerated, or attended with retraction of the nipple, or enlargement of the lymphatic ganglions, or when it coexists with similar disease in other regions of the body; and, finally, when it occurs in a female who is forty, or upwards of forty years of age, who is descended from cancerous parents, and who has ceased to menstruate, or menstruates irregularly. Under such circumstances, Boyer concludes with Monro that extirpation should be undertaken only at the most urgent solicitation of the patient, to whom the danger of a relapse should always be carefully explained beforehand.

The views of Mons. Vidal* so nearly agree with those of Boyer that it would be superfluous to present them in detail in this place. A rapid abstract must, therefore, be sufficient.

A relapse after operation, says this writer, is the most frequent result; one, indeed, generally to be looked for, either at the original site of the malady or elsewhere. The circumstances which are most favourable to the success of operations, and which should, therefore, be regarded as indications for its performance, are, the youth of the patient, the regularity of the menstrual function, an external exciting cause, the absence of hereditary taint, the recent standing of the complaint, the small volume and mobility of the tumour, freedom from enlargement of the axillary ganglions, and the presence of but little cellulo-adipous matter in the breast. Ablation is contra-indicated when the tumour is old, voluminous, ulcerated, or spread over the whole breast; when it is complicated with engorgement of the lymphatic ganglions; and when it occurs at the approach of the critical period, or is attended with great irregularity of the menstrual function. Considered separately, however, these circumstances do not, in the opinion of Vidal, absolutely forbid the operation any more than when the mamma is flattened and of stony hardness, when the nipple is retracted, and when the patient is descended from cancerous parents; circumstances which, according to Boyer and his followers, preclude all hope of success. In short, he thinks that

* Traité de Pathologie Externe, t. iii. pp. 804–5. Troisième edition, Paris, 1851.

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one or more of these symptoms may be present, and yet the operation have a favourable issue. When the tumour is adherent, whether to the integuments or muscles, the prognosis is very unfavourable, and should always seriously arrest the attention of the surgeon. No one ought to operate when the disease extends to the intercostal muscles and bones of the chest.

Bérard and Denonvilliers,* speaking of cases of cancer favourable for operation, express themselves almost precisely in the same language as Boyer and Vidal. The small size, circumscribed character, superficial situation, and perfect mobility of the tumour, together with its recent formation, and its development under the influence of some external and appreciable cause, the soundness of the cutaneous covering, and the absence of engorgement of the lymphatic ganglions, are so many indications for the employment of the knife; while the reverse of these circumstances are so many contra-indications. These writers consider the coexistence of several tumours, and those tumours which result from relapse, as particularly unpropitious to a successful issue.

There is, according to these writers, a class of cancers in which it is never prudent to employ the knife, although they appear to be exempt from most of the complications which render the issue of the operation doubtful. These are scirrhous cancers, very chronic in their progress, occurring in old persons, causing hardly any pain or functional disturbance, producing no detrimental impression upon the constitution, and remaining indefinitely stationary, or, at all events, until some external injury, or indiscreet medication, revives their activity. An operation, under such circumstances, might no doubt succeed; but in depriving a patient of a malady which occasions so little inconvenience, the benefit which it confers would hardly compensate for the dangers attendant upon its execution.†

Mons. Velpeau‡ considers the removal of scirrhus of the breast proper only when the disease is entirely circumscribed in its character; or, in other words, when the malady is strictly confined to the gland, when there is no engorgement of the axillary ganglions, and when the internal organs are perfectly sound. "We may hope," says he, "for a complete success if the scirrhus be lobular, perfectly circumscribed, and easily enucleated. But we should expect such a result

* Compendium de Chirurgie Pratique, t. i. p. 696, Paris, 1840.

† *Op. cit.* t. i. p. 698.

‡ A Treatise on the Diseases of the Breast. Translated by Dr. Parkman. Pp. 61–62, Philadelphia, 1841.

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with but little confidence in a case where the scirrhus is ramified, radiated, or of the simple or disseminated ligneous species." Under such circumstances, the operation might be performed with a view of prolonging life for a few months, or of ameliorating the patient's suffering; but beyond this no benefit would accrue from it. In speaking of encephaloid, Mons. Velpeau declares that, in extirpating it, we have only to fear its reproduction with renewed intensity.

J. B. F. Léveillé, of Paris, the author of a treatise on surgery in four volumes,* teaches that the prognosis in cancer, whatever may be its nature, is always extremely unfavourable; for, says he, on the one hand, there is no medical treatment that can resolve the disease; and, on the other, the success of our operations is so uncertain that many of the most reputable practitioners advise against their adoption on the ground that, in the true form of the malady, they are never of any permanent benefit. It is true, he continues, that the parts may heal; but they either reopen, or they become the seat of new tubercles after six months, a year, or several years; so that the affection is merely suspended, not annihilated. Notwithstanding this, Léveillé is in favour of extirpating scirrhous tumours, when they are a source of great and constant suffering, as a means of prolonging life. Speaking of the numerous patients operated on by Desault, and many of whom he carefully watched, this author declares that none survived at the time of the publication of his work in 1812. One only lived fourteen years, and died in 1806 with an enormous carcinoma in the other breast, complicated with enlargement of the mesenteric ganglions.

Richerand, the elegant author of the *Elements of Physiology*, and of a celebrated work on Surgery,† removed a large number of cancerous breasts with variable results, but, on the whole, so encouraging, that he may justly be ranked among the advocates for this operation. Two circumstances, according to this writer, contra-indicate this proceeding, namely, too great an extent of the local vice, and a general cancerous infection. If the breast is adherent to the ribs, immovable, and as it were glued to the anterior wall of the chest, it is not possible to determine the depth of its roots, and, consequently, it might be impracticable to effect the total ablation of the morbid substance, without which success could not, of course, follow the operation. Such a case, therefore, formally contra-indicates such kind of

* Nouvelle Doctrine Chirurgicale, t. iv. pp. 98–99, Paris, 1812.

† Nosographie et Thérapeutique Chirurgicales, t. iv. p. 381. Cinquième edition, Paris, 1821.

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interference. Mere engorgement of one or more of the axillary ganglions on the affected side does not forbid the use of the knife; but the reverse is the case when the lymphatic invasion exists in the opposite axilla, inasmuch as it evinces a general cancerous infection, and the malady soon recurs, spreading with more violence and rapidity than if no operation had been performed. Nor does great adhesion of the tumour to the wall of the chest, according to Richerand, contra-indicate the use of the knife, inasmuch as experience has proved that the patient may be cured, even when we are obliged to remove the great pectoral muscle and scrape the ribs.

§ 5. VIEWS OF GERMAN, DUTCH, AND DANISH AUTHORS.

The views of the surgeons of these countries, upon the results of operations in malignant diseases, do not differ, so far as I have had an opportunity of making myself acquainted with them, in any essential manner, from those of the practitioners of other parts of Europe or of the United States. While some, especially those who flourished in the latter half of the last century, speak rather encouragingly of the use of the knife, others are so much dissatisfied with it that they have thrown all their influence into the opposite side of the scale, denouncing the remedy, except in a very few rare instances, as cruel, improper, and even unjustifiable. The startling and interesting observations adduced by David Van Gescher, of Holland, seem to have first aroused the attention of the physicians and surgeons of Northern Europe to the uncertain, if not futile, nature of excision as a curative agent in cancerous affections. In his *Treatise on Carcinoma*, published at Amsterdam in 1767,* he gives, from his own experience and that of others, numerous cases of this operation, in not one of which a radical or permanent cure was the result. It is worthy of remark, however, that most of these cases were examples of open cancer, or cancer in its more advanced stages, in which, as is well known, no mode of treatment that has hitherto been devised, answers any other purpose than that of a mere palliative. Similar views had been previously promulgated by Trieler, Van der Haar, Kaltschmidt, Damen, De Wind, and Uhorn; and since then the same kind of doctrine has been taught by Theden, Siebold, Kapp, Vogel, Rust, and nearly every modern author of Holland and Germany. In 1789, the Chirurgical Society of Am-

* Proeve over de Vornaamste Langduurige Gezwellen, Amst. 1767.

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sterdam offered a prize of fifty ducats (a sum which was subsequently doubled) for a well-authenticated case of a complete and *unexceptionable* cure of carcinoma, either in its occult or ulcerated stage; but, strange to say, not a single candidate presented himself, such was the state of the science upon the subject at that period.

In 1772, Peter Bierchen* published a treatise at Stockholm, in which, after inveighing against cicuta and other remedies, he strongly insists on the utility of extirpation, especially when performed early in the disease. When there is ulceration of the tumour, or lymphatic invasion, he considers it as an evidence that the growth has taken too deep root, and that, consequently, an operation would only hasten the fatal result. P. Van Esch,† of Gand, in 1772, and J. C. Bauer,‡ of Hersfeld, in 1773, published each some successful cases of amputation of cancerous breasts. J. L. Schmucker,§ the celebrated Prussian surgeon, also about this period, wrote in favour of the use of the knife, and narrated several cases in illustration of its beneficial effects. It would appear that he had performed the operation a great many times, for he was a man of much reputation and extensive practice. Schmucker entertained some singular notions on the subject of carcinoma of the breast, and the removal of this gland by the knife. Thus, for example, he declares that the operation rarely, if ever, succeeded when there was inflammation of the edges of the eyelids. His experience, indeed, had taught him that the existence of this complaint was a sign of a contaminated state of the fluids, and, therefore, highly unfavourable to a radical cure. F. T. Oehme,‖ of Warsaw, and Deneke,¶ of Stralsund, both published, about the same time, the results of some successful cases.

The celebrated anatomist and surgeon, Peter Camper,** of Leyden, was an advocate for operative interference in mammary carcinoma. He advises that the knife should be resorted to at an early stage of the disease, before there is any involvement of the axillary ganglions, or serious disorder of the general health. He made it a rule never to operate, however favourable the other symptoms might be, when the patient complained of a fixed lancinating pain in the

* Introedes tal om Kraftskœder, Scrophulœse och Veneriskesor, och Swulnaderg, Stockholm, 1772.

† Heelkondige Vaarnemingen, Gand, 1772.

‡ Zwei Chirurg. Warnehm., Hersfeld, 1773.

§ Chirurgische Warnehmungen, th. ii. p. 51.

‖ Observ. einer Scirrheusen Frauenbrust mit öffnem Krebs, Warschau, 1774.

¶ C. C. Lerche, Diss. Observ. de Cancro Mammæ, Gœtting. 1777.

** Genees-Naturen Huishondkondig Kabinet, No. 3, p. 194, 1779.

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chest, between the second and third ribs, which, of all the contra-indications, he considered as the most important. He avers that he was never deceived by this sign, and hence he never disregarded it in his practice.

A. T. Richter, the author of the celebrated *Anfangsgründe der Wundarzneykunst*,* expresses the opinion that extirpation of the mammary gland would succeed more frequently, if it were not regarded as a last resort, and if it were employed before trial is made of other remedies. It rarely, however, succeeds, even in occult cancer, and hardly ever in the ulcerated form of the affection. The best means of preventing relapse is to heal the wound by the first intention. Richter recommends the removal of the whole breast, and also all enlarged lymphatic ganglions.

C. L. Schmalz,† of Leipsig, asserts that the extirpation of an indolent scirrhous tumour of the breast is generally followed by the happiest results. The cure, on the other hand, is seldom permanent when the operation is postponed until the part is very painful, while a relapse may always be expected when ulceration has occurred. Gerret Jan Van Wy‡ insists upon the indispensable necessity of amputating cancerous breasts, and states that he himself, in one instance, happily performed the operation where the malady was attended with enlargement of the axillary ganglions.

Henry Callisen,§ of Copenhagen, the author of a celebrated work on surgery, published towards the latter part of the last century, thinks that early and prompt ablation is required, especially in cancer of the breast, on account of the multiplicity of lymphatic vessels, and the consequent danger of contamination of the neighbouring structures. At a more advanced period, and particularly when the malady has been developed without any obvious cause, after the age of forty, there is always repullulation, after operation, at the end of several years, if not sooner. He states that he saw hardly one woman survive of all those upon whom he had operated who were more than thirty years of age. From this remark, we may conclude that most of his cases were followed by a return of the complaint.

The brothers Wenzel, Joseph and Charles,‖ two eminent German anatomists, saw a great number of breasts amputated by Wiedmann,

* Vol. iv. p. 886.
† Seltene Chir. und Medicinische Vorfælle, Leipzig, 1784.
‡ Heelkondige Mengelstoffen, Amsterdam, 1784.
§ Princip. System Chir. Hodièrna, pars ii. p. 132.
‖ Arnemann's Magazin, b. ii. s. 3, p. 336.

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and nearly all the patients remained cured. Several, in whom the wound had been long cicatrized, had a relapse, dependent, seemingly, upon some peculiarity of the skin and adipous tissue. In no instance, however, did the secondary tumour attain the volume of the primitive one; so that, although farther removal might be improper, the first operation generally subserved the purpose, if nothing more, of a palliative.

Dr. Lawrence Heister,* Professor of Physic and Surgery in the University of Helmstadt, is in favour of extirpation of the mammary gland, provided there is no disease of the integuments, or of the adjacent lymphatic ganglions. In that case, he remarks, the operation will not cure the patient; for the virus of the cancer which lies concealed in other parts will in a short time induce a recurrence of the disorder. Notwithstanding this, there are some instances, though they are rare, in which these bodies are indurated, and yet excision is followed by a complete cure.

The late Dr. C. B. Zang, Director of the Surgical Clinique of Berlin, and the author of a large work on *Operative Surgery*,† considers the following circumstances as contra-indicating excision of the breast: first, the existence of a cancerous dyscrasy, as evinced by the state of the skin, and the presence of fever, cough, pain behind the sternum, emaciation, and tendency to thoracic effusion; secondly, infection of the adjacent lymphatic ganglions, especially those of the axilla, although an instance occasionally occurs in which, when this is the case, the operation is successful; thirdly, firm and extensive adhesions of the tumour to the pectoral muscle; fourthly, the rapid development of the morbid growth, or its steady and persistent progress; fifthly, the coexistence of the malady in other organs; sixthly, its hereditary origin; and lastly, an open state of the sore. If, when these contra-indications are absent, an operation is decided upon, it should be performed as early as possible, before any precious time has been lost by vain attempts to cure the disease by internal remedies. Zang, however, looks upon extirpation merely as a palliative, no matter how early or how thoroughly it may be executed; because cancer, in his opinion, is always a constitutional, and not a local affection.

In the article on cancer of the mamma in Rust's *Handbuch der*

* A General System of Surgery, vol. ii. p. 61. English Translation, London, 1768.

† Darstellung Blutiger Heilkünstlerischer Operationen, dritter theil, p. 83, Vienna, 1818.

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Chirurgie, published at Berlin, in 1830,* it is stated that extirpation of the affected organ is the only remedy that holds out any prospect of permanent cure, and that this, unfortunately, is but very rarely successful. The operation is recommended to be performed very early in the disease, before there is any constitutional contamination, or derangement of the catamenia. When the tumour has made great progress, is hereditary in its origin, or is attended with retraction of the nipple, adhesion of the skin, ulceration, enlargement of the axillary lymphatic ganglions, disease of a similar character in other parts of the body, as the uterus, or suppression of the menses, all hope of success from excision is at an end, and should, therefore, be refrained from, inasmuch as it can do the patient no good, and will only bring surgery into discredit. Rust himself,† one of the most eminent and experienced surgeons of his day, had no confidence in extirpation of cancerous breasts; on the contrary, he felt satisfied that it never could effect a permanent cure, and insisted upon it that, whenever such a result occurs, there is some mistake in the diagnosis of the case.

Professor Chelius,‡ of the University of Heidelberg, one of the latest and most respectable German writers on surgery, says that the only remedy for scirrhus of the breast is its removal by the knife, and the earlier this is done the more favourable may the result be expected to be. Where the disease is already somewhat advanced, and where the nipple is retracted, the general health disordered, and the menstrual function irregular, or entirely arrested, the result of the operation is doubly doubtful. Still, it is the only remedy to prevent ulceration. Where this process is already established, where there is hardening of other organs, and where the tumour is firmly attached to the surrounding parts, extirpation will be utterly useless; at all events, it can do nothing more in such a case than a palliative. Chelius has not noticed a more rapid progress after the operation, but, on the contrary, considerable relief for a long time.

Of ninety-eight amputations of the breast, performed for carcinomatous disease at the surgical clinique of the University of Breslau, by Professor Benedict,§ two ended fatally from exhaustion soon after the operation; and in all the rest, except thirteen, the malady returned after the wound was cicatrized, and destroyed life. With

* Vol. iii. p. 520.

† Handbuch der Chirurgie, b. iii. p. 488.

‡ System of Surgery, translated by South, iii. p. 539, Phila. 1847.

§ Rust's Magazine, No. 2, vol. xliv.; Amer. Journ. Med. Scien. vol. xvii p. 513, 1835.

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regard to those that recovered, Dr. Benedict expresses his conviction that, in several at least, there was an error in the diagnosis, the gland being affected with scrofulous, or some innocent disorder.

Of the many operations performed for the removal of cancerous breasts by Professor Pockels, of Germany, a single one only had a favourable termination. The tumour, in this instance, was considered to be identical with that of reticular carcinoma, first described by Müller, of Berlin.*

§ 6. VIEWS OF ITALIAN AUTHORS.

But little is known, in the United States, of the surgical literature of Italy; few of our physicians are acquainted with the Italian language, and hardly any one visits that country in quest of professional information. The surgical sun of that once favoured and enlightened land, the home of poetry, philosophy, and the arts, has never risen since the death of the illustrious Scarpa, whose fame was coextensive with the European and American Continents. The principal surgical writers of Italy, during the last fifty years, are Monteggia, Palletta, Flajani, and, above all, the great surgeon whose name we have just mentioned. From these, therefore, we shall gather what information is necessary to the illustration of the present subject.

The views of Monteggia,† respecting extirpation of cancerous breasts, do not differ materially from those of the French writers, especially Lassus. He agrees with this author that the operation is improper when the malady is complicated with adhesions of the tumour to the circumjacent parts, enlargement of the axillary ganglions, severe cough, and difficulty of respiration. He enjoins it upon his readers that the knife should always be carried through the healthy tissues, and that the whole gland should be removed, however limited the heteromorphous deposit. He also lays much stress upon the importance of preserving a sufficiency of integuments to secure union by the first intention, or, at all events, the speediest possible closure of the wound. In speaking of carcinoma in general, Monteggia remarks that few patients escape relapse after the complaint has made some progress; for, in that event, the shock of the operation, superadded to the baneful impression which the cancerous poison

* Müller on the Nature of Cancer, by West, Part I. p. 84, London, 1840.

† Instituzioné Chirurgicale, vol. vii. p. 212, second edition, Milano, 1815. See, also, vol. ii. cap. xx.

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has made upon the system, will almost certainly induce such a result, and that at an early period.

After extirpation of a carcinomatous breast, Monteggia* recommends the establishment of a large issue in the corresponding arm, with a view of maintaining a discharge of pus at least for several years, if not during the remainder of life. Desault, he says, who neglected this precaution, allowed the general health to take care of itself, and the consequence was that most of his patients soon experienced a relapse of the disease.

J. P. Palletta, who wrote on cancer of the mammary gland at an early period of his professional career, considers scirrhus, even in its incipiency, as a mere local manifestation of a constitutional disorder, particularly when it is not produced by any external cause. He was in favour of early operations, and thought it of no special advantage to save much integument; on the contrary, he was in the habit of removing the skin freely whenever he found it much attenuated or otherwise diseased; and he made it a rule, moreover, always to excise the entire organ. His advice is never to extirpate an old carcinomatous tumour, or one which is complicated with disease of the chest, or with a cancerous diathesis. His opinion, founded upon the result of personal experience, was that all operations of this kind are, in general, merely palliative measures.†

Scarpa‡ thought that extirpation of the cancerous tumour could be performed with a prospect of success only in the earlier stages of the malady, while there is yet an absence of sharp, lancinating pain, and the morbid deposit is still a mere germ, without any constitutional disorder. The chance of success is greatly diminished, if indeed not wholly destroyed, if the surgeon wait until the disease has made much progress, or until there is enlargement of the axillary ganglions, or a carcinomatous cachexy. His opinion on this subject seems to coincide nearly with that of Palletta. Scarpa believes that the surest way of preventing relapse consists in removing, along with the affected gland, a large portion of the neighbouring tissues, which, although apparently healthy, are not so in reality, but usually contain some cancerous matter, and thus serve as a nest for the reproduction of the malady. Thus, for example, if the tumour were not larger than a fist, he advises that at least twice that amount of

* *Op. cit.* vol. vii. p. 213.

† Weigel's Italiaenische Bibliothek, th. iii. st. 2, p. 199, Leipsig, 1797.

‡ Practical Observations on Cancerous Complaints, London, 1793.

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substance should be removed, in order to place the parts in the best possible condition for a permanent cure. It would seem that this great surgeon, in the course of his long practice, met with only three cases of cancer where, after operation, there was no repullulation.*

Joseph Flajani,† a modern Italian surgeon, was a decided advocate for operative interference in all cases of cancerous breast, unattended with disease of the chest, violent pains in the arm, and enlargement of the axillary ganglions. His opinion was that, although the malady might occasionally be resolved, yet such a termination was so infrequent that it should not be considered as an objection to the use of the knife. Advising that the operation should be performed at as early a period as possible, he remarks that in twenty-seven cases in which this suggestion was adopted he met with only two where there was any recurrence of the morbid action. He considers it of no particular advantage to husband the integuments, or to favour union by the first intention. On the contrary, he seeks to promote suppuration and discharge by keeping the wound open, lest the matter should be confined, and thus produce mischief in the surrounding parts. His experience has taught him that relapses are more frequent after prompt than after tardy closure of the parts.

Although Flajani prefers to operate early in the disease, yet he does not consider the use of the knife as positively contra-indicated in the ulcerated form of the malady. Of several females who were subjected to this treatment, under these circumstances, four were cured, one died of consumption, and two retained their cancerous sores, but lived comfortably a long time. In every instance in which the disorder involved the entire gland, or which was attended with sharp lancinating pain in the chest and neighbouring parts, with fever, and enlargement of the axillary ganglions, or where the affection showed itself as a consequence of the suppression of the menses, or some other habitual evacuation, the operation infallibly hastened the death of the patient.

* Compendium de Chirurgie Pratique, par Bérard and Denonvilliers, t. i. p. 694, Paris, 1840.

† Collezione d'Ossero et Riflessione, t. i. p. 256; Sprengel, Hist. de la Medécine, t. viii. p. 486.

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SECT. II.—CANCER OF THE EYE.

The most common forms of malignant disease of the ball of the eye, and the only ones that require any notice in this report, are encephaloid and melanosis. The variety of soft cancer, first described by Mr. Hey under the name of fungus hæmatodes, is by no means infrequent, but as it generally occurs in combination with the encephaloid deposit it does not seem to me to be entitled, in any way, to separate consideration. Most pathologists and morbid anatomists of the present day agree, if I mistake not, in regarding the two affections as mere subdivisions of one and the same growth. Of scirrhus, properly so termed, I have never seen an instance in this organ, and I doubt whether there is a well-authenticated case of it on record, notwithstanding what has been said about it in the books.

Melanosis and encephaloid, with their different varieties, are most common in young subjects, and they generally run their course with great rapidity, often terminating fatally in six or eight months, while very few live beyond a year or a year and a half. When extirpated, the disease invariably returns; or, if this remark be too sweeping, it reappears so generally and so constantly as to render all surgical interference absolutely worse than useless. I am certain that nothing could ever induce me again to undertake the extirpation of the globe of the eye in any case, either for encephaloid or melanosis. When I was a student in Philadelphia, I saw the late Professor McClellan remove this organ in three instances for these affections, and in each there was a reproduction of the malady in less than a month. The patients were children under nine years of age, and in two, the symptoms and progress of the malady were such as to hold out strong inducements for operation. I have extirpated the ball of the eye in eight cases, in six for encephaloid, and in two for melanosis, and in every one, so far as I have been able to judge, I believe that I have done mischief, by hurrying the patient prematurely to the grave. In one instance I performed not less than three operations almost in so many weeks, first removing the eyeball, and then portions of the lids and neighbouring parts, but all to no purpose. My patient died from the effects of the malady in a few months from the time of the first excision. Some years ago I saw a young gentleman, aged about thirteen, upon whom Professor Mussey, of Cincinnati, had already operated twice, with the effect of a

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speedy relapse in each instance. When the case fell into my hands, some weeks after the last operation, the morbid growth had already made such rapid strides as utterly to preclude the propriety of farther interference. The youth went home, and died a few months after.

Anxious to ascertain the views of the surgeons of our ophthalmic institutions, I addressed, early in the progress of my labours, letters to a number of them upon the subject, but regret to say that two only have responded to my call, namely, Dr. Littell, of Philadelphia, and Dr. Bethune, of Boston. The former of these gentlemen, who has been so long connected with Wills Hospital, informs me that his experience respecting extirpation of the eyeball is actually less than that of many surgeons in private practice; for it would seem that the nature of the complaints, and the concurrent testimony of our ablest writers to the inutility of surgical interference have precluded him, in cases clearly malignant, from subjecting his patients to a procedure which gives so little promise of success, and which, in many instances, could hardly fail to accelerate the fatal event.

"My own observation," continues Dr. Littell, "abundantly confirms the unfavourable prognosis so unanimously given; every case of fungus hæmatodes of the orbit, and of medullary or encephaloid disease of the globe—maladies improperly confounded by many authors—which I have traced to its termination, having had intracranial origin or connection which utterly precluded all hope of permanent relief. In one instance, the morbid growth seemed to arise from the sphenoidal cells; in another, of fungus hæmatodes manifesting itself by an elastic swelling, simulating an encysted tumour, below the upper margin of the orbit, it was attached to the dura mater; and in a third, of medullary disease of the eyeball, there was extensive alteration of the optic nerves, and a large tubercle lying on the optic chiasm, though no indication was afforded of grave cerebral affection until comparatively a short time prior to dissolution."

Dr. Littell states explicitly that he never extirpates the eyeball in any case of malignant disease; nor, so far as he is aware, is the operation ever performed by any of his colleagues at Wills Hospital, with two of whom, Dr. Hays and Dr. Parrish, he has been associated since the opening of that institution in 1834.

Finally, Dr. Littell thinks that, in the treatment of medullary disease, attention to the general health, by placing the patient under the most favourable hygienic influences, with the alterative action of

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some of the preparations of iodine, as the iodide of iron and the compound tincture of iodine, may have some effect in retarding the progress of the symptoms. In an instance recently under his care it appeared plainly beneficial. His experience, however, is too limited, it would seem, to enable him to speak confidently on the subject.

The subjoined cases of malignant disease of the eye and orbit have been communicated to me by Dr. George A. Bethune, surgeon of the Massachusetts Charitable Eye and Ear Infirmary.

Case I. *Colloid Tumour of the Orbit.*—The subject of this case, a blacksmith, aged thirty-five, had generally enjoyed good health. In 1832, he had an attack of inflammation of both eyes, which have remained weak ever since. Five years ago, he noticed a pterygium at the inner angle of the left organ, followed by impairment, and, finally, by complete failure, of the sight. For two or three months past he had occasional "twinges" in this eye, and an increasing "blur." When first seen by Dr. Bethune, in May, 1847, the globe was pushed outwards and forwards, and the pterygium, which was thick and vascular, extended partially over the cornea. A similar formation existed on the right eye. Connected with the base of the pterygium in the left organ was an elastic tumour, which filled the inner commissure of the lids, and which was more prominent below than above.

On the 14th of May, the tumour, which extended deeply into the orbit, and adhered closely to the bone, was removed along with the globe, as it was impossible to separate them in the dissection. The eye was perfectly healthy. The tumour, of the size of an English walnut, consisted of a soft gray substance, made up of large jelly-like granules, and was considered by Professor J. B. S. Jackson as a specimen of true colloid cancer. The parts gradually healed, and the man was discharged on the 30th of October, in good health. He was seen nearly two months after, without any appearance of relapse.

Case II. *Melanosis of the Globe.*—Mrs. G., forty-three years of age, has generally enjoyed good health till within the last six years, when she began to have pains at her catamenial periods. Three years ago, she was found to labour under a uterine polypus, which was removed. Twelve months previously, she was taken with pain and redness in the right eye, succeeded, by degrees, by total loss of vision. In June, 1847, the anterior half of the globe projected, in

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the form of an irregular fungous mass, half an inch beyond the lids; the tumour was of cartilaginous hardness in front, and of a mixed white and red hue; behind, it was of a mixed black and red; and, still farther back, there was a shot-like tubercle, nearly black, with a livid tint. The remainder of the globe was much injected, but not enlarged, and a sanious discharge exuded from its surface. The eye was extirpated on the 18th of June, and in a month after, although the wound was not completely healed, the general health was good. In December, the disease returned in the orbit, and probably proved fatal soon after.

The eye was but little enlarged, except at the point already mentioned; it was everywhere solid, and, instead of its humours, there was a firm, dark, brownish mass, of an irregular figure. Posteriorly, there was a whitish substance, of a gristly consistence; the optic nerve was spotted black, and the sclerotic coat was much altered in its texture.

Case III. *Carcinoma of the Eye.*—A farmer, aged seventy-four, who had ordinarily enjoyed good health, observed a red spot on the sclerotic coat of the right eye, which gradually increased, and soon acquired the volume of a small pea; it commenced about two lines from the outer edge of the cornea, and was accompanied with unusual vascularity of the adjacent parts. The remainder of the ball was perfectly sound, and the sight was unimpaired. The operation, performed early in November, 1849, presented nothing extraordinary, and in a few weeks the man was discharged cured. When last heard from, in December, 1851, there was no return of the complaint. The tumour, examined microscopically, exhibited strongly, but not decisively, the usual indications of malignant disease.

Case IV. *Melanosis of the Eyeball.*—A farmer, sixty-eight years old, noticed, twenty years ago, a red spot on the outer corner of the left eye, which gradually expanded into a pterygium; this, after having long remained stationary, began to pain him, and to increase in size, ultimately obstructing the sight. On examining the organ, in February, 1850, a tumour, of a wedge-shape, black, smooth, and irregular, was found to project from its anterior surface, so as to separate the lids. Extirpation was performed on the 12th of the month, and the patient left the Infirmary on the 20th, the wound being only partly healed. On the 20th of June, when last seen, he was improving, without any indication of relapse. Under the

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microscope, the tumour presented, first, numerous cells, apparently epithelial in their character; and, secondly, a great many cells of irregular outline, containing sub-cells and nuclei; also, some of a marked caudate figure, and an immense number of black granules, some isolated, and some aggregated into masses.

CASE V. *Melanotic Tumour of the Ball and Orbit.*—A gardener, aged thirty-six, whose general health has usually been good, was attacked, sixteen months ago, after getting wet, with pain in the left eye, extending to the back of the head, and accompanied with redness and soreness. These symptoms have continued at intervals ever since, and have been followed, at first, with gradual, and finally with complete loss of sight. About six months ago the ball gave way, and began to enlarge until the present time; it is now disorganized, and forms a mass of the size of a small apple, which projects between the lids, and is encircled, in part, by a firm, irregular ring. The organ was extirpated on the 17th of September, 1850. There was some hemorrhage, but this was easily arrested by a tampon; a great deal of swelling and suppuration followed, and the man was not discharged until the 17th of October. When last seen, July 11, 1851, there was no appearance of a return of the malady.

The globe of the eye was altered in its form, but not enlarged, and was filled by a uniform black mass. The cornea was nearly effaced, the sclerotica was much changed, and the humours were destroyed. On the outside of the ball was a large mass similar to that within. The remainder of the growth, forming the larger part of the tumour, varied remarkably in colour, some portions being white, some gray, and others black or grayish-black. There was no appearance of encephaloid. The optic nerve could not be distinctly traced.

SECT. III.—CANCER OF THE GUMS AND JAWS.

The very singular growth, to which the term epulis is usually applied, is generally obscure in its character, and consequently difficult of diagnosis. Experience teaches that certain forms of this affection, which have not, however, been hitherto well described, are of a fibrous, or fibro-plastic structure, and, therefore, comparatively harmless, giving rise, perhaps, merely to some pain and functional disturbance. Occasionally, however, the growth is of a scirrhous nature, and sometimes, though very rarely, it is found to be en-

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cephaloid. In either case, the progress and symptoms are such as usually attend these formations in other parts of the body.

The only remedy for scirrhous epulis is excision, performed at the earliest possible moment after its appearance. As the tumour generally springs from the socket of one of the teeth, ordinarily a grinder, mere ablation of the soft parts will not suffice; to be effectual, and unfortunately this rarely happens even under the most favourable circumstances, the corresponding portion of the bone must be removed at the same time, not only far away from the disease, but in its entire thickness. Partial excision is worse than useless. The following case, which seems to have been originally of a scirrhous character, and which may be considered as typical of this form of cancer, strikingly illustrates the hopeless character of the complaint, and the utter futility of excision as a curative measure.

Mr. S., a tall, slender man, aged thirty-two, of Allen County, Kentucky, consulted me in April 1851, respecting a tumour of the lower jaw, which he first observed three months previously. Commencing in a soreness of the gum round the last grinder of the right side, it was soon succeeded by a small red, tender swelling, which gradually increased until it was the size of the end of the thumb, when it was cut out by a physician in the country. In two weeks the tumour, now fully three times as large as before, was removed a second time, along with the last grinder, the root of which was much diseased.

When the patient reached Louisville, the tumour formed two large cylindrical masses, one on each side of the dental arch, of a pale red colour, devoid of pain, elastic, and of firm consistence, extending from the ramus of the jaw to the first bicuspid tooth. The cheek was round and unchanged in form; but just beneath the angle of the bone the parts were hard and swollen, in consequence of recent suppuration. On the 27th of April, I removed the morbid mass, along with all that portion of the jaw which lay between the bicuspid tooth and the ascending ramus. Slight erysipelas followed the operation, but the wound soon healed, and the patient went home in a fortnight.

Early in September, I learned that the gap left by the removal of the bone was occupied by a tolerably large mass, quite hard, and the seat of a constant discharge of pus. The tumour was steadily increasing, but was free from pain and soreness. On the 24th of the month, I operated upon him a second time, removing the whole of the new growth, which was now as large as a pullet's egg, and about

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three-quarters of an inch of the anterior extremity of the ramus of the jaw, as this supported the diseased structure. The wound healed well, except at a small point, which remained fistulous for some months, owing to the difficulty of keeping its edges approximated in the first instance. On the 2d of July, 1852, the patient informed me that he was in good health, and that there was not the slightest appearance of disease of any kind.

Soon after receiving the above intelligence, however, the malady began to show itself again, in the form of a tumour at the anterior extremity of the ramus of the jaw, which soon acquired the volume of a pullet's egg, and became the seat of sharp stinging pains, darting about in different directions. It was very dense and firm, of a pale colour, and quite tender on pressure. The jaw in front of the tumour was sound, and the general health excellent.

On the 31st of August I excised the ramus at the articulation. The wound united by the first intention, and the man returned home in a fortnight, apparently in perfect health. The morbid growth, scarcely as large as a pullet's egg, and of a dense firm structure, partly fibro-cartilaginous, and partly osseous, was attached to the inner surface and anterior extremity of the bone, the latter of which was unnaturally thin and slightly notched. The external surface, neck, condyle, and coronoid process were all sound; but the inner surface, nearly as far back as the dental foramen, was somewhat roughened and thickened, from the effects of the disease.

Early during the past winter, this relentless disease returned just in front of the ear, forming a rapidly increasing tumour, which is at this time, April 2, 1853, nearly the volume of an ordinary fist. Of course, further interference is out of the question, though the general health remains pretty good.

When epulis presents itself in the encephaloid form, whether primarily or secondarily, no operation, however early resorted to, will be likely to result in anything but failure.

Scirrhus of the jaw bones must be exceedingly rare, as I have not, in a practice of twenty-five years, met with a solitary case. Many instances of the kind are, I am aware, reported in our surgical and periodical literature; but a careful examination of them fully proves that they were examples, not of hard cancer, but of encephaloid, or of fibrous, fibro-plastic, and fibro-cartilaginous growths. Melanosis is also very rare; perhaps still more so than scirrhus, while colloid is more frequently met with, especially in the lower

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jaw, in what has been described under the vague names of osteo-sarcoma and spina-ventosa.

By far the most common disease of the upper jaw, of its body, as well as of its sinus, requiring operation, is encephaloid, which, in whatever form it may begin, or at whatever age it may seize its victim, always terminates fatally, whether it be permitted to pursue its own course, or whether an attempt be made to arrest it by excision. Here again we have to lament the want of reliable statistical tables, showing the results of surgical interference in a given number of cases of extirpation of the diseased structure. Of eleven cases operated on by Lizars, Syme, Robert, Scott, Earle, Guthrie, and Hetling, only one was completely successful. Was that one of a malignant character? The late Mr. Liston removed the upper jaw in seven instances, in only one of which the malady returned, and proved fatal. Dieffenbach relates five successful operations; Gensoul three, and one of failure; Rignoli had one successful case, and one case of failure.*

Who can doubt that the above cases, designated as successful, were of a malignant character? No intelligent, scientific, and experienced surgeon could, I am sure, come to such a conclusion. Hence, these cases lose much of their value in a practical point of view; because they cannot serve as a safe guide in future operations. One great misfortune in regard to nearly all morbid growths of the upper jaw is the difficulty of diagnosticating them; and so long as this difficulty exists will surgeons be likely to report as cases of cancer formations of the most simple and innocent description; the excision of which must necessarily be followed by the permanent cure of the patient.

Looking at my own cases of excision of the upper jaw, seven in number, I find that four have terminated fatally; two from a recurrence of the disease, one from pneumonia, and one from dysentery; that two have entirely recovered; and that one has suffered a relapse, but is still living, five years after the extirpation of the morbid growth. In this case, that of Mrs. Green, I excised nearly the whole of the upper jaw in 1847; and since then several pieces have been removed, at different intervals, on account of a recurrence of the complaint, by Dr. Sloan, of Indiana. Although the tumour, in this instance, was not inspected with the microscope, yet I am certain, from a careful examination of its physical properties, that it was

* Brit. and Foreign Medical Review, vol. vii. p. 250.

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cephalomatous in its character; a circumstance which imparts more than ordinary interest to the case, inasmuch as it holds out encouragement to the surgeon in an affection usually regarded as desperate, if not hopeless.*

It is proper to state that in one of the above cases, marked as having recovered, the disease for which excision was performed was not malignant, while the tumour in the other possessed all the physical properties of encephaloid. I saw the subject of the latter case four years after the operation, when she was perfectly well.

The most common form of malignant disease of the lower jaw is encephaloid, which, judging from my own experience, is much more frequent in childhood and adolescence than at any other period of life. Indeed, the very worst cases that I have seen occurred before the tenth year, and ran their course with a rapidity truly frightful. Most persons thus affected die within six or eight months from the commencement of the attack; and if an attempt be made to relieve them by operation, the malady is almost sure to return in a very short time, either at the cicatrice, or in the adjacent structures, especially the lymphatic ganglions. The prognosis is here, if possible, still more unfavourable than in medullary disease of the upper jaw.†

SECT. IV.—CARCINOMA OF THE LIP.

Cancer of the inferior lip—for it is this organ that this disease nearly always implicates—may be of the true carcinomatous character, or it may be purely cancroid. The relative frequency of these two maladies has not yet been determined, though it is well ascertained that they are most commonly met with in old persons, and that they are much more frequent in the male than in the female. Practically, it would be interesting and important to know how to discriminate between cancerous and cancroid affections of the lip; but upon this subject our knowledge is too imperfect to enable us to arrive at any positive conclusions. All that can be said, with any degree of certainty, is that genuine carcinoma of the lip always returns after extirpation, whereas the epithelial form of the malady is not unfrequently curable by the knife, and sometimes also, but more rarely, by escharotics.

* Gross on Excision of the Upper Jaw, in Western Journ. Med. and Surg. vol. x. p. 207, third series, 1852.

† Western Journ. Med. and Surg. vol. x. p. 278, 1852.

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I have been at some pains to illustrate this branch of the subject by a reference to cases; but after having ransacked all the periodicals at his command, Dr. D. D. Thomson, of this city, to whom it was confided, assures me that he has not been able to find more than three well-reported examples of extirpation of cancer of the lip upon record. The first of these cases is related by Mr. Cline, in the second volume of the *London Lancet*, for 1828–9. The patient, a blacksmith, between sixty and seventy years of age, had no return of his disease twelve years after the operation. The affected parts were hard and ulcerated, and the glands under the chin were enlarged to a great extent. The other cases are mentioned by Mr. Loyd, in the same periodical for 1850, but are deficient in their details. In one of the cases, the operation had been performed nine or ten years ago, and in the other thirteen years ago, without any relapse. Mr. Loyd, in the same communication, refers to two other cases of cancer of the lip operated upon long ago, without any untoward occurrence.

Dr. Benedict,* Professor of Surgery in the University of Breslau, mentions fifty-one cases in which the lip was removed at the surgical clinique of that city, and which were all successful, except one, where the patient was in a state of great weakness at the time of the operation. He regards the operation, however, merely as a palliative measure, as the disease invariably returns, either at the cicatrice or in some other part of the body. In a few cases, in which there was no relapse, he supposes that the disease was strictly local, or produced by some external cause, and not of a genuine cancerous character. He also asserts, contrary to the generally received opinion among modern surgeons, that the absence of tumefied ganglions under the jaw or in the neck does not justify a favourable prognosis; alleging that these bodies, or the lymphatic vessels communicating with them, usually become implicated at an early stage of the malady, and thereby predispose to a relapse in the event of a resort to the knife.

I subjoin the following facts, in further illustration of the subject: John Shipley, aged fifty, was admitted into the Louisville Marine Hospital, July 21, 1842, on account of a tumour of the lower lip, which made its appearance seventeen months before, in the form of a small wart. About eight weeks after it was first noticed he pulled it out, and applied lunar-caustic to the part; but it soon began to grow again, and, gradually increasing in size, at length became ulcer-

* Rust's Magazin, No. 2, vol. xliv; Amer. Journ. Med. Scien. vol. xvii. p. 513.

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ated, and the seat of a severe burning and darting pain. At the time of his admission, the whole margin of the lip was indurated, and covered with a fungous growth, discharging a thin, sanious, and highly offensive fluid. On the 26th of July, Dr. Dodson, one of the surgeons of the hospital, assisted by Dr. T. L. Caldwell and myself, extirpated the affected structures by a free incision, embracing nearly the entire lip from one angle of the mouth to the other, and extending fully as low down as the commencement of the chin. Several vessels having been tied, the gap was filled with lint, and the wound soon healed by the granulating process. It should be stated that there was no disease of the gums or jaw, nor any enlargement of the neighbouring lymphatic ganglions. The tumour was of a firm scirrhous consistence; and the patient was very feeble and emaciated, at the time of the operation, from pain and loss of appetite and sleep.

I saw Shipley again in July, 1851, nine years after the above operation. His health had been perfectly re-established, and no tendency to relapse had ever manifested itself in the remnant of his lip, which was perfectly soft and pliant. As he was constantly obliged to wear a band around his mouth to confine the saliva, he had lost, from the pressure thus occasioned, the lower incisor and the right cuspid and bicuspid teeth; but the gums and jaw were entirely sound. The object of his visit to me was to get me to make him a new lip, which I accordingly did by the sliding process.

In 1846, I excised the central portion of the lower lip, embracing about one-half of the organ, from a gentleman of the name of Folsome, aged about forty, of Point Commerce, Indiana. The tumour was about the size of a pigeon's egg, hard and firm in its consistence, and the seat of a sharp, lancinating pain; it had begun in the form of a small chap or fissure, which had existed for many years, and had been a source of much annoyance. The general health had never suffered. This gentleman had no return of his disease in October, 1852, when I last saw him.

In the following case, the operation was performed by Dr. B. W. Dudley, of Lexington. The patient was the venerable and much respected Dr. Galt, recently of this city, now of its vicinity. The tumour appeared in 1820, and continued to increase for six years, when it was destroyed by powerful escharotics by the late Dr. Harrison, of Bardstown, Kentucky. The disease seemed to be completely eradicated, but some time after it returned, and continued to harass the patient until May, 1830, when it was excised by Dr. Dudley.

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Since then the lip has remained perfectly well. Dr. Galt is now upwards of seventy years of age.

Professor Eve informs me that he has removed the whole of the inferior lip for what was supposed to be a cancerous affection in a man aged eighty, and that three years afterwards his mouth, cheek, and neck remained perfectly free from the disease. In a case of this affection, operated on by Professor Pope, of St. Louis, the parts retained their healthy condition; but cancer formed in the lung, and destroyed the patient.

The tables of Professor Hamilton, of Buffalo, contain five cases of cancer of the lower lip, all occurring in elderly subjects, and all followed by relapse, either immediately or at a period more or less remote from the operation. In one case, that of a man of forty, the disease began in the form of a wart, and was destroyed by the actual cautery; it returned, and after it had remained as an ulcer sixteen years the potential cautery was applied; it reappeared again in a month, and, spreading rapidly, was removed with the knife. The patient had remained well five years, when Dr. Hamilton reported the case.

In another case, the cancer was seated at the commissure of the lips. The patient was thirty-eight years of age, and the disease had made its appearance two years before in the form of a fissure. The parts were excised, and the gap was closed by anaplasty. Immediate relapse was the result.

There is reason to believe, as I have already stated, that genuine carcinoma of the lip nearly always, if not invariably, returns after ablation. "It is matter of common belief," says Mr. Walshe,* "that the lip is one of the situations in which the excision of cancer may be undertaken with the strongest hopes of success. There can be no question that the prevalent notion on this point is an exaggerated one, and has arisen in consequence of syphilitic sores having been cut away as cancers." Mr. Walshe has in some instances known the disease repullulate, after apparently the most thorough excision, in two or three months; and in a case which he observed in 1834, in the practice of Dupuytren, the progress and fatal issue of the affection were obviously hastened by the use of the knife. Velpeau says† it is important to operate freely and early in cancerous tumours of the lip; otherwise, they return in the neighbourhood

* On the Nature and Treatment of Cancer, p. 258, London, 1846.

† Operative Surgery, by Mott and Townsend, vol. iii. p. 345, New York, 1847.

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and beneath the jaw with frightful rapidity. He relates the case of a robust peasant who died on the ninth day after the removal of a small cancerous excrescence from the lower lip, in whom, although his general health seemed to be good, hundreds of cerebriform granules were found in the liver.

The tables of Mons. Leroy D'Etiolles, of Paris, contain the results of 148 operations for cancer of the lip, of which 126 were performed upon males, and 22 upon females. Of the former, 114 were by excision, and 12 by caustic; the relapses were 15, that is, about one-eighth of the entire number. The 22 other operations were performed with the knife, and were followed by 7 relapses; that is, one-third of the entire number. Thus, it would seem that cancer of the lip is more liable to recur after operation in the female than in the male, in the proportion of upwards of one-half.* It is difficult, however, to say what reliance should be placed upon these tables, or whether they should be received at all as evidence in regard to the absolute and relative liability of cancer of the lip to return after extirpation. In France, to the inhabitants of which they refer, and where their accuracy can be best appreciated, very little confidence appears to be put in them by Mons. Leroy's professional brethren.

A good case of carcinoma of the lip, in which the gap, produced by the operation, was filled by cheiloplasty, and in which there was no return at the end of the third year, is related by Dr. Stout, of Easton, Pennsylvania, in the *Philadelphia Medical Examiner* for January, 1850. The disease had commenced about five years before in the form of a fissure, and gradually involved the whole of the lower lip, extending from one angle of the mouth to the other, nearly as far down as the base of the chin, and spreading over about one-half of the upper lip. The four inferior incisor teeth, together with the corresponding portion of the gum and jaw were removed with the tumour, which was very thick and hard, but not painful. There was no enlargement of the neighbouring lymphatic ganglions. The patient was fifty-three years of age at the time of the operation in November, 1844, and there was no evidence of relapse in April, 1848, when the case was published.

* Amer. Journ. Med. Scien. vol. viii. p. 244, 1844.

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SECT. V.—CANCER OF THE PENIS.

The penis is occasionally the seat of malignant disease, chiefly in aged subjects; and the form in which it most commonly presents itself is the scirrhous. Encephaloid is extremely rare, both as a primary and secondary affection; and, as to melanosis and colloid, I am not aware that any cases of these morbid products have ever been noticed here. We have no statistics of malignant diseases of the penis, throwing any valuable light upon the results of operations undertaken for their relief. In my own practice, I have generally declined interference, for the reason, first, that most of the cases which have fallen under my observation were too far advanced to justify a resort to the knife; and, secondly, because I have never had any confidence whatever in the curative agency of excision in genuine cancer of this or any other part of the body. I have very lately performed the operation upon a gentleman, aged sixty-one, in whom the small size, the recent standing, and the circumscribed character of the tumour, as well as the total absence of lymphatic and constitutional invasion, augured most favourably, but who, nevertheless, was threatened with a return of the disease in less than five weeks. The extirpation in this case was most thorough, the knife being carried through the body of the organ far behind the gland, upon the upper and back part of which the morbid growth, hardly six lines in diameter, was situated.

Under proper treatment—consisting of a purely vegetable and milk diet, mild laxatives, and great attention to cleanliness—patients with scirrhus of the penis will, I presume, generally live quite as long and as comfortably without as with an operation. In several cases, with the history of which I am acquainted, life was not destroyed until more than three years after the appearance of ulceration of the tumour and enlargement of the lymphatic ganglions of the groin; a period seldom attained by individuals undergoing operation.

The tables of Professor Hamilton, annexed to this report, refer to three cases of cancer of the penis in which amputation was followed by relapse, respectively, at six, twelve, and twenty-four months. Dr. Warren's paper contains nothing definite upon the results of operation in the cases therein described. In one of the cases, remarkable for its hereditary character, the man died eight months after the amputation. This patient's father, grandfather, and great-grandfather, all died of cancer of the penis.

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Professor Walshe* affirms that experience strongly exposes the inefficacy of excision in cancer of the penis. "When the disease," says he, "is actually established, removal of the morbid mass with the knife would appear to afford the only chance of saving the patient; and, unfortunately, this chance is a singularly sorry one. General experience corroborates the statement of Mr. Travers, that, 'if the penis be amputated at some distance beyond the hardened part, and before the disease has existed long, the patient *may* escape a return of it; but this is a *rare instance of good fortune.*'" * * "For my own part," continues Mr. Walshe, "I can find no recorded case of permanent recovery after amputation of the penis for unquestionable cancerous disease."

SECT. VI.—CANCER OF THE TESTICLE.

Scirrhus of the testicle is exceedingly infrequent. In fact, almost the only well-authenticated case of this form of carcinoma is that described by Sir Astley Cooper, in his *Lectures on Surgery*, by Mr. Tyrrell. The patient was forty-four years of age, and the organ was extirpated nine months after the first appearance of the disease. The wound healed kindly enough, but the thigh and leg, which were œdematous at the time of the operation, remained swollen, and the man died a month after his return home. Melanosis and colloid rarely attack the testicle, and I do not know of any cases in which the gland was removed for these affections.

The most common form in which malignant disease of the testis presents itself, is the encephaloid, fungoid, or cerebriform. The malady here, as elsewhere, is always fatal when left to itself. The only remedy is amputation, and this, unfortunately, is nearly constantly followed by relapse in a very short time. I have performed several operations for this form of carcinoma, without, I am satisfied, benefiting my patient in the slightest degree, except, perhaps, in so far as temporary relief from pain was concerned. The disease, in every instance, promptly returned, either at the original site or in some other part, and rapidly proved fatal. Mr. Curling states† that he has scarcely known a solitary example of castration having been performed for this affection in which the patient survived for any lengthened period. Of five cases recorded by Sir Astley Cooper, there was not one in which there was not a recurrence of the dis-

* On the Nature and Treatment of Cancer, pp. 411–12, London, 1846.

† A Practical Treatise on the Diseases of the Testis, p. 378, Philadelphia, 1843.

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ease; and Sir Benjamin Brodie, in his large experience, never knew but one patient whose life was prolonged by the operation beyond a few years.

Dr. Otto Baring, of Hanover, in Europe, seems to entertain a more favourable opinion of excision in this affection than the majority of English writers. In his elaborate *Treatise on Medullary Fungus of the Testicle*, published in 1833, and comprising the most complete body of facts that has yet been furnished on the subject, he gives the particulars of several cases in which the operation was performed, with the most gratifying results, by Rust, of Berlin, Langenbeck, of Göttingen, and Hagedorn, of Stade. In two of these cases, a period of five years, in another, of three years, and in a fourth, of two years, had elapsed since the removal of the organ, and yet there was not the slightest appearance of relapse, or of constitutional derangement. Dr. Baring considers castration proper in all cases where the disease is of local origin, or the result of external violence, and where the system remains free from contamination. Even in enlargement of the ganglions of the groin, he thinks an operation not wholly inadmissible; whereas, such a step is altogether unwarrantable when the disorder has extended to the spermatic cord, or affected the constitution.

Professor Pope, of St. Louis, has excised the testicle in four cases for encephaloid. "The only one," says he, "that I know of now, has the other gland affected like the first."

An instance in which the patient, aged thirty-five, has remained well for at least twelve years after the extirpation of an encephaloid testicle, came under the observation of Dr. J. B. Flint, of this city, in 1840, and is recorded in a former volume of the *Transactions* of this Association. Although the wound healed kindly, yet the man continued weakly for many months, but finally recovered under the use of tonics, and was well when the case was reported in April, 1850.

In the table of Dr. Forsyth, accompanying this report, and drawn up at my request, twenty-two out of the twenty-seven cases are stated to have been fatal; of the rest, in one, the disease returned; in one, the result is not given; in another, the patient remained well five months after the operation; in a fourth, the man was enjoying good health at the end of two years; and of the other, no report was made after the first month. Three of the cases are marked as scirrhus, and of these one recovered, that is, he was alive at the end of a month. All the other cases were encephaloid.

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Table of Twenty-seven Cases of Malignant Disease of the

No.	Age.	Residence.	Cause.	Duration.	Progress.	Mode of operation.	Structure.	Result.	Size.	Surgeon.
1		Worcester.				Castration.	Encephaloid	Death		A. Cooper.
2	46	Ramsbury.		4 mo's	Rapid	"	"	"	3 times as large as natural.	Dr. Blackman.
3	40	London.				"	"	"		A. Cooper.
4	48	Rotherham.					"	"		Mr. Lunn.
5	44	Pottenham.		8 mo's	"	"	Scirrhous	"		A. Cooper.
6	25	Cambridge.				"	Encephaloid	"		Okes.
7	5	London.				"	"	Return		Cline.
8	1					"	"	Death		Earle.
9	1			2 mo's	"	"	"	"	Hen's egg.	Langstaff.
10						"	"	"		Cline.
11	27					"	"	"		Cruveilhier.
12	84			10 mo's	"	"	"	Not given	8 times the size of the other testicle	Curling.
13				1 year	"	"		Death		Hunter.
14	28	Edinburgh.		9 mo's	"	"	"	"	Goose egg.	Newbigging.
15	18				"	"	"	"		Dr. Thompson.

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Testicle. By ALEXANDER FORSYTH, M. D., of Louisville.

Authority.	*Post-mortem* examination.	Remarks.
Cooper on Testis, p. 156.	A tumour behind the duodenum, and one in the loins, of a white, fibrous, and brain-like substance, as large as the head of a child; mesenteric glands enlarged; aorta nearly closed by a fungous secretion.	
Cooper on Testis, p. 159.		A few months after the operation, the patient died with legs and thighs swelled, a large tumour in the abdomen, &c.
Cooper on Testis, p. 160.	A large tumour, of a pale white colour, curd-like matter, and of the consistence of cream, filled nearly the whole of the abdominal cavity; the liver contained two white tubercles; thoracic viscera healthy.	This patient died ten weeks after the operation.
Cooper on Testis, p. 162.	Abdominal viscera more or less affected with the same disease.	
Cooper on Testis, p. 169.	No *post mortem* reported.	The testicle in this case presented a hard, white, compact, tuberculated, and, in some parts, very vascular appearance.
Cooper on Testis, p. 179.	The abdominal cavity presented, in all directions, large irregular masses of a soft pulpy structure.	This patient died 18 months after the operation.
Curling on Testis, p. 368.		
Curling on Testis, p. 368.	Brain and other parts similarly diseased.	
Curling, p. 368.	The lumbar glands, lungs, and dura mater were affected with same disease.	
Curling, p. 372.	A large tumour in the loins, which had affected the vertebræ so as to press upon the spinal cord.	The patient died paralytic.
Curling, p. 372.	The sixth and seventh cervical vertebræ, and the posterior extremities of the first and second ribs, diseased in the same way.	This patient also died of paralysis.
Curling, p. 375.		The history of this case is not given complete.
Curling, p. 382.	A large tumour was found in the abdominal cavity, resembling coagulated milk with whey in it; the colon, stomach, and other viscera, were connected by a similar tumour; the liver was filled with small tumours of the same kind.	This was a case of carcinoma of the tunica vaginalis.
Wardrop on Fungus Hæmatodes, p. 134.	Spermatic cord indurated; a large tumour surrounded the aorta and vena cava, of a soft consistence and a dark-brown colour; the liver contained tubercles of a blackish colour; in the pelvis of the kidney, a quantity of purulent matter was found; the lungs were tuberculous.	
Wardrop, p. 139.	Spermatic cord sound; the lymphatic glands along the lumbar vertebræ very much enlarged; when cut into were found of white colour and soft consistence.	This patient died on the tenth day after the operation.

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Table of Malignant Disease

No.	Age.	Residence.	Cause.	Duration.	Progress.	Mode of operation.	Structure.	Result.	Size.	Surgeon.
16	33	Edinburgh		8 mo's		Castration	Encephaloid	Death	Large as two fists.	Wardrop.
17	47	Massachusetts	Fall	30 y'rs	Very slow	"	"			J. C. Warren.
18	27	"	"	2 y'rs	Slow	"	"	"	Large.	J. C. Warren.
19	30			3 y'rs	"	"	"	"		J. C. Warren.
20	35	Louisville		2 y'rs	"	"	"	"	Turkey egg.	J. B. Flint.
21	54	England		9 mo's	Rapid	"	"	Recovery	Goose egg.	Lawrence.
22		"				"	"	Death		Lawrence.
23	1	"		2 mo's	"	"	"	"	Hen's egg.	Langstaff.
24	36	"		1 year	"	"	"	Recovery		Langstaff.
25	2½	"			"	"	"	Death	Goose egg.	Brown.
26	50	"		4 mo's	"		Scirrhous	"		Key.
27	45	"				"	"	Recovery		Key.

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of Testicle—Continued.

Authority.	*Post-mortem* examination.	Remarks.
Wardrop, p. 140.	A chain of glands extended from the internal ring (very much enlarged) to a large diseased mass situated on the lumbar vertebræ; this mass was pulpy—some parts as soft as cream, others firmer than brain.	This testicle weighed two pounds; the whole gland converted into a pulpy mass, none of the natural structure remaining.
Warren on Tumours, p. 331.	No examination of the body. The authority thinks the abdominal organs were undoubtedly diseased, but attributes the death to internal erysipelatous inflammation.	This patient received the injury when 17 years old; the swelling continued occasionally until 4 months before the operation; then became permanent and very painful.
Warren on Tumours, p. 332.	The membranes of the brain covered with lymph; the ventricles contained water; the lungs inflamed, and contained many tubercles; abdominal viscera not diseased.	Seven years before the operation, this patient felt pain in the testicle; had swelling; occasionally after had pain and swelling; 2 years previous to operation swelling became permanent.
Warren on Tumours, p. 334.	The body greatly emaciated; the abdominal cavity contained water; a knot of scirrhous glands extended from the groin to the aorta, which was buried in a mass of enlarged and softened glands; the right lung tuberculous and indurated; left lung partially diseased in same way. Brain not examined.	
Alex. Forsyth.	A large brain-like tumour in the pelvic cavity; a chain of diseased glands reaching from the groin to the stomach; pyloric extremity of the stomach diseased in the same way; the brain and lungs not examined.	This case has never been reported; I am familiar with its history, and have the tumour in my possession.
Lancet, vol. ii. 1846–7, p. 444.		Five months after there was no return of the disease.
Lancet, vol. i. 1829–30, p. 828.	No examination. Thoracic viscera supposed to have been diseased.	This patient lived a year after the operation.
Lancet, vol. i. 1835–6, p. 405.	The absorbent glands in the lumbar region affected with same disease; left lung much diseased in same way; a large tumour on the internal portion of the parietal bone; other viscera healthy.	
Lancet, vol. i. 1835–6, p. 407.		The patient was in good health two years after the operation.
Lancet, vol. i. 1836–7, p. 639.	No examination.	This patient was paralyzed in the lower extremities before death; a large tumour could be felt in the abdomen.
Lancet, vol. ix. 1825–6, p. 138.		This is reported true scirrhus, although no operation had been performed when the case was diagnosed. (Diagnosis no doubt incorrect.)
Lancet, vol. i. 1826–7, p. 14.		This patient (according to Mr. Key) left the hospital in one month, quite well. No report after that.

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SECT. VII.—CANCER OF THE UTERUS.

Perhaps the most common form of carcinoma of the uterus is the scirrhous, and next in point of frequency the encephaloid. What has been called the cauliflower excrescence, by Sir Charles Clark and others, is probably a mere variety of the latter affection, and is, beyond question, of rare occurrence. As to melanosis and colloid, they have never, so far as my information extends, been the subjects of operation in the uterus.

Excision of the uterus for carcinomatous disease has been very seldom performed by American and English surgeons. In Germany and France, on the contrary, it has been resorted to not unfrequently, but with a result so discouraging that it has, of late years, been almost entirely proscribed by conscientious practitioners. The operation is said to have been first performed by Professor Osiander, of Göttingen, who has published nine cases of its successful employment. Dupuytren, also, occasionally executed it, and, it is said, with almost invariable success. But its great advocate, unquestionably, was Mons. Lisfranc, of Paris, whom Dr. Balbirnie, in his enthusiasm, has styled the *apostle* of excision. This surgeon, so celebrated in his day, stated, in 1828, in a paper communicated to the Royal Academy of Medicine, that he had performed this operation, up to that period, on thirty-six individuals, of whom thirty were then well, while three had died, and three were in progress of recovery. One female, operated on some years before, had since become pregnant, and had recently given birth to twins.*

Subsequently to this communication, Lisfranc apprised the profession that he had excised the neck of the uterus, or the entire organ, ninety-nine times; but in this statement he is contradicted by Mons. Pauly, who, after a careful examination of all the facts, declares that he could make out only fifty-three cases. Of the results of these fifty-three cases we have, unfortunately, authentic information of only nineteen. Of these, only one was permanently benefited; four died within twenty-four hours after the operation; twelve had an immediate relapse; and in two others, in which the disease was imperfectly removed, death happened only the more rapidly, on account of the interference of the surgeon.†

* Practical Formulary of the Parisian Hospitals, by F. S. Ratier, M. D. p. 17.
† Churchill on the Diseases of Females, p. 191, Phila. 1843.

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The operation of excision of the neck, or of the neck and body of the uterus, is not only generally useless, as it respects a permanent cure, or even considerable temporary benefit; but it is extremely liable to be followed by immediately fatal consequences, and ought, therefore, on this account, to be abandoned as a common proceeding. The greatest danger to which it leads is prostration, which is often so severe as to lead to speedy dissolution. Very few survive this shock when the entire organ is removed. Another danger is hemorrhage. Out of nine patients operated on by Lisfranc, under the observation of Pauly, six suffered from this cause, and of this number three died within twenty-four hours. Sometimes death is occasioned by pelvic phlebitis, or by peritoneal inflammation. Finally, instances are mentioned where the difficulties were so great that, in attempting to excise the affected parts, the operator has opened the peritoneum and the urinary bladder. The former of these accidents happened to Lisfranc; the latter, to Sauter and Roux.

It was in consequence of the indifferent success following this operation, as a curative agent, and of the great danger attending its execution, that Dupuytren, Osiander, and other surgeons and obstetricians were induced to discontinue it. Even Lisfranc rarely resorted to it in the latter periods of his career.

A case, in which excision of the neck of this organ was performed, is mentioned by Professor Atlee, of Philadelphia, in the sixteenth volume of the *American Journal of the Medical Sciences*. The operation was performed on the 21st of December, 1846, and the woman, who was forty-six years of age, died on the 9th of May, 1847, within a few days after reaching her residence at Pittsburg. The parts were inspected for the last time on the 24th of April, when the wound was found to be perfectly smooth and sound. The general health had not been good during the winter, and the system was evidently in a cachectic condition. It is to be regretted that there was no examination of the body.

Professor Eve, of Georgia, on the 16th of April, 1850, performed the daring operation of removing the entire uterus, through the vagina, for encephaloid disease, in a negress, aged twenty-eight. The woman recovered from the immediate effects of the excision, but died at the end of three months and a week, from a return of the complaint. No *post-mortem* examination was made. An account of this operation, accompanied by two drawings of the morbid specimen, has been published by Professor Meigs, of Philadelphia, in the *American Journal of the Medical Sciences* for October, 1850.

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The following paragraph, by Professor Meigs, in the paper just referred to, has a direct bearing upon the subject of this report: "M. Colombat de l'Isère informs us that the operation—extirpation of the uterus *in situ*—has been executed by Sauter, by Hoelscher, twice by Siebold, and thrice by Langenbeck; four times by Blundell; once by Bauner; once by Lizars; twice by Recamier; once by Dubled; twice by Roux, and once by M. Delpech; while this operation by Professor Eve adds one integer to the whole number, which amounts to twenty operations, *in all of which the result was contrary to the hopes of the surgeons.*"

The subjoined table, drawn up for this report by Dr. William H. Miller, of this city, gives the results of operations in twenty-one cases of cancer of this organ:—

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Table of Twenty-one Cases, showing the Results of Excision of the Neck of the Uterus for Malignant Disease.
By WILLIAM H. MILLER, M. D., of Louisville.

No.	Married.	Single.	Children.	Age.	Temperament.	Residence.	Cause.	Duration.	Symptoms and progress.	Mode of operation.	Result.	Structure.	Surgeon.	Authority.	*Post-mortem* appearance.	Time last seen.
1	1			40	Unknown	England		2 y'rs	Slow; discharge purulent, bloody, and offensive.	Ligature	Partially successful	Cauliflower excrescence	Ashwell.	Ashwell on Diseases of Females, p. 313.		Died of a relapse 2 years afterwards.
2	1		5	33		Scotland		3 y'rs	Slow; discharge watery, bloody and offensive; constant hemorrhage.	Excision	Successful	Cancer	Simpson.	Edinburgh Medical Journal, vol. lv. p. 104.		Shortly afterwards became pregnant.
3		1		25		Bologna			Great hardness and ulceration of the neck.	"	"	"	Francesco Rizzoli.	Ibid., p. 297.		
4		1		33		Brussels	Mental anxiety		Lancinating pain in the uterus; fetid excoriating discharge.	"		"	Lisfranc.	Medico-Chirurgical Review, vol. x. p. 160.		One month.
5	1		6	30		France	Hereditary	1 year	Lancinating pains; hemorrhage, nausea, &c.	"	"	"	"	Ibid.		Two months.
6		1		25		"	Following abortion	8 mo's	Lancinating pains; vegetations; cervix tumefied and indurated.	"	"	"	"	Ibid.		One month.
7		1		22		"		Over a year	Lancinating pains; tumefaction, induration, and leucorrhœa.	"	"	"	"	Ibid.		Four months.
8	1		1	35		"		5 mo's	Progress rapid; induration and ulceration of womb; lancinating pains.	"	"	"	"	Ibid.		One month.
9	1			42		"	Mental affliction	3½ y'rs	Slow; induration and ulceration; fetid discharge; acute pains.	"	"	"	"	Ibid.		Two months.
10	1	1		27		"		5 y'rs	Great pains; yellow skin; general health bad; irregular menstruation; ulceration and induration.	"	"	"	"	Ibid.		Six weeks.
11	1			50		Constance		6 mo's	Copious hemorrhages; acute pain; numerous hard excrescences on the neck; painful and fungous; sanious discharges.	"	"	"	Sauter.	Balbirnie on Diseases of the Womb, p. 328.		Died two months afterwards of indigestion.
12	1			50		England		9 mo's	Ulcerations; hemorrhages; neck of uterus of cartilaginous hardness.	"	"	"	Blundell.	Ibid.	Fungous degeneration of both ovaries; peritoneal redness; rectum and colon distended.	Died a year afterwards of a relapse.

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Table of Excision of the Neck of the Uterus for Malignant Disease—Continued.

No.	Married.	Single.	Children.	Age.	Temperament.	Residence.	Cause.	Duration.	Symptoms and progress.	Mode of operation.	Result.	Structure.	Surgeon.	Authority.	*Post-mortem* appearance.	Time last seen.
13	1		3	50	Nervous	France		8 mo's	Constant pain following cessation of menses; ulceration of a foul fungous nature.	Excision	Successful	Cancer	Recamier.	Balbirnie on Diseases of the Womb. p. 333.		Six months.
14	1		8	42		"			Large tumour on the neck of the womb; callous, bleeding, soft, and granulated.	"	"	Cauliflower excrescence	Duges.	Heming's Translation of Madame Boivin, p. 297.		Died in three months.
15	1		1	30		"		4 mo's	Large tumour on the neck; bleeding and granulated.	"	"	"	"	Ibid. p. 299.		Return of disease a year afterwards.
16	1		3	36		"			Progress rapid; cervix voluminous, indurated, and of a deep red colour.	Caustic	"	"	"	Boivin and Duges, p. 260.		Died in a month of peritonitis.
17	1			34		"			Cauliflower excrescence, with a sero-sanguineous discharge.	Knife	"	"	"	Ibid. p. 300.		Living four years afterwards.
18	1		1	27		"		1 year	Great emaciation; fever; yellow skin; uterine discharge of a cancerous odour.	Excision	"	Cancer	Lisfranc.	Balbirnie on Organic Diseases of Womb.		Died in five months.
19	1		1	54		"			Has had cancerous symptoms for years; fungous growth on the neck, with profuse discharge.	"	"	"	Dupuytren.	Ibid.		No return of disease eight years afterwards.
20	1			50		England		8 mo's	Cachectic consistence; os uteri ulcerated, and of a stony hardness; hemorrhages.	"	"	"	Blundell.	London Med. Gaz. August, 1828.	Fundus of bladder cancerously diseased: death caused by constipation.	Died in one year.
21	1		6	60		France			Profuse fetid discharge; ulceration and induration of neck; cylindrical fungoid tumour attached to the womb.	Ligature	"	"	Recamier.	Medico-Chirurgical Review, 1826, p. 266.		Died one month afterwards.

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SECT. VIII.—CANCER OF THE ANUS AND RECTUM.

The most common form of cancer of these outlets is the scirrhous; encephaloid is observed only occasionally; and in one or two instances they have been known to be the seat of colloid. Rokitanski* and Curling† allude to the occurrence of epithelial cancer of these parts; but of this I have never seen an instance, and the affection is unquestionably very rare.

Cancer of the anus and rectum does not differ, in respect to its curability by excision, from cancer in other regions of the body. In my own cases, I have invariably declined all interference of the sort, under the conviction, first, that it could not possibly result in any permanent benefit; and, secondly, even supposing that it would be followed by a prolongation of life, which, however, it rarely is, the advantage thus gained would be more than counterbalanced by the distress and inconvenience occasioned by the loss of the sphincter muscle. Lisfranc, of Paris, and Dieffenbach, of Berlin, have been, in modern times, the warmest advocates of excision of cancerous tumours of the anus and rectum. The former of these celebrated surgeons performed the operation repeatedly, but with what success cannot be determined, because no reliance is placed by any one in his statements. Dieffenbach‡ asserts that he had recourse to the knife in not less than thirty cases of this kind. In one, the disease reappeared within four weeks, and in some others in three months; but the larger proportion continued well many years afterwards. Who will believe that all these cases were cases of real cancer, or even epithelial formations? Certainly, no one who knows anything of these complaints. We must, therefore, conclude that Dieffenbach was mistaken in his diagnosis; or, what would be less agreeable, that he has wilfully published what is untrue.

SECT. IX.—CANCER OF THE BONES.

The bones, as is well known, are liable to malignant disease in various forms, especially the encephaloid and colloid. In what is

* Manual of Pathological Anatomy, translated for the Sydenham Society, vol. ii. p. 100.

† Observations on the Diseases of the Rectum, p. 116, Lond. 1851.

‡ Brit. and For. Med.-Chirurg. Review, Oct. 1850.

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denominated *osteo-sarcoma*, the textures characteristic of these two deposits often exist, either alone, or in combination. The case which I am about to describe appears to me to have been essentially of a colloid nature, though upon this point we have no positive information. The case has become memorable in the annals of surgery, not so much on account of its pathological peculiarities, as by reason of the daring character of the operation performed for its relief.*

The patient, Horace Wheeler, aged twenty-seven, of Randolph, Vermont, had an osteo-sarcomatous tumour, larger than a hen's egg, and involving the metacarpal bones of the thumb and index finger, which Professor Mussey removed in 1817. In September, 1831, more than thirteen years afterwards, the arm was amputated at the shoulder-joint, the malady having seized upon the radius and the upper half of the humerus, where it formed a very large swelling. The patient had suffered excessive pain during the last few years, the shoulder-joint was very stiff, and the soft structures in the neighbourhood were much thickened and indurated. The wound healed readily, and the parts appeared sound; but, subsequently, several abscesses formed in the stump, and some months elapsed before the cure was completed. Five years afterwards, the disease returned in the scapula and clavicle; and, in September, 1837, Professor Mussey performed the daring feat of removing both these bones in their entire extent. The immense wound united by adhesion, and became consolidated without the formation of a teaspoonful of pus. In the summer of 1852, Professor Mussey heard from his patient, who sent him his compliments, informed him that he was perfectly well, and that he intended to visit Ohio this summer, when he hoped to pay him his respects.

The tumour, in the last operation, contained a quantity of thick glue-like substance, enveloped by a mass of cartilage, interspersed with small osseous deposits. A part of the clavicle and more than three-quarters of the body of the scapula, with its spine, acromion, and coracoid process, exhibited a coarse spongy texture, with exuberant bony vegetations. The appearances were similar on the humerus, and on the metacarpal bone of the thumb.

Mr. Stanley, of London, who has published the best work on the diseases of the bones in the English language, thinks that encepha-

* Amer. Journ. Med. Scien. vol. 21, p. 390, 1837; Transactions Amer. Med. Association, vol. iii. p. 331, 1850

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loid of the osseous tissues is probably always at first local, and that it continues so until the solids and fluids become affected by the absorption of the morbid matter, when secondary tumours form in other parts of the body. "If, therefore," he remarks,* "the original encephaloid tumour is removed before the blood has become poisoned, the disease will not return; but, if the morbid matters have already entered the circulation, it will be reproduced." In consonance with these views, Mr. Stanley advises that the limb upon which the tumour is situated should be amputated as early as possible, due regard being had, of course, to the condition of the neighbouring structures and the system at large, for where these are involved all interference of the kind is improper. I imagine, not so much from personal experience as from a careful study of many of the recorded cases of malignant disease of the bones, that the morbid action will, in the generality of instances, return more or less promptly after removal of the affected structures, no matter how early and how effectually this may be done. If I mistake not, Mr. Stanley's own cases, those, for example, adduced under the name of osteoid tumours, which he singularly enough separates from encephaloid, strikingly confirm the truth of this position. That the disease here is as much an affection of the constitution as when it occurs in the soft parts, is shown by the fact that it is almost always attended with contamination of the adjacent lymphatic ganglions, and involvement of the internal organs.

GENERAL CONCLUSIONS.

From the facts and statements which have now been presented, embracing the opinions of many of the most intelligent, experienced, and distinguished practitioners in different ages and in different parts of the world, the following conclusions may be legitimately deduced:—

First. That cancerous affections, particularly those of the mammary gland, have always, with a few rare exceptions, been regarded by practitioners as incurable by the knife and escharotics. This opinion, commencing with Hippocrates, the father of medicine, has prevailed from the earliest records of the profession to the present moment. Nature never cures a disease of this kind; nor can this be effected by any medicine or internal remedies known to the profession.

* A Treatise on Diseases of the Bones, p. 151, Phila. ed. 1849.

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Second. That excision, however early and thoroughly executed, is nearly always, in genuine cancer, followed by a relapse at a period varying from a few weeks to several months from the time of the operation.

Third. That nearly all practitioners, from the time of Hippocrates to the present day, have been and are still averse to any operation for the removal of cancerous tumours after the establishment of ulceration, rapid growth, firm adhesions, organic change in the skin, lymphatic invasion, the cancerous dyscrasy, or serious constitutional derangement; on the ground that, if had recourse to under these circumstances, the malady almost inevitably recurs in a very short time, and frequently destroys the patient more rapidly than when it is permitted to pursue its own course.

Fourth. That in all cases of *acute* carcinoma, or, in other words, in all cases of this disease attended with very rapid development and great bulk of the tumour, extirpation is improper and unjustifiable, inasmuch as it will only tend to expedite the fatal result, which, under such circumstances, always takes place in a very short time.

Fifth. That all operations performed for the removal of encephaloid cancer and its different varieties, are more certainly followed by rapid relapse than operations performed upon scirrhous or hard cancer.

Sixth. That in nearly all operations for cancerous diseases hitherto reported, the history has been imperfectly presented, being deficient in the details which are necessary to a complete and thorough understanding of the subject in each case. This remark is particularly true in reference to the diagnosis of the malady, the minute examination of the morbid structure, and the history of the case after the operation, as to the period of relapse, the time and nature of the patient's death, and the result of the *post-mortem* examination.

Seventh. That cancerous affections of the lip and skin, now usually described under the name of cancroid diseases, are less liable to relapse after extirpation than genuine cancerous maladies, or those which are characterized by the existence of the true cancer-cell and cancer-juice.

Eighth. That although practitioners have always been aware, from the earliest professional records, of the great liability of cancer to relapse after extirpation, a great majority of them have always been, and still are, in favour of operation in the early stage of the disease, especially in schirrus, before the tumour has made much

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progress, or before there is any disease of the lymphatic ganglions, or evidence of the cancerous cachexy.

Ninth. That many cases of tumours, especially tumours of the breast and testicle, supposed to be cancerous, are in reality not cancerous, but of a benign character, and, consequently, readily curable by ablation, whether effected by the knife or by escharotics. It is to this circumstance that we must ascribe the astonishing success which is said to have attended the practice of Hill, of Scotland, Nooth, of England, and Flajani, of Italy.

Tenth. That all operators insist upon the most thorough excision possible; removing not merely the diseased mass, but also a portion of the surrounding and apparently healthy tissues, as well as all enlarged and indurated ganglions.

Eleventh. That the practice has always prevailed, and still obtains, to save, if possible, a sufficient amount of healthy integument to cover the wound, and to unite, if possible, the wound by the first intention; on the ground that these precautions will tend much to retard, if not to prevent, a recurrence of the disease.

Twelfth. That much stress is laid by writers upon a properly regulated diet, and attention to the bowels and secretions after operation, as means of retarding and preventing relapse.

Thirteenth. That there is no remedy, medicine, or method of treatment which has the power, so far as we are enabled to judge of its virtues, of preventing the reproduction of the morbid action after operation, no matter how early or how thoroughly it may be performed.

Fourteenth. That life has occasionally been prolonged, and even saved, by operation after relapse, as in some of the remarkable cases mentioned in a previous part of this report; but that, as a general rule, such a procedure is as incompetent to effect a permanent cure as a first extirpation.

The following points may be considered as of an unsettled character; at all events, opinion respecting them is much divided, and farther observation is necessary before they can be positively determined, either affirmatively or negatively.

First. Excision is of doubtful propriety in all cases in which the disease is of hereditary origin, or where it occurs in several members of the same family.

Second. It is doubtful whether an operation should be performed when the patient is very young and the disease is of rapid growth.

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There is reason to believe that surgical interference, in such a case, will only expedite the fatal issue, which is generally inevitable.

Third. It is problematical whether an operation should be performed when the disease is attended by suppression of the menses, or by great irregularity of this discharge.

Fourth. Not a few surgeons regard a resort to the knife as of questionable efficacy when there is a quickened state of the pulse, occasioned by the local irritation.

Fifth. There appears to be no general agreement among surgeons as to whether extirpation is proper when there are two or more coexistent and accessible cancerous tumours.

Sixth. It is supposed, but the fact is not established, that excision of carcinomatous tumours only tends to hasten the patient's death.

Seventh. It is doubtful whether, as has been asserted by different surgeons, the prospect of a permanent cure is greater, all other things being equal, after an operation on an old cancer, than after an operation on a cancer of recent standing.

Eighth. It has been stated by writers of great respectability, among others by Dr. Macfarlane, of Glasgow, that, in robust women of sanguine temperament, the reappearance of cancerous disease, and its subsequent progress, are more rapid after operation than in nervous or lymphatic persons; an assumption demanding verification.

Ninth. It requires to be proved whether excision ought to be performed in the ulcerated stage of malignant disease, as a means of prolonging life and of procuring comparative relief from suffering.

In bringing my labours to a close, I feel conscious that I have added nothing whatever to our previous information of malignant diseases. Nor, in truth, could this have been reasonably expected. My sole aim has been to sum up our knowledge upon the subject; to construct, as it were, a mirror which should reflect the practice and opinions of our predecessors and contemporaries, and thus serve as a guide to future travellers in the same path. In a word, my object has been to show, not only what has been done, but what remains to be done. The facts which I have collected, if useful at all, are so rather in a negative than in a positive point of view. Our knowledge of the results of surgical operations in malignant affections is in a state of transition, which has much to hope from the future, but can gain little, if anything, from the past. What the microscope and animal chemistry, guided by the hand of modern science, may do for the subject, it would perhaps, be premature to predict; already they have

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rendered the cause essential service, and it would certainly be unphilosophical to suppose that they are incapable of affording farther light.

Finally, in drawing up this report, I have rarely appealed to my own experience, believing that, even if it were fully exposed, it could add nothing of real value to the general stock of our knowledge upon a subject respecting which so much has been written by others. In point of number, indeed, my own cases could not be put in competition with the extensive scale of facts brought forward by some of the surgeons, the results of whose labours it has been my duty to bring before the Association. I have little, indeed, I might say, no confidence in any operation for malignant diseases, except the cancroid varieties; and I have for years past, as Professor of Surgery in the University of Louisville, deemed it my duty to discourage a resort to the knife in all cases of the kind, especially in scirrhus and encephaloid of the mammary glands, ample experience having satisfied me of the utter futility of such an expedient, however early and efficiently employed. In cutaneous cancer, on the other hand, my rule has always been to operate, provided the disorder has not advanced so far as to preclude the possibility of removing the whole of the morbid growth; and provided, also, that there is no evidence of constitutional infection. To this rule I shall rigidly adhere, unless my own experience, or the experience of others, shall show me the impropriety of it, when I shall most cheerfully abandon it.

S. D. GROSS, *Chairman.*

Louisville, April 20, 1853.

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REPORT

ON THE

EPIDEMICS OF TENNESSEE AND KENTUCKY.

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REPORT ON THE EPIDEMICS OF TENNESSEE AND KENTUCKY.

THE Committee, to whom was referred the duty of investigating and reporting upon the epidemics of Tennessee and Kentucky, respectfully report:—

That, in the discharge of their duty, they have used all practicable diligence in procuring contributions from all parts of the two States composing their district, but have not received as much aid as they could have desired. The Committee believe that there has really been but little epidemic influence at work within their district. Epidemic cholera, nevertheless, is reported to have prevailed to a considerable extent in three localities; but from these they have in vain endeavoured to procure any information. They, however, have the satisfaction to believe that considerable interest has been awakened among the physicians of the district by the efforts made by this Association to procure satisfactory and reliable information from all parts of our country, upon this important subject.

Your Committee would wish to say a word upon the plan adopted for this report. The plan of last year's report was decided upon after much reflection, when it was confidently presumed that the labours of the Committee would terminate with the then current year.

It was seen to be liable to two very grave objections. 1. It separated the topography and meteorology of a place from the diseases which had prevailed there. 2. It prevented the contributions by individuals from being presented unbroken, and as distinct as the Committee would desire, and, as they doubt not, would have been preferred by the contributors.

These objections, great as they were felt to be, were considered as more than counterbalanced by others which the plan adopted made available. Although the topography and meteorology were sepa-

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rated from the diseases, the topography and meteorology of every place from which a contribution had been sent were arranged under their proper heads, and could be readily consulted in reference to any disease which might be under consideration. The necessity of detailing the same symptoms of the same disease, as it appeared at different points, was avoided. The treatment pursued by different gentlemen in the same disease, whether in the same locality or in different regions, was brought into immediate juxtaposition. By the construction of tables, which should show the liability of different ages, sexes, and colours to each disease, it was hoped a foundation would be laid upon which a superstructure of much useful information might be erected. It was thought, too, that this plan would greatly aid such persons as might wish to refer to the report, touching any particular point But a paramount consideration was the saving of space. The reflection arose, that the Association has organized twenty-seven committees to report upon as many distinct subjects. Eight of these committees are to report upon the epidemics occurring throughout the whole United States. It is, therefore, of the utmost importance that these reports should condense into the smallest possible compass, consistent with perspicuity, the information derived from such an extent of country.

If these reasons determined the form of the report for a single year, they apply with much greater force to a committee which shall make several consecutive reports. For this reason, also, so much as relates to topography, symptoms, &c., that was reported last year is omitted this.

TOPOGRAPHY.

In the report made to the Association in May, 1852, this Committee gave some account of the topography of Memphis, Tenn., with which they had been favoured by Professor Shanks of that city. They now add some additional information upon that important point, furnished by Dr. Geo. R. Grant, of the same city.

Memphis is situated on a high bluff, some fifty or more feet above the high-water mark of the Mississippi River, one hundred and twenty feet above low-water mark, in lat. 35° 8′ N. The soil upon which the city is built, as well as the soil of the county of which it forms a part, is purely alluvial, of different depths in different places, reposing on a bed of sand, containing, in many localities, the remains of testaceæ of former times. In digging wells, no rocks of

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any kind are met with. Water is generally found upon reaching the sand-bed. This water is more or less impregnated with the carbonate of lime, which it derives from the decomposing shells among which it percolates.

For some years past, much labour has been expended in grading the streets, by which very many reservoirs or artificial ponds have been created, no attention being paid to drain off the water from the numerous basins formed by the embankments. Besides many stables for private use, in some of the principal streets, and in the most populous parts of the city, are ranged, in pretty close proximity, many *livery stables*. On the confines of Bayou Gayoso, a natural sewer, which runs from near the lower edge of the city, and empties into Wolf River, at the upper end, and which *ought to be made* of incalculable advantage for drainage, are many cattle and slaughter-pens. Add to these, along the alleys and on the back parts of lots are thrown all sorts of filth. "On inspecting these alleys, on which the temples of Cloacina are mostly located, we shall have presented to our sight the disgusting spectacle of accumulated 'night soil;' and the olfactories will be greeted with odours which remind one of neither 'the sweet scents of Arabia' nor of 'the pure waters of Helicon.' . . . It may be safely questioned whether in this form they are as prejudical to health as the same materials when accumulated in the narrow and shallow pits required for their concealment by the laws of the city. Pits nine feet *deep* and three or four *wide*, walled in with bricks and mortar, only serve to place the effete matters which they contain sufficiently near the surface to undergo rapidly the putrefactive process in warm weather, whilst the chance of speedy desiccation is prevented by the fluids in which they are kept constantly immersed." The city of Memphis contained, by the census of 1850, 6,369 whites, 2,471 blacks; total, 8,840. Something more than one-fourth of the whole population being blacks. The present population is estimated at 12,000.

OLDHAM COUNTY, KY.—The epidemics which occurred in the practice of Dr. John Swain were limited to a small space—a circle whose radius is half a mile would cover nearly the whole district. A general description of the county is contained in the report of '52. At present, the space in which the epidemics prevailed alone will receive attention. A narrow elevation of land, commencing at Ballardsville, and running out some ten or more miles in a south-westerly direction, with a few slight interruptions, is the dividing ridge between Floyd's and Curry's Forks. At the point of disease, Curry's

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Fork runs within half a mile of the crest of this ridge, and washes its base on the W. and N. W. slope. From the sides of this hill, innumerable springs, strongly impregnated with carbonate of lime, break forth, which supply the residents with water. The branches from these springs silently worm their way into the little pools or basins found in the bed of the creek. From the middle of July to the end of September, there was no washing-rain, these pools remaining stagnant. From the facts that the epidemics were confined to a very narrow space; that the most and the worst cases occurred at and near the centre; that they diminished in virulence and in number as they approached the periphery; that the weather was hot and dry; Dr. Swain was well satisfied all this evil was occasioned by the evolution of malaria in and near this creek. There are two whites to one black in this county.

Harrison County, Ky., from which contributions have been furnished by Drs. Addams, Dille, and Hawkins, is situated in the limestone blue grass region, mentioned in the report for 1852, and adjoins Bourbon on the north and Scott on the east. Neither of the gentlemen named has given the topography of his especial locality. There are rather more than two whites to one black in the county.

Floyd County, Ky., from which we have an interesting contribution on dysentery, is situated in the eastern edge of the State; is mountainous, and thinly settled. There is no topography furnished; but it is presumed to be in a freestone region, and hilly. There are twenty-six whites to one black in the county.

Casey County, Ky., is a thinly settled county. Nothing is known of its topography; but being situated on the head waters of Salt and Green rivers, it is presumed to be hilly and broken. There are eight and a half whites to one black in this county.

Edmonson County, Ky., is another thinly settled county; the country broken, and hilly. There are eleven and one-fifth whites to one black in this county.

From Dr. W. W. McNally we have the following description of Lincoln County, Tenn.:—

Lincoln County, Tenn., is thirty-five miles long by twenty-five miles wide, and traversed its whole length by Elk River, a stream of some importance, but navigable for steamboats but a few miles from its mouth. It is also intersected by a great number of creeks, large and small, which flow north and south from the high land, and empty themselves into Elk River. The country is exceedingly broken by ranges of hills, extending, on the north side of the river, from Elk

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Ridge, the dividing ridge between Duck and Elk rivers, to said river, and from Pea Ridge, a branch of the Cumberland Mountains, on the south.

The *soil*, on the north of the river, and from one to three miles on the south, is a rich *alluvium*, and is very fertile; that of Pea Ridge, as it is called, is sandy and gravelly, it being also poor and unproductive. The hills are composed principally of blue limestone rock, though most of them are covered to a considerable depth with the same kind of rich alluvial soil of which the valleys are composed.

A great number of fossils are found in nearly all parts of the county, though the variety is not very extensive. There is, however, quite a number of varieties of corals, encrinites, pentremites, and shells to almost any amount. Indeed, many of the rocks appear to be almost entirely composed of them. The limestone rocks are all of secondary formation. But few fossils are found on Pea Ridge; they are found, however, on the tops of the *highest hills*, on the north of the river.

The water in the limestone districts is all of a *strong limestone* character. On Pea Ridge, it is *freestone*.

Diseases.—Until within the last two or three years, *bilious remittent*, and *intermittent*, and *congestive fevers* were the principal diseases which visited this county, especially in the summer and fall. They were epidemic every year, *in every part of the county*. Pea Ridge was more exempt than any other part; but occasionally they prevailed there as extensively as elsewhere. But for two years past they have been only partially so. During last summer and fall, they prevailed pretty extensively in those portions of the county *not visited by dysentery and typhoid fever*.

Fayetteville, the principal town of Lincoln County, Tenn., is situated about one-third of a mile north of Elk River, on high ground, and measurably beyond the reach of any malaria which may arise from that river, as is shown by its remarkable exemption from malarious diseases, they having never prevailed there to any extent. In this town, having a population of about 900, there were at least one hundred cases of dysentery, and not more than *five* deaths; a smaller number of deaths in proportion to the cases than occurred in any section of country which I know of.

Dr. Thomas Lipscombe, a member of this Committee, furnishes the following account of Bedford County:—

BEDFORD COUNTY, TENN.—The geographical position of Bedford

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County, of which Shelbyville is the county seat, and near the centre, is between latitude 35° and 36°, longitude 9° and 10°, and partakes of the general character of the middle portion of the State, in geological formation. The same kind of limestone exists here as is found in Kentucky, and in the eastern and middle portions of this State. The county is nearly square, being some twenty-five miles from east to west, and between twenty-five and thirty from north to south, and is pretty well watered. Duck River runs through the county from east to west, though not in a direct line, and affords sufficient water all the year for manufacturing purposes. The course of this stream furnishes a large amount of rich alluvial soil, a considerable portion of which is every few years inundated by freshets in the river. Several creeks of large size, also, are bounded by soils of unusual fertility, and they also occasionally overflow the low lands along their margins. During the years 1840–3, this was especially the case, subjecting the inhabitants living adjacent to them to intermittent, remittent, and bilious fevers. Since that time, the overflows have been less frequent and less extensive, either on account of a general enlargement of the mean dimensions of the channel of the stream, or a less amount of rain falling during that period. The eastern, south-eastern, southern, and south-western portions of the county are much more undulating, and even broken, and much more bountifully supplied with springs and running streams than the other parts of the county that present a more level surface. These hills and ridges abound in blue limestone, and consequently the springs and fountains emerging from their bases, and the creeks flowing along their vales, are strongly impregnated with lime. The hills above alluded to present on their surface an abundance of small stones of the silicious character. Where they are not too steep for cultivation they are very productive, and repay the husbandman with most abundant crops. And what is a little strange to me, they are as productive as the rich alluvial hollows, and support vegetation much better during protracted droughts than the lowlands. Some fossils have been found, and shells of various descriptions; though from want of attention to this subject, the variety is not very extensive. The barren lands of Coffey and Franklin counties join this county on the east and south-east, and constitute the table-lands of Cumberland Mountain. The streams of water to which they seem to give origin are beautifully transparent, and are reputed to be freestone. Whether this be true or not, I cannot say; but they are evidently much less impregnated with lime than the springs and

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streams peculiar to this county. In the northern and western portions of our county, the springs and streams are less abundant, and consequently there is a greater necessity for wells, in which good water is usually attained at an average depth of from thirty to forty feet. In this part of the county, in former years, intermittents, remittents, and bilious fevers were more prevalent than in the more broken parts of the county. But, for the last nine or ten years, these types of fever seem to have been gradually disappearing, and the *enteric* or *typhoid fever* to have taken their place.

Shelbyville, the county seat of Bedford County, is situated immediately on the north bank of Duck River, very near the centre of the county, and contains 1,500 inhabitants; and, since the severe and very fatal visitation of cholera in 1833, has been more healthy than the surrounding country. The water used by a large majority of the inhabitants is supplied by two never-failing springs, which are strongly impregnated with lime. A few families have wells, and a few others have cisterns. The cisterns have been too recently introduced among us for an estimate to be formed as to whether persons using water from them or from the springs enjoy the greatest exemption from disease. But in the neighbouring city of Nashville, during the prevalence of cholera in 1848, 1849, and 1850, families who used cistern water had fewer symptoms of the disease, and escaped the epidemic to a greater extent than those who used strong limestone water.

METEOROLOGY.

At MEMPHIS, Tenn., the state of the weather in 1852 differed very remarkably from that of 1851; not so much, however, in temperature, as in the quantity of rain which fell. In 1851, the quantity of rain was probably not far from 34 or 35 inches, whilst in 1852 it was 46.78 inches. It is doubtful whether this latter quantity, though more than one-third greater than that of 1851, was equal to the average. But the difference in the quantities which fell in the hot months was still more strongly marked. In June, July, August, and September, of 1851, but *four* inches of rain fell, whilst in the same months of 1852, *fourteen* inches fell.*

* It is unfortunate that figures, which it is said never lie, cannot always be relied on. In a paper (*Report on Health, &c.*, of Memphis, by C. T. Quintard, M. D.) which has just come to hand, the quantity of rain which fell at Memphis, in 1851, is stated to have been 32.89 inches; that for 1852, 40.85 inches. By comparing that table

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The greatest height of the barometer during 1852, was in January, 31.01 inches; the least was in April, 29.00 inches. Annual range, 2.01 inches.

The greatest heat was in July, when the thermometer rose to 99°. In August, June, September, and May, it was at 97°, 96°, 95°, and 91°, respectively. Greatest cold in January, when the thermometer fell to 2°. Thus giving an annual range of 97°.

For more minute information refer to the accompanying tables.

TABLE A.

Condensed Table of Meteorological Observations made at Memphis during 1852.

1852.	BAROMETER.			THERMOMETER.			RAIN.
Months.	Maximum.	Minimum.	Monthly mean.	Maximum.	Minimum.	Monthly mean.	Quantity inches for each week.
January	31.01	29.23	30.12	66	02°	38°	1.60
February . . .	29.79	29.21	29.50	65	32	51	6.14
March	29.78	29.13	29.45	87	25	56	2.23
April	29.58	29.00	29.29	85	37	61	3.68
May	29.75	29.32	29.53	91	45	68	4.43
June	29.77	29.32	29.54	96	50	73	2.51
July	29.70	29.45	29.57	99	61	80	1.83
August	29.70	29.39	29.54	97	55	76	6.28
September . . .	29.68	29.38	29.53	95	48	71	3.55
October	29.78	29.22	29.50	88	40	64	2.00
November . . .	29.74	29.12	29.43	76	28	52	4.97
December . . .	29.84	29.12	29.48	70	28	49	7.66

There fell three snows in January, one on the 18th of February, and one on the 17th of March.

Mortuary tables. Table B., showing the aggregate number of deaths for each month; the sexes of the whites being distinct.

with the one contained in this report, some very remarkable discrepancies are observed. In April, 1852, one gives the quantity as 3.68 inches; the other, as 1.68. In July, one gives 6.28 inches; the other, 4.28. In September, one gives 3.55 inches; the other 1.65. Total amount, according to one, 46.78 inches; according to the other, 40.85 inches. Yet both sets of tables were prepared from records kept at the Navy Yard! Both sets of tables agree in the quantity of rain which fell in June, July, August, and September, 1851; but differ nearly *four inches* in the quantity which fell in the corresponding months of 1852.

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Table B.

Aggregate number of Deaths in each Month.

Months.	White males.	White females.	Coloured.	Total.
January	12	14	12	38
February	18	10	4	32
March	9	14	7	30
April	20	8	7	35
May	28	16	11	55
June	54	36	29	119
July	44	37	23	104
August	34	11	13	58
September	36	18	13	67
October	34	12	18	64
November	32	14	10	56
December	24	16	8	48
Total	345	206	155	706

Of these there died of children 5 years of age and under	263
From 5 to 10 years	29
From 10 to 20 years	57
From 20 to 30 years	131
From 30 to 40 years	87
From 40 to 50 years	45
From 50 to 60 years	24
From 60 to 70 years	9
Over 70 years	13
Persons whose ages are not recorded	50
Total	706

Besides the above-mentioned 706 deaths, 114 are reported as having died at the hospital, which, though situated beyond the limits of the city, is principally furnished with patients from it.

Table C, compiled from the records of the Board of Health, exhibits at one view the number dying from each of the principal outlets of life, as also the months in which each was most fatal.

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TABLE C.

1852. DISEASES.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total.
Cholera	1	...	...	...	8	30	16	...	6	3	11	2	75
" infantum	...	...	...	1	...	1	...	...	...	1	1	1	5
" morbus	...	...	...	1	1	2	1	...	...	5	5	...	15
Consumption	3	3	2	3	4	2	9	7	6	5	5	5	54
Croup	2	...	...	...	...	...	1	...	...	1	1	1	6
Diarrhœa	1	...	2	1	9	8	11	1	4	8	6	3	54
Dysentery	...	1	...	...	...	3	...	1	1	3	2	2	13
Fever	...	...	...	...	...	...	3	...	...	...	...	...	3
" bilious	1	...	...	...	...	3	2	3	2	1	1	...	13
" congestive	3	1	1	1	1	...	3	2	3	4	3	1	33
" intermittent	...	...	...	...	...	...	1	2	1	3	...	...	7
" typhoid	1	...	2	2	1	6	5	10	1	6	4	3	41
Gastro-enteritis	...	...	...	...	...	2	3	1	1	...	...	...	7
Inflammation of brain*	1	2	1	1	1	7	9	2	1	2	...	1	28
" bowels	1	2	...	2	...	7	...	1	1	2	...	1	17
Measles	...	...	1	1	8	21	6	4	...	...	...	...	41
Pleurisy	1	...	...	1	...	...	...	...	...	...	...	...	2
Pneumonia	5	3	4	3	1	...	...	...	...	2	2	6	26

These tables afford matter for very serious reflection, when taken in connection with the condition of the city. But, as these reflections apply more legitimately to the subject of general hygiene than to the duties of this Committee, they are not followed up.

Dr. Swain, of BALLARDSVILLE, OLDHAM Co., Ky., has kindly furnished a meteorological table for six months—March to August—which embraces the most important period of the year, as respects disease. It is the more valuable from its being kept in a small village, in a rural district.

TABLE D.

Observations taken at Ballardsville, Oldham County, Ky., 1852. *Fahrenheit, under shelter.* By JOHN SWAIN, M. D.

	Minimum.	Maximum.	Range.	Mean temperature.	Mean temperature at sunrise.	Mean temperature at meridian.	Coldest day.	Mean temperature of coldest day.	Warmest day.	Mean temperature of warmest day.	Day of minimum.	Day of maximum.
March,	22	74	52	52.02	51.00	51.11	20th	27.1	25th	67.2	2)th	26th
April,	39	78	39	52.02	49.01	55.02	7th	48.0	30th	74.0	6th	30th
May,	48	78	30	65.08	62.04	69.12	20th	54.2	29th	73.1	2)th	29th
June,	54	84	30	70.01	65.27	78.13	10th	59.2	17th	77.2	1)th	15th
July,	60	92	32	76.00	62.00	81.12	17th	70.4	8th	82.2	8th&22d	2d
August,	64	82	18	74.16	71.15	74.00	1st	65.0	23d	78.0	1st	19th

* We have been informed on reliable authority that, out of respect to friends, under this head are placed many cases dying of *delirium tremens*.

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March.—There were 6 rainy days, and 3 days of snow. 16 days wind southerly; 13 days wind northerly; and 2 days east wind.

April.—There were 12 rainy days. Snow on the 5th. 13 days southerly wind prevailed; 12 days northerly wind; 4 days easterly wind; 2 days west wind.

May.—There were 7 days of rain. 15 days southerly wind; 16 days northerly wind. Clouds principally cinus and cinostratus.

There was no note, or not sufficient to make statements, for July and August.

At FAYETTEVILLE, LINCOLN Co., Tenn., the summer was dry and moderately warm; there being but few excessively hot days. The fall was somewhat wet, but not remarkably cool.

In SHELBYVILLE, TENN.—During the winter of 1850–1, as also of 1851–2, we had a less amount of rain than had usually fallen during our winters. This had been the case also during the summers intervening. The winter of 1850–51 was dry and mild; the winter of 1851–2 was dry, and not intensely cold until in January, when the mercury fell to 8 or 10 deg. below zero. This was the coldest weather experienced here since January, 1832, just twenty years before. The spring and summer of 1852 were also dry and cool; the summer very dry. Before particularizing the diseases of last year, I may be indulged in a few remarks of a somewhat general character, on the diseases of former years. For many years, even from the earliest settlement here, diseases of supposed or accredited miasmatic origin, prevailed extensively in this village, as well as through the greater portion of the counties, especially along the courses of the streams, and in many other places; for in nearly all parts of the county, the process of clearing land, deadening timber, &c., was regularly carried on; which furnished abundant materials for the generation of miasma. For a long term of years, fevers of almost every hue were rife from June until November; generally, most prevalent in September. During 1835, all the different types of fever supposed to be of miasmatic origin were very prevalent; we had continued, remittent, and many varieties of intermittent, including the ordinary forms of quartan, tertian, and quotidian; and well-marked cases of true congestive or pernicious fever, and the syncopal and apoplectic forms, as so well delineated by Alibert. During that year, our village furnished some examples of these different types. During this period, and down as late as about 1846, the general type of disease was sthenic; since 1845 or 6, this characteristic of disease has been yielding to the opposite or asthenic form. Since that

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period, we have had more or less enteric or typhoid fever to contend with; and less and less of the old-fashioned bilious, remittent, and intermittent. Still, this change has not been universal or absolute, as we still have a few cases each year of the former type, and others of a mixed variety. As late as March, 1852, I treated a case of pneumonia in a gentleman, aged fifty years, considered delicate, and yet he required as free a use of the lancet, and bore it as well as any patient that I ever treated in my life. He also bore tart. antimony as well. But this case seemed an exception to a very general rule.

DISEASES.

Influenza has prevailed extensively in the district of Shelbyville, Tenn.; but being considered a disease of little importance, and no deaths having resulted from it, no gentleman has done more than mention its existence, or given the number of cases treated.

Cholera has prevailed to a considerable extent in three or four circumscribed localities. The only accounts the Committee have been able to procure are in reference to the disease as it occurred in Memphis, Tenn., and in Paris, Ky.

Time of prevalence.—At Paris, Ky., the ravages of cholera were confined almost entirely to the month of August. At Memphis, Tenn., it prevailed from the middle of May to the middle of July.

Causes.—At Paris, these seem not to have been appreciated. At Memphis, there were abundant sources for the elimination of miasma, as will be evident upon consulting the topography.

Persons most liable.—At Paris, it attacked very few of those who had remained during its prevalence in 1849; its principal force falling upon new comers and those who had fled during the former visitation. Among these, it assailed all classes and conditions, but bore with particular severity upon the Irish labourers on the railroad. The young, robust, and male sex suffered most; the white more than the black; the intemperate more than the temperate. At Memphis, the intemperate and filthy, those occupying crowded, badly ventilated houses, suffered from cholera; whilst the well-provided and temperate were visited by diarrhœa and dysentery.

Symptoms.—At Paris, there was less vomiting than usually characterizes the disease, but more purging and sweating. Nothing is said as to cramps. At Memphis, the symptoms seem to have been not materially different from the ordinary course. Dr. Grant, how-

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ever, remarks that a profuse colliquative perspiration was more certainly of fatal import than any other symptom. An unusual mortality marked the disease in both places.

Treatment.—At Paris, the treatment was various; but the administration of calomel, in such manner as to promote hepatic secretion, was chiefly relied on. In a few cases, cold salt water taken internally and freely applied to the surface, was followed by very satisfactory results.

At Memphis, all modes of treatment were tried, and all signally failed. At Hickman, Ky., but five cases were reported; treated by calomel and opium. All died.

		NO. ATTACKED.							DIED.							
		Col.	Und. 15.		15 to 40.		Ov. 40.	Total.	Un. 15.		15 to 40.		Ov. 40.		Total.	
			M.	F.	M.	F.	M. F.		M.	F.	M.	F.	M.	F.		
Bourbon, Ky.,	W.	8	1		12	3	" 2	21	"	1	6	"	2	"	9	Dr. Ray,*
"	B.	1	2		3	1	" "	7	"	"	1	2	"	"	3	Paris, Ky.
Hickman,		"	"	"	"	"	" "	5	"	"	"	"	"	"	5	Dr. H. C. Catlett.

Hooping-cough has in a few places been more or less rife. The only comment is a remark by Dr. McNelley, of Fayetteville, Tenn. Having seen nitric acid recommended by F. C. T. Arnoldi, he tried it in eight cases, considered pretty bad, in one family. He diluted the acid with water to the strength of lemonade, and sweetened with sugar. Of this, he gave as much as the patient would drink, and as often as he would drink it. Two weeks after, *all but one* were *entirely well.* This one had not used it freely. He insisted upon the use of the remedy; and in another week, *he* was well. Hence, although he thinks the cases too few to determine the value of the remedy, he views it as an agent of great promise.

Returns have been received of various diseases, as *pneumonia*, *rheumatism*, *dropsy*, *croup*, &c., which are omitted in this report; because, though of much interest to the physician, they do not come legitimately under the head of *epidemics;* and because it is considered important that no unnecessary space should be occupied.

Measles.—At Memphis, Tenn., measles prevailed pretty extensively from February to October. Dr. Grant considered it a mild disease. In fact, he details no symptoms, except mentioning that, in May and June, the accompanying diarrhœa assumed a more malignant character from the superadded influence of miasmatic poison.

At Clarksville, Tenn., Dr. Haskins, a member of this Committee,

* Of these, two were consultation cases.

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had 71 cases, and presumes there were 300 cases in the town, which contains about 3,000 inhabitants. Here, too, the disease was considered mild. It is supposed by Dr. H. that not more than five deaths occurred. He lost but one. That was in a destitute family, and in a moribund state, when first seen.

At Lexington, Ky., this disease prevailed to a great extent, from January to April. Few deaths; no symptoms or treatment detailed.

At Cynthiana, Harrison County, Dr. Adams reports measles to have prevailed to a great extent during the spring, so as to leave unattacked very few persons who had not had it previously.

At Knoxville, Tenn., according to Dr. Ramsay, the disease began in the latter part of February, and was increasing towards the last of April; which is the latest information on the subject.

Mortality.—As at Clarksville and Lexington, the mortality was trifling at Cynthiana; but two deaths are said to have occurred, and those complicated with pneumonia.

At Memphis, however, 41 deaths are returned from measles. Dr. Grant thinks that these deaths should more properly have been ascribed to diarrhœa and cholera infantum. By inspecting the mortality table, it will be seen that only two deaths are returned from cholera infantum,* and but four from dysentery, during the four months that measles was so fatal, that is, from May to August. It is true, diarrhœa caused 29 deaths during these four months; but when we see 39 deaths ascribed to measles, we cannot but think there is great reason to believe that Dr. G.'s interpretation is the true one, viz.: "In May and June, the accompanying diarrhœa assumed a more malignant character from the superadded influence of the miasmatic poison on subjects predisposed to previous disease; the *epidemic* having passed through its stages, yielded its sway to the endemic poison of the season." "Hence, we find in the bills of mortality 29 deaths from measles recorded in these two months; more than twice as many as are recorded in the other six months of its prevalence." From Knoxville, no death is returned.

Cause.—Undoubtedly, a specific contagion. From facts observed at Knoxville, Tenn., Dr. Ramsey, one of the members of this Committee, is well satisfied that the period of incubation in measles is *not two weeks*, or any other definite time.

Symptoms.—We have no detail of symptoms; but Dr. Ramsey

* During the eight months of measles, only three deaths are charged to cholera infantum.

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mentions an unpleasant swelling of the glands of the throat, which occasioned more or less difficulty, until the eruption became plentiful, when it subsided gradually. There was also a tardiness in the eruption, minute purple spots being frequently visible under the skin for several days before they appeared on the surface. In many cases, too, there was pneumonic complication at the commencement. The eruption lasted about six days from the time it appeared on the face until it disappeared at the extremities.

Treatment.—There is no treatment given, as pursued at Memphis or Clarksville. Indeed, generally, there seems to have been little need of treatment. Dr. Addams, of Cynthiana, mentions that the apartments of the sick were kept well ventilated. Cold and iced water and toddy were allowed; and in some cases the face, breast, and extremities were sponged with cold water, with entire satisfaction. Dr. Ramsey confined his patients to tepid drinks, restricted diet, occasional opiates, astringent and sometimes acidulated gargles, and, when necessary, a little castor-oil. Some cases, however, attended by pneumonic symptoms, were treated by a lye poultice over the breast; with calomel and ipecac., followed by iodide of potash, in two-grain doses, every five hours, to a child four or five years old.

There was a marked slowness in convalescence in many cases, attended by slight feverish movements in the evening, to remove which required tonics and even quinia.

Table showing at one View the Color, Age, and Sex of those reported as suffering from Measles, with the Mortality and Average Days' Attendance.

REPORTED FROM	Colour.	CASES ATTENDED.							FATAL CASES.							Average days' attendance.	REPORTED BY
		Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.	Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.		
		M.	F.	M.	F.	M.	F.		M.	F.	M.	F.	M.	F.			
Grundy Co., Ten.	W.	2	3	...	...	...	...	5	...	...	...	...	...	...	...	...	Dr. J. V. W. Conn, Pelham.
"	B.	1	...	...	...	...	...	1	...	...	...	...	...	...	...	5	
Casey Co., Ky.	W.	18	17	11	2	...	..	48	...	...	...	...	...	...	...	...	Dr. Armstrong, Liberty.
"	B.	4	3	...	1	...	...	8	...	...	...	...	...	...	...	8	
Clarksville, Ten.	...	...	...	...	...	...	...	71	...	...	...	...	...	...	1	...	Dr. Haskins, Clarksville.
Knoxville, Ten.	W.	11	9	1	4	...	1	26	...	...	...	...	...	...	...	...	Dr. Ramsey, Knoxville.
"	B.	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	

Furunculous Epidemic.—It has occasionally been observed that phlegmons and whitlows have prevailed to a very considerable degree; so much so as justly to be considered epidemic. This was true to a marked extent in Memphis, Tenn., during the summer of both

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1851 and 1852. During the first year, they were attributed to the great heat and drought; but in 1852 they could not be ascribed to these causes. Hence they may more properly be considered as dependent upon an epidemic condition of the atmosphere.

It frequently happens that seasons of unusual sickness are ushered in by inflammatory affections of this or analogous kinds. So frequently, indeed, is this the case, that when these affections are uncommonly numerous at the beginning of a season, there are good grounds to fear that an epidemic of some kind is at hand.

These affections are not generally, perhaps, ascribed to malarious influences; yet, in this country at least, they have usually been attended or followed by diseases of that class.

Nothing is said as to persons most liable, but it is probable that children and young persons suffered most from boils. Neither is anything said as to treatment. Nothing different from the ordinary management of such cases, it is presumed, was necessary. The only thing worthy of note is, that, upon such affections becoming common, the population should be warned that they are in danger, and cautioned to be more than commonly careful to obey hygienic rules, and to avoid exposure to any exciting cause of disease.

Cholera Morbus.—This disease prevails in the hot months of the year, and a few cases occur throughout the West and South-West annually.

Causes.—A predisposition to this disease is created by the continued heat of summer, whereby the powers of the digestive organs are greatly weakened. A sudden change of temperature (usually at night), finding the body unprotected, or some indiscretion in eating, is usually the exciting cause.

It is scarcely necessary to detail the symptoms, as they are well known and readily recognized.

Treatment.—As the stomach is usually thoroughly evacuated before seen by a physician, the first indication is to quiet the stomach and bowels. Dr. Grant attains this object by the prompt administration of one grain of *morphia* in a spoonful of ice-water; if ejected, to be *immediately* repeated till retained. The patient is encouraged to eat pounded ice. One or two doses are usually sufficient to arrest the disease. After the disease has been quieted for some twelve or eighteen hours, a mild laxative is given.

Dr. Swain obtained his purpose by sinapism to epigastrium, a pill of two grains calomel and half-grain opium, with camphor, every hour, until three were retained; or by four to six grains of opium

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and six of calomel, followed by another grain of opium in an hour. Sometimes an injection of oak-bark tea and laudanum was given. Scarcely a case required a second visit.

Table intended to show at one View the Colour, Age, and Sex of those reported as suffering from Cholera Morbus, with the Mortality and Average Duration of Treatment.

REPORTED FROM	Colour.	CASES ATTENDED.							FATAL CASES.							Average days' attendance.	REPORTED BY
		Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.	Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.		
		M.	F.	M.	F.	M.	F.		M.	F.	M.	F.	M.	F.			
Oldham Co., Ky.	W.	1	2	4	6	...	...	13	...	1	...	1	...	...	2	8	Dr. Swain, Ballardsville.
"	B.	...	1	2	1	...	...	4	...	...	...	...	...	...	...	...	
Lincoln Co., Ten.	W.	1	...	2	1	1	...	5	...	...	...	...	...	...	...	...	Dr. McNelley, Fayetteville, Ten.
"	B.	...	...	...	1	...	...	1	...	...	...	...	...	...	...	2	
Bourbon Co., Ky.	W.	2	...	...	...	...	...	2	...	...	...	...	...	...	...	...	Dr. Ray, Paris.
"	B.	...	3	1	...	...	...	4	...	...	...	...	...	...	...	...	"

Cholera Infantum.—This is a disease which has almost disappeared from considerable portions of this district, whilst it continues to prevail in other portions as much as ever. Notwithstanding much attention has been bestowed upon this disease in the United States, and especially in the West and South-West, it is doubtful whether the diagnosis is not even yet involved in some obscurity; whether, in fact, other diseases are not frequently called by that name.

Cholera infantum usually begins early in the season, in May, for example, and frequently continues until October. It attacks children from shortly after birth up to two or three years, the second summer being the time during which it is emphatically apt to occur.

Causes.—It is one of the maladies usually ascribed to malaria; and it is supposed that the delicacy and susceptibility of the infant is the reason why this form of disease appears before others arising from the same cause. In aid of this malarious influence, a high temperature occupies a conspicuous place in its causation. In malarious districts, it frequently happens that the inhabitants keep their houses open during the day, and on the approach of night shut them up for the purpose of excluding the night air, which is thought to be particularly injurious to health. In this way, the heated air of the day is introduced into the house and retained during the night, so that the children are continually kept in a highly heated and debilitating atmosphere. The necessary consequence of this is a state of the body exceedingly irritable, and in a condition to be readily acted

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on by any exciting cause of disease. Many physicians in the South and West must remember very many instances in which, when called to see a child at night, they have been ushered into a room which was almost suffocating. Doubtless, the irritation occasioned by teething has much to do in developing the disease. Any impropriety in feeding, exposure to a cold or damp atmosphere, or indeed any indiscretion is almost sure to develop the disease in children whose systems are rendered irritable by the influence of malaria and highly heated rooms, as above described.

Dr. Grant, of Memphis, draws a distinction between that malaria arising from decaying vegetable matter, and that miasm which arises from excrements. To the latter he attributes bowel complaints generally, except Asiatic cholera, whilst he imputes paroxysmal fevers to the former.

Prophylactics.—The most certain method of protecting children against this dread scourge is to remove them, before hot weather sets in, from miasmatic regions. But all persons have not the means to remove their children from their homes, and keep them away for four or five months. Hence it becomes necessary to inquire what can be done to guard against the disease at home. Much can be done, and unquestionably ought to be done by the public authorities of cities to protect the citizens, children and adults, against such diseases. But this subject will not be discussed here. Great care must be used to husband the resources for preserving health, and at the same time guarding against disease. The child should be kept upon food most suitable for its condition; should not be weaned between March and October; should have a warm or cold bath, according to his strength and ability to stand the shock, every morning; should be exercised in the open air daily, when the weather will admit; not too soon in the morning, lest he be brought in contact with malaria — nor too late, to avoid the great heat of the day. The body should be protected against sudden vicissitudes of weather. For this purpose, very fine flannel or silk worn next the skin will be required by many. All crude and unripe fruits, nuts, and everything difficult of digestion, must be avoided. The chamber usually appropriated to the child should be large, well ventilated, and well lighted; yet the heated air of noonday should be as much excluded as possible; and the air of morning, after the sun shall be up an hour or so, and of the evening, should be admitted freely. If these rules be observed with perseverance and discrimination, the child will have a very reasonable prospect of escape; but if neglected, an attack must be expected.

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Symptoms.—But two papers detailing symptoms have come to hand; one by Dr. W. T. Baskette, of Murfreesboro', Tenn.,* and the other by Dr. T. M. Woodsen, in Middle Tenn.† From these the symptoms are arranged.

Digestive Organs.—At the commencement, the tongue is little changed, perhaps a little white. Subsequently, it becomes red, dry, or black. In the last stage, sordes about the lips.

Stomach.—In many cases, the disease is ushered in by copious vomiting without premonition, which continues to recur at short intervals, when everything swallowed is immediately ejected. Indeed, this symptom is very similar to malignant cholera, both in degree and effects. It is attended by intense thirst. In the progress of the disease this irritability may disappear or become greatly mitigated. At the same time, in many cases, the *bowels* become affected with profuse diarrhœa, the dejections being at first fecal, but soon becoming serous, with or without a yellow colour or greenish tinge. During the disease, these evacuations may assume a great variety of appearances—greenish, yellowish, brownish, white, &c.; serous, mucous, curdled, slimy, tinged with blood. In some cases, they assume very much the appearance of "scrabbled eggs," as if the white and yellow mucus had been stirred together. Sometimes they are inodorous, at other times sour, or of a highly offensive or even sickening smell. In many cases, however, a *diarrhœa* of some days' duration constitutes the initiatory stage, which is succeeded by copious vomiting and purging.

Circulatory System.—In a few cases, there is some fulness and frequency of pulse, with an evening exacerbation, for several days before the stomach and bowels become affected. Most commonly, however, the fever comes on later. In such cases as are introduced by profuse vomiting and purging, the pulse speedily fails, the features become pale, shrunken, and even livid. Indeed, many cases simulate Asiatic cholera very perfectly. When this stage passes away, it is followed by more or less fever, the pulse being small, frequent, and irritable; the surface of the body may be uniformly warm, or the head and body may be unduly hot, whilst the feet and hands are decidedly cool. As the disease advances, the pulse becomes more irritable, the circulation more irregular, the skin cool, flabby or waxy, and of a tawny colour.

Cerebral and Nervous System.—At the onset, the child may be

* Southern Journ. Med. and Phys. Sciences, vol. i. p. 82.

† Nashville Journ. vol. iv. p. 4.

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entirely prostrated by the force of attack. If this is of a milder character, it may be irritable and fretful. The abdomen, during the disease, sometimes becomes tender and tympanitic. Towards a fatal termination, the child lies rolling his head from right to left, continually moaning and uttering the most plaintive cries; frequently throws his hands towards his head, or strives to ram them into the mouth; the eyes become glassy and listless, convulsions come on, and death closes the scene. The *skin* soon loses its healthy hue, becomes pale and sallow, dry and husky. Towards a fatal termination, a leaden hue prevails to a greater or less extent, and the skin becomes clammy. In protracted cases, sudamina are thickly sprinkled over the thorax and abdomen. These, Dr. Dewees looked upon as indicating certain death. In that prognosis he is occasionally wrong.

This disease is sometimes cured in a few days. Sometimes it recurs at intervals throughout the summer, and until frost in the fall. In many instances, the emaciation is extreme, so that the child seems to be literally "nothing but skin and bones."

Treatment.—Dr. Swain says: "I generally treat it successfully with, ℞. Opii gr. ⅛, calomel gr. ½; divided into eight doses, one of which is to be given every hour. Or, ℞. Pulv. Dov. gr. ii; hydrarg. c. cretæ gr. ii. M. in chart. No. viii. divid. One every three hours; and a diet (if weaned) of rice-water, crust coffee, or boiled milk." Drs. Ford, of Liberty, and Conn, of Pelham, Tenn., give no account of treatment.

Dr. Woodson commences the treatment with powders of quinia and calomel, and Dover's powder (quinia from one-half to three grs.) every three or four hours, according to the severity of the case and the time intervening before the next paroxysm. If the brain is involved early, the Dover's powder may be omitted, substituting ipecac. and rhubarb. If vomiting is severe, the ipecac. to be omitted until the stomach is quieted. When great irritability of stomach is present, that must be allayed before any medicine is given. For this purpose bruised mint steeped in spirits, and applied over the stomach and bowels; using at the same time the following: ℞. Tinct. opii camphorat. ʒi; arom. spt. ammoniæ ♏x; creosot. ♏i. M. In doses of 5 to 15 ♏, diluted with mint tea, every half hour, or after each vomiting, is a very valuable prescription. For the same purpose, an injection of a solution of table-salt in water. But nothing perhaps answers this purpose so well as applying a blister to the epigastrium, removing the cuticle, and sprinkling the surface with morphia and quinia. Vegetable astringents, during the first stage, do no

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good. Warm bathing should be used twice daily, until the disease is entirely removed. The skin, also, should be protected by flannel. After the febrile paroxysms shall have subsided, mineral tonics, as preparations of iron, arom. sulph. acid.; also, quinia and columbo. Mild vegetable astringents may also be used, as black or dewberry root.

Dr. Baskette treats the commencing diarrhœa with magnesia and rhubarb, and spiced syrup of rhubarb, which is usually promptly successful. If these fail, however, he gives: ℞. Calomel, pulv. Dov., acet. plumb. āā gr. ii, in pulv. No. viii. divid. One every two hours, until the whole are taken. If these should not move the bowels within a few hours after the last has been taken, they should be assisted by a little castor-oil. When the stomach is very irritable, minute doses of calomel or hydrarg. c. creta, combined with bicarb. soda or chalk, frequently administered until the stools show the action of the medicine, which should be aided by frequent injections of table-salt (a teaspoonful to four ounces of water). Should there be fever, with pain, tenderness, and distension of the belly, a few leeches to abdomen, followed by warm cataplasms, will be of great service. If there is coldness of the extremities, they should be thoroughly warmed by rubbing with spirits, in which Cayenne pepper or mustard, or both, have been infused. If there is delirium, photophobia, redness of eyes, &c., leeches to the temples or behind the ears, with warm embrocations to the extremities, will be proper. Also, cloths wrung out of cold water applied to the head. When the arterial excitement has been sufficiently removed without relieving the cerebral symptoms, the head should be shaved and covered with a blister. When the vomiting has been controlled, but the bowels remain too free, the child should be frequently bathed and rubbed with a soft brush or naked hand, and flannel worn next the skin. The powders of calomel, sac. saturn. and Dover's powder, one every hour until purging is arrested, should also be used. When there is tenesmus, with slimy, offensive stools, he gives a dose of hydrarg. c. creta, followed by a teaspoonful of castor-oil and a drop or two of laudanum, aided by anodyne enemata and warm cataplasms to the bowels. After the oil operates, a teaspoonful of an emulsion, containing three drops turpentine and two of laudanum, is given every four or six hours.

When the force of the disease is broken, but occasional nausea, fretfulness, fever, want of appetite, apparently healthy stools, except their fluidity, remain, he gives mucilaginous drinks, occasional doses

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hydrarg. c. creta, and small quantities of bicarb. soda. For the annoying chronic diarrhœa, which is so apt to remain or to be renewed by slight indiscretions, he gives minute doses of acet. plumb. ¼ gr., Dover's powder, $\frac{1}{12}$ gr., every one, two, or four hours, until the bowels are restrained; or a teaspoonful of emulsion containing four drops turpentine, and one drop and a half of laudanum, every three, four, or six hours. To support the strength during convalescence, he gives quinia and acid. sulph.; also, a mixture of quinia, tr. catechu, and chalk mixture. When the diarrhœa is persistent, and stools offensive, there is reason to suspect ulceration of the mucous coat of the bowels. For this he uses: ℞. Mucil. gum acaciæ f ℥iii; nit. argent. gr. ii; tr. opii gtt. 24; loaf sugar q. s. A teaspoonful three or four times a day. For the hydrencephaloid state, in which the little sufferer lies in a restless, comatose state; moans and rolls his head about; his skin cool and flabby; his pulse weak, almost imperceptible, though not very much increased in frequency; hands thrown in unsteady gyrations towards his head; eyes dull and half closed, pupils dilated, extremities cool, &c., he gives stimulants, tonics, and astringents; catechu, chalk mixture, wine whey, brandy toddy, stimulating liniments, frictions over the spine, warm bath. He very properly considers this state as depending upon exhaustion occasioned by the preceding evacuations.

Dr. G. W. Booth, of Harden County, Tenn., declares, after many years' trial, that sulph. quinia, given in large doses without much regard to the stage of the disease, is pre-eminently successful. On the other hand, Dr. Grant says that he saw no good whatever from the use of quinia; on the contrary, if it had any effect, it did harm.

Table intended to show at one View the Colour, Age, and Sex of those reported as suffering from Cholera Infantum, with the Mortality and Average Period of Treatment.

REPORTED FROM	Colour.	CASES ATTENDED.							FATAL CASES.							Average days' attendance.	REPORTED BY
		Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.	Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.		
		M.	F.	M.	F.	M.	F.		M.	F.	M.	F.	M.	F.			
Grundy Co., Ten.	W.	10	11	...	...	...	...	21	1	2	...	...	...	...	3	...	Dr. Conn, Pelham.
"	B.	8	5	...	...	...	...	13	...	1	...	...	...	...	1	4	
Casey Co., Ky.	W.	3	4	...	...	...	...	7	...	...	...	...	...	...	...	9	Dr. Armstrong, Brownsville.
Oldham Co., Ky.	W.	3	7	...	...	...	...	10	...	...	...	...	...	...	...	...	Dr. Swain, Ballardsville.
"	B.	1	...	...	...	...	...	1	...	...	...	...	...	...	...	4½	

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Diarrhœa.—Although sporadic cases occur at all seasons, yet it never assumes an epidemic form except during summer. The last year it prevailed at Memphis, principally from May to October; during which time, forty-one out of the fifty-four cases proved fatal.

Causes.—Diarrhœa seems to depend upon the same causes as cholera infantum, cholera morbus, and dysentery. This, it is believed, is generally admitted; there being perhaps no difference of opinion as to what those causes are, if we except that Dr. Grant considers them emphatically miasmata exhaled from excrement. It may be well to remark here, that Dr. Grant adverts to a general opinion that disorders of the bowels depend, in a great degree, upon the hard water with which Memphis abounds. After very carefully-conducted observations, he declares his disbelief in the operation of that cause. Having made it a point to inquire of those applying for treatment, he has found that the patients were as often in the habit of using river or cistern water as well water. Neither have these diseases become at all less common since the city has been well supplied with cisterns, and cistern water has come into general use.

Symptoms.—At Memphis, diarrhœa existed in two distinct forms:

1. The *Serous Diarrhœa.*—This form usually came on without premonition. Sometimes indigestion and flatulence preceded it; the discharges were of a serous character; thin and frequent. No pains in the bowels, fever, or constitutional disturbance. After continuing some time, loss of appetite, thirst, and whitish stools supervened.

2. Just before, and during the time, cholera prevailed, a form of diarrhœa, properly called *bilious*, was observed. It was attended by fever, frequent pulse, coated tongue, great thirst, nausea and vomiting, scanty and high-coloured urine, and frequent painful discharges from the bowels of liquid green or black-coloured matter. The suffering was frequently exceedingly severe. At Fayetteville, Tenn., it was marked by a tendency to run into dysentery.

In Oldham Co., Ky., it was marked by copious watery stools without nausea, fever, or other marked disturbance of the system.

Those most liable at Memphis were strangers, or "unacclimated" persons. These were observed to be peculiarly liable.

Treatment.—The serous form was usually very promptly relieved by astringents and opiates, singly or combined. Dr. Grant was particularly partial to sulph. zinc. and tannic acid, combined with opium. After arresting the disease, for the purpose of maintaining the functions of the liver, which were apt to be somewhat impaired by the disease and the remedies, he advised blue pill, or hydrag. c. creta, for

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a night or two. For the bilious diarrhœa, which was considered as somewhat connected with choleraic influences, he gave calomel and opium. The disease usually yielded before, or as soon as the constitutional effects of the mercury appeared. When the accompanying fever was of a remittent type, which was usually the case, quinia was an essential remedy, and aided very much in arresting the disease.

Dr. Swain used abortive doses of opium, with or without calomel, or teaspoonful doses of the following: ℞. Gum camphor, laud., spts. lavend. comp. āā ʒij; alcohol a pint; which he found entirely satisfactory.

Dr. McNelley says it was very easily cured at Fayetteville, provided it was treated early, but does not say what treatment he pursued.

Table intended to show at one View the Colour, Age, and Sex of those reported as suffering from Diarrhœa, the Mortality of Each, and the Average Time of Attendance.

REPORTED FROM	Colour.	CASES ATTENDED.							FATAL CASES.							Average days' attendance.	REPORTED BY
		Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.	Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.		
		M.	F.	M.	F.	M.	F.		M.	F.	M.	F.	M.	F.			
Grundy Co., Ten.	W.	8	9	8	12	1	1	39	...	1	...	...	...	...	1	...	Dr. Conn, Pelham.
"	B.	3	2	4	3	...	...	12	...	...	...	...	...	...	...	11	
Edmonson Co., Ky.	W.	...	...	...	...	3	9	12	...	...	...	...	...	...	2	15	Dr. Ford, Brownsville.
Oldham Co., Ky.	W.	2	1	3	1	...	2	9	...	...	...	...	...	...	...	...	Dr. Swain, Ballardsville.
"	B.	...	...	1	...	...	...	1	...	...	...	...	...	...	...	4½	
Casey Co., Ky.	W.	...	...	5	6	1	...	12	...	...	...	...	...	...	...	1	Dr. Armstrong.
Lincoln Co., Ten.	W.	9	8	1	2	...	...	20	1	...	...	...	...	...	1	3	Dr. McNelley.
"	B.	...	1	...	...	...	...	1	...	...	...	...	...	...	...	...	"
Bourbon Co., Ky.	W.	15	13	18	11	2	2	61	...	...	...	...	...	...	...	...	Drs. Ray, Paris.
"	B.	3	5	7	3	3	2	23	1	...	...	...	1*	...	2	...	"

Dysentery. Time of Prevalence.—In Floyd Co., Ky., dysentery was very prevalent during the summer and fall of 1852. Dr. Hurt thinks there must have been 500 cases in the county, whose population is 5,714. He saw 143.

At Colemansville, Harrison Co., it prevailed from June 17 to September 20.

About the first of June, dysentery made its appearance in Lincoln Co., Tenn., and spread with fearful rapidity throughout the eastern,

* Over 100 years.

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and in some portions of the western and north-western portions of the county. It became the most extensive and fatal epidemic that has ever visited that county, and it did not entirely disappear until December. The same was true of the adjoining county of Bedford.

In Bourbon, this disease prevailed from July to October; but a few cases occurred before and after that period.

Persons most liable.—Of 143 cases seen by Dr. Hurt, 142 were white, 1 black; 56 males, 87 females; 42 were under 15 years; 37 between 15 and 30; 29 between 30 and 45; 16 between 45 and 60; and 19 between 60 and 100 years.

Of 50 cases seen by Dr. Hawkins, Colemansville, 45 were white—32 males, 13 females; 5 were black—3 males, 2 females. Ages not specified.

In Oldham, 54 whites—28 males and 26 females, and 9 blacks—7 males and 2 females, were attacked. 56 were under 40 years; 7 above that age.

In Casey County, Dr. Armstrong reports 123 cases; of which 111 were whites, and 12 blacks. Of the 111 whites, 70 were males, 41 females. Of the 123, 57 were under 15 years; 45 from 15 to 40; and 21 above 40 years.

In Bourbon, of 131 cases, 50—23 males and 27 females—were white; 81—43 males and 38 females—were black.

In Grundy Co., Tenn., Dr. Conn reports 107 cases; of which 88—41 males, 47 females—were white; and 19—11 males, 8 females—were blacks. 37 were under 15 years; 56 between 15 and 40; and 14 over 40 years.

In Lincoln, Co., Tenn., it attacked all ages, sexes, and colours indiscriminately. In the eastern portion of the county, scarcely a family escaped, and the mortality was very great. I have no means of ascertaining the number of cases, or the proportion of deaths. There were, however, several hundred deaths in the eastern portion alone.

In Bedford County, the disease was much more fatal in children. It was also more fatal among females than males.

From Brownsville, Edmonson Co., Ky., Dr. Ford reports 54 cases, apparently all white; 23 males, 31 females. 50 under 15 years.

Causes and Prophylaxis.—Under these heads, there is nothing to be added to what was said last year, except that Dr. Armstrong, who lives in the vicinity of salt-works, says: "All those who would

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drink freely of the brine water from the salt wells, were almost entirely protected against flux."

In Lincoln Co., Tenn., Dr. McNelley was satisfied that the disease was not influenced by any geological formation. It appeared equally rife, and equally fatal, in limestone and in freestone regions. The town of Fayetteville, and its immediate vicinity, seem to have suffered much less, both in number of cases and in deaths, than any other portion of the county where the disease prevailed. The weather did not appear to exercise any influence on the disease. Those changes which usually produce an increase or an abatement of malarious diseases, he found to produce no effect whatever. Neither does he think the disease of malarious origin. He is at a loss to what cause to ascribe it, but, as dysentery and typhoid fever prevailed coextensively, and in localities distinct and separate from those occupied by malarious fevers, he thinks the two former diseases were dependent upon the same cause; a cause different from that productive of the latter.

Symptoms.—After a careful examination of the symptoms detailed in various contributions with which this Committee has been favoured, they are found so similar to those given in the report of last year, that it is not considered necessary to repeat them. Dr. McNelley found the pulse to vary but little from the natural standard; the tongue was very red; the soreness of the belly, the tormina and tenesmus very distressing.

Dr. Lipscomb, in Bedford County, adjoining Lincoln, found the pulse much accelerated, small, and corded, ranging from 100 to 120, 130, 140, or even to 150. The dysenteric discharges were more copious, and attended with more irritation of the rectum and bladder, than he had ever before seen.

Drs. Ray, of Paris, Ky., divide dysentery into simple and typhoid. The symptoms which distinguished typhoid from simple dysentery were, great heat and tenderness in the abdominal organs; rapid feeble pulse; restlessness and anxiety; with dejections of bloody mucus, or sometimes bloody serum; great and rapid prostration of the vital powers towards the close of the disease; and especially the rapidity of its course and great mortality.

Post-mortem Appearances.—After typhoid dysentery, Drs. Ray found: "In addition to extensive and violent inflammation of the mucous surface of the rectum and colon—in many parts of which there was an entire destruction of the membrane—there was inflammation of the mucous coat of the ileum and jejunum; in some places

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of the ileum the ulcerations had nearly passed through the coats of the bowel. In one case, severe inflammation of the duodenum; in this case, severe and protracted vomiting was one of the most annoying symptoms."

Treatment.—When certain dyspeptic symptoms — as lassitude, headache, depraved appetite, slight nausea, sense of emptiness in the stomach—indicated the approach of dysentery, Dr. Hurt, of Prestonsburg, Ky., gave an emetic of ipecac., and followed it with five or ten grains of calomel in a few hours. If this should not move the bowels in five or six hours, he gave a dose of castor-oil or sulph. magnesia. After thoroughly evacuating the stomach and bowels, opium, in anodyne doses, repeated every six or eight hours. The healthy secretions from the liver, kidneys, and skin were promoted by one to six grains of calomel, combined with ipecac. and opium, and given once, twice, or thrice daily, with nitr. potass., or spirits nitric ether, or not, as the state of the kidneys might indicate. To arrest the bloody discharges and ulceration of the mucous membrane, astringents are proper; of which, tannic acid, in doses of one to two grains, every three, four, or six hours, is his choice—most commonly in union with the medicines previously mentioned. When the symptoms do not yield to the course advised, and typhoid symptoms supervene, a more tonic and stimulant course is required. For this state, oil of turpentine or balsam copaiba, alone or mixed, in the proportion of one part turpentine to two of copaiba, may be given. Of this mixture, fifteen to twenty drops every three or four hours, with an occasional dose of castor-oil, will be a very suitable formula. If debility is very great, spts. of camphor may be advantageously combined with the oil. Sinapisms and stimulating liniments will be advisable at all stages of the disease, especially towards the last.

Dr. Swain tried abortive treatment with opium, but could only succeed at the onset of a case. As few such cases presented, he next tried a mixture of sulphate magnesia, muriate of soda, and vinegar, with which he was not at all satisfied. Injections of emulsion of egg and turpentine answered no better. In the pre-diarrhœal stage he frequently arrested the disease at once, by the tincture of camphor, laudanum, and spirits of lavender comp., as mentioned under the head of diarrhœa. When diarrhœa continued, after the dysenteric symptoms had disappeared, he gave one quarter of a grain of nit. argent., combined with opium, calomel, or blue mass, with the best effects.

Dr. Hawkins avoided stimulating medicines of all kinds. Hope's

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Mixture he found worse than useless. As soon as fever supervened he gave blue mass and opium, and continued it until the fever was reduced, which was about six or eight days, if the patient recovered. If the patient died, of course the fever continued longer. As soon as the fever subsided he discontinued the blue mass, without any regard to the mucous bloody stools. After laying aside the blue mass, he gave some mild astringent, for the purpose of moderating the bloody mucus. As a mild and yet certain purgative is necessary in this disease, a dose of castor-oil and turpentine was given every twenty-four hours, to which, when much pain existed, laudanum was added.

At Memphis, the disease seems to have been very mild, and the remedies very prompt in affording relief; opiates, by the mouth or introduced into the rectum, being the chief articles. An occasional laxative was sometimes necessary, when a powder of rhubarb and the Dover's powder, suited to the age, &c., answered every purpose.

Dr. R. Thompson, of Salisbury, Tenn., claims* for the leaves of the peach-tree, *Amygdalus Persica*, the character of a sovereign remedy for dysentery. He directs a handful of the leaves to be infused in a pint of boiling water, to which a teaspoonful of laudanum and 10 grs. alum are to be added. One-third of this is to be used as an injection, to be repeated after each discharge. If the disease is of some standing, he applies a poultice of the leaves, thickened with wheat-bran, to the abdomen, and gives a half-teacupful of a weak tea of the leaves every hour or two. Constipation is obviated by small doses of salts. Peach-tree leaves is a remedy which may very readily be believed to be serviceable in mild cases of dysentery. But it is not at all surprising that Dr. T. should have seen cause to modify his statement† by his experience in other epidemics of dysentery, though he still places it high on his list of remedies.

In the Louisville Marine Hospital, during 1851, eighteen cases of acute dysentery were treated by salts and opium, viz., from one to four drachms of sulphate magnesia were taken every hour, until serous stools without blood or mucus were produced. Then a Dover's powder was given, and repeated occasionally. Enemas of spirits of turpentine, and an egg, to which a teaspoonful of laudanum was added when there was much tenesmus, were administered, once, twice, or oftener, in the day. But one of the eighteen patients died.

* Nashville Journ. vol. ii. p. 188.
† Ibid. vol. iv. p. 143.

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In this case, death was ascribed to the advanced stage of the disease and feebleness of the patient.

Many cases of chronic dysentery were treated by [sulphate of?] copper and opium, in the hospital, which had resisted ordinary means.

Dr. McNelley treated his first two cases with calomel, opium, and castor-oil and spirits of turpentine, and injections of starch and laudanum. Both died. He then adopted the saline treatment. Purged freely with sulphate of magnesia and Seidlitz powders, which immediately quieted the tormina and tenesmus, and checked the dysenteric stools. He then gave a full dose of sulphate of morphia or opium; then blue mass gr. iv, does not say how often, until the dysenteric stools again appear. As soon as they appear, the salines and opiates are repeated, as before. One free purgation was generally sufficient to check the discharges for twenty-four hours. This course was pursued until the disease was cured. As a means of relieving pain, hot poultices were applied to the abdomen. Of twenty-six cases treated in this way, all recovered.

In many cases, a *most violent and fatal diarrhœa* supervened upon the *dysentery*. This diarrhœa, Dr. McN. was so fortunate as to relieve, in every instance, by the following: ℞. Subnitrat. bismuth. grs. lviii; nitrat. argent. gr. iv; pulv. opii gr. vi. Make into twelve pills. One pill every four hours to an adult. Indeed, he gave these pills before the dysenteric stools ceased, if the disease was assuming a chronic character.

Dr. Lipscomb, viewing the disease as one of inflammatory irritation, with or without hepatic torpor, modified his treatment accordingly. If the pulse would at all justify it, he bled freely in the beginning of a case, even to syncope. In a few cases, this operation was repeated. When used to this extent, it produced marked amelioration of symptoms. The venesection was generally followed by cups to the tender parts of the abdomen. If there was any apparent deficiency of the biliary secretion, he gave divided portions of ipecacuanha, morphia, and calomel, until a sufficiency of calomel had been taken to insure a full impression on the liver. This was followed by ol. ricini. As soon as the excitement was lowered, he gave ol. terebinth. in doses of eight or ten drops, alternated, in some cases, with acet. plumb.; in others, with tannic acid, combined with one-half or one-third of ipecac., and as much morphia as the system would bear, which was generally ⅛ or ¼ grain every three hours.

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Sometimes, the sulph. cupri, in place of the astringents above mentioned, produced an excellent effect. But when the epidemic had become general, there was so much irritability that the smallest quantity of the sulphate could not be borne. The rectum was so exceedingly irritable that no injection of any kind could be retained. As a substitute, five, six, or seven grains of opium was made into a pill, and introduced up the rectum every two, three, or four hours. This seemed to act very happily in soothing the excessive tenesmus. As the disease progressed, and considerable prostration became manifest, he combined with the medicines before mentioned sulphate of quinia in the morning, and camphor in the evening. During convalescence, he gave a mixture of camphor [water?], nitric acid, and tinct. opii, which answer well in many cases. As external applications, when there was much tenderness of the abdomen, he preferred free cupping, followed by full vesication; the blistered surface to be dressed every few hours with emollient poultices.

In concluding his remarks upon the treatment, Dr. L. says, some physicians proscribed the lancet entirely, whilst in his hands it ameliorated the disease, and rendered it much more manageable. Others proscribed mercurials. Others saw congestion of the portal circle in all cases; and therefore relied too exclusively on mercurials. His observation showed him that when the hepatic secretion was inadequately performed mercurials were demanded, and their specific action was always followed by marked relief to the dysenteric symptoms. On the contrary, when, every few hours, an admixture of bilious matter could be perceived in the dysenteric stools, no good was to be expected from mercurials. The epidemic was more fatal than dysentery had ever been before. This, he believes, was more owing to an adynamic tendency than to an increased severity of the disease itself. Owing to a deficiency of innervation, many cases of sudden, unexpected, and irremediable prostration occurred, and at an earlier period of the disease than had occurred previously. This, in some cases, was accompanied by extreme congestion, marked by cold surface, livid countenance, feeble pulse, great restlessness, and alvine discharges like "new cider." The same kind of discharges, in other cases, were accompanied by well-marked symptoms of typhoid fever. These were the worst cases, and nearly all terminated fatally. Complication with other diseases, as occurring in cases affected with hooping-cough, &c., made the disease more certainly fatal.

Drs. Ray have little to add to what they said last year, except that in simple dysentery the state of the various secretions was such as

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to admit, or even require the use of mercurials, which acted beneficially. The typhoid form did not admit mercurials. For this variety a pill, composed of opium and nit. silver, each one grain, given every four hours, if the frequency of the dejections and severity of griping required, was found to be of very great benefit. A black boy, aged twelve years, took this prescription every four hours (unless asleep), for ten days, with constant daily improvement. No other remedy (of which they tried several) seemed to make any impression on the disease, or mitigate his sufferings.

Table intended to show at one Glance the Colour, Age, and Sex of those returned as suffering with Dysentery, with the Mortality of Each, and Average Time of Attendance.

REPORTED FROM	Colour.	CASES ATTENDED.							FATAL CASES.							Average days' attendance.	REPORTED BY
		Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.	Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.		
		M.	F.	M.	F.	M.	F.		M.	F.	M.	F.	M.	F.			
Grundy Co., Ten.	W.	3	25	28	19	10	3	88	...	...	...	...	...	...	...	...	J. V. W. Conn, Pelham, Grundy Co., Ten.
	B.	4	5	6	3	1	...	19*	...	...	...	...	...	...	...	8	
Floyd Co., Ky.	W.	†	...	...	...	...	...	142	...	...	...	...	...	...	...	...	G. Hurt, Prestonsburgh, Ky.
"	B.	...	...	...	...	...	...	1	...	...	...	...	...	...	4	12½	
Edmonson Co.	W.	20	30	8	1	...	...	54*	...	...	...	...	...	...	7	10	Dr. J. L. Ford, Brownsville.
Oldham Co.	W.	12	17	12	7	4	2	54	...	2	...	...	1	1‡	4	8	J. Swain, Ballardsville, Ky.
"	B.	5	1	2	...	...	1	9	1	...	1	...	...	...	2	...	
Harrison Co., Ky.	...	†	...	...	...	...	...	50§	...	...	...	...	...	...	4	...	J. H. Hawkins, Colemansville, Ky.
Casey Co., Ky.	W.	31	17	27	15	12	9	111	...	...	...	...	...	...	...	7	J. M. Armstrong, Liberty, Ky.
"	B.	8	6	2	1	...	...	12	...	...	...	...	...	...	...	...	
Lincoln Co., Ten.	W.	3	6	...	7	1	1	18	...	1	...	...	...	...	1	10	W. W. McNelley, Fayetteville, Ten.
	B.	5	3	...	...	1	1	10	1	...	...	...	...	...	1	...	
Bedford Co., Ten.	W.	8	11	6	12	4	3	44	1	3	1	5	3	1	14	...	T. Lipscomb, Shelbyville, Ten.
	B.	7	2	6	4	...	1	20	1	...	1	...	...	...	2	8	
Bourbon Co., Ky.	W.‖	7	10	11	12	4	3	47	3	1	...	1	1	...	6¶	...	Drs. Ray, Paris.
	B.‖	21	11	14	21	2	2	71	3	1	...	1	1	...	6**	...	"
"	W.††	1	...	...	2	...	...	8	1	...	...	...	...	...	1	...	"
"	B.††	5	2	...	2	1	...	10	3	1	...	1	...	...	5	...	"

* No symptoms or treatment given.

† So reported that the ages and colours cannot be separated to be conformed to the table.

‡ Died at the end of four months of chronic peritonitis.

§ Two of these consultation cases.

‖ These were cases of simple dysentery.

¶ Six consultation cases.

** Five consultation cases.

†† These were cases of typhoid dysentery.

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Bilious Fever.—Under this head will be embraced intermittent and remittent fever, as also continued fever, when considered as originating from miasmata of the same grade as that producing the other two forms of fall fever.

Intermittent Fever prevailed to a slight extent in *Oldham* County, Ky., principally in August and September.

At Memphis, Tenn., bilious fever prevailed, under different forms, from July to November.

Causes.—The Committee have nothing to say on this head which is at all new. They would remark, however, that by referring to the topography of that portion of Oldham County, Ky., from which they have a contribution, it will be seen that the causes usually considered as giving rise to autumnal fevers, were present to a considerable degree. When we turn to Memphis, Tenn., we shall see a remarkable coincidence in the general condition of the city and the state of health. In the first place, a vast amount of earth removed from one place to another. This upturning of earth, much mixed with vegetable and animal remains in progress of decomposition, has always been looked upon as a fruitful source of miasmatic fevers. Secondly: As if the exposure of such fresh earth was not sufficiently noxious in its effects, in levelling the streets a vast number of basins were formed, in which all the rain falling in their immediate vicinity was collected and retained. Here, too, were received considerable quantities of matters, vegetable, animal, and mixed, in which the only scavengers, the hogs, luxuriated in great delight. Thirdly: The great amount of filth, of various kinds, about livery and other stables, slaughter-houses, privies, &c.; and all without any attempt at drainage. The lessons of experience should not be slighted; and, therefore, it may be well to animadvert for a moment on the very remarkable parallel between the present condition of Memphis and that of the neighbouring city of Natchez, Miss., thirty years ago. The two cities are very much alike in several respects, both being situated on high bluffs; the surface of the site of each was originally very rough and uneven; the geological formation very similar, perhaps identical. From about 1817 to '20, the citizens of Natchez began to grade their streets. In prosecuting this improvement, the hills were dug down, and the hollows at the intersections of the streets were filled up, without any provision for the escape of water which might collect in the basins. From 1819 to 1827, the city was visited by a series of epidemics, on which it is distressing to reflect, even at this period of time. The effects of this course of "improvement" was

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forcibly placed before the city authorities at the time. In the language of Professor Merrill, "the population having been several times decimated by death, business and property greatly declined, and the city found itself scarcely able to continue the improvements, and scarcely worth them if made." But "war begets poverty; poverty, peace." Sickness caused a suspension of "improvements;" improvements being suspended, health returned. With returning salubrity came a new population; with the new population came, in 1834, '5, and '6, new "improvements;" which in turn were followed, in accordance with the predictions of the older citizens, in 1837 and '39, by new epidemics. "The eyes of the living were again opened to see their danger and its cause, and since then but little grading has been done." (*Merrill's Address.*) The city has again become remarkable for its salubrity. For a number of years past, Memphis has been "treading in the footsteps of her illustrious predecessor," and is now paying the penalty. As she has within her limits prophets which cry aloud, night and day, it is to be hoped she will yet hear them, before her reputation for mortality shall frighten emigrants from her borders.

Age, Sex, &c., most liable.—In Oldham, white males suffered almost exclusively. Of 14 cases reported by Dr. Swain, all were whites, 11 were males; 4 under 15 years, 6 from 15 to 40, 4 over 40. Average attendance, 5 days. No deaths.

From Pelham, Grundy County, Tenn., Dr. J. V. W. Conn reports 137 cases of intermittent fever; of these, 114 are whites, 23 blacks; 95 were males, 42 females; 24 were under 15 years, 86 between 15 and 40, and 27 over 40 years. Of remittent fever he reports 34 cases, of which 19 were whites, 13 blacks; 15 were males, 17 females; 15 were under 15 years, 17 from 15 to 40 years. Also, of bilious fever (probably continued) 9 cases, of which 6 were whites, 3 blacks; 5 were males, 4 females. All between 15 and 40 years.

Of the intermittents, the average duration of attendance was 5 days; of the remittents, 7 days. Continued, 14 to 24 days.

One male and one female died, both under 15 years of age.

The returns from Memphis throw no light upon this head.

Symptoms.—The principal symptoms, as detailed by Dr. Swain, of Ballardsville, were violent headache, sometimes delirium. Tongue heavily coated; nausea; bowels costive; pulse frequent, rising to 120 or 130; respiration hurried.

At Memphis, bilious fever appeared as a remittent in July, August, and part of September, when it changed to an intermittent, usually

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either a quotidian or double tertian. The attack of intermittents was usually ushered in suddenly and unexpectedly; the quotidian most commonly coming on during the forenoon, the tertian during the afternoon. The attacks of remittent were equally sudden. In this form there was usually considerable hepatic derangement, as manifested by vomiting of bilious matter, yellowness of the skin and eyes, as also by the colour of the stools and urine. Occasionally, very grave cases occurred, in which the biliary secretion was entirely suspended, there being no appearance of bile in the discharges from the stomach or bowels; urine heavily loaded, and jaundiced state of the skin. As cholera was prevailing to a considerable extent when bilious fever began, we might have expected diarrhœa to be a prominent symptom in the latter disease. Such was not the case; but, on the contrary, decided costiveness marked most of the cases.

Dr. Conn does not give us the symptoms which characterized the disease in his vicinity.

Dr. Armstrong, of Liberty, Ky., found bilious fever in his vicinity characterized by great pain in the head, bilious vomiting, bilious diarrhœa; high febrile excitement following the chill; tongue coated, becoming dark and dry as the disease advanced; muscular soreness and debility. Remissions were generally very slight until convalescence set in.

Treatment.—In the intermittent form, at Memphis, the prime object was to prevent a recurrence of the next paroxysm. Experience showed that one mild paroxysm was no guarantee that the next would not be severe, or even fatal. Hence, quinia was given at once. If any derangement of stomach or liver existed, they were attended to simultaneously. The nervous and vascular systems were brought, and retained under the influence of quinia until the disease was eradicated.

In the fever about Ballardsville, Dr. Swain bled sometimes. He occasionally vomited before the chill came on. To arrest the paroxysms, he used the following prescription: ℞. Pil. hydrarg. ʒiii; quiniæ ϑj; piperin. gr. iii; opii gr. i. M. pill. No. vi. divid. Of these pills, two were given five hours before the chill was expected, and repeated every two hours; so that the last two pills were taken one hour before the chill. The same prescription was repeated on the next day. As the disease is so very apt to recur at the end of two weeks, he always resumed the pills at that time, and always successfully. He had a case of excessive enlargement of the spleen, which yielded readily, as by a charm, to the exhibition of the

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liquor oxysulphatis ferri, prepared thus: ℞. Ferri sulph. ʒii or ʒiii; acid. nitric. ʒiii; aquæ destil. ℥iiss. Rub the sulphate of iron and nitric acid together, and add the water. Dose, ten drops, thrice a day, in sweetened water, increasing one drop daily, until fifteen are taken for a dose.

For enlargement of the liver or spleen, consequent on intermittent fever, Dr. John Travis, of Benton County, Tenn., recommends, with the utmost confidence, the following treatment: "I draw blood from the arm, give blue pill or calomel until slight ptyalism is induced; then reduce the salivation by flor. sulph. loti, supertart. potas., tinct. myrrh, and elm water. I give a one-grain quinia pill every hour in the absence of fever (I regard quinia as a stimulant), six or ten pills being given every day, until the chills are cured. I then give bitters to prevent their recurrence, and continue the bitters until the patient regains some strength.

℞. Ardent spirits 1 qt.
Rad gentian. ℥ii;
Aurant. cort. ʒi;
Fol. uvæ ursi ʒi;
Quiniæ gr. x. Mix.

Of these bitters, a table-spoonful must be taken three times a day, an hour before meals."

What has become of the liquid arsenite of potassa, as a remedy for obstinate and relapsing intermittents? In doses of ten or twelve drops, three times a day, it was used with very marked success thirty years ago.

The cold infusion of eupatorium perfoliatum also was very valuable for the same purpose. Have we anything better now? It is doubtful.

Bloodletting seems to be little practised in remittent fever at Memphis, the pulse rarely seeming to call for it. When used to mitigate any urgent symptom, or even in diseases of undoubted inflammatory character, it seems to be badly borne. The principal indications were to allay irritability of the stomach, restore or correct biliary secretions, remove constipation, and especially prevent the next paroxysm, if possible. For this purpose, anodynes, mercurials, and purgatives were employed separately, or combined, according to need, in each particular case. These were used, not so much to eradicate the disease as to put the different organs of the body in as healthy a condition as possible. For arresting the disease, reliance was placed on quinia. This was used according to the

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urgency of the case. Complete remissions were not waited for; on the contrary, it was given at any stage of the disease. Great frequency of pulse, whether during the paroxysm or apyrexia, was judged one of the strongest inducements to give quinia. The mode of giving it was in a single dose of from five to ten grains. If the pulse did not lessen in frequency, and increase in fulness and strength in three hours, another and larger dose was repeated. A moisture upon the skin was also considered a favourable effect from quinia. When the vascular and nervous systems were brought under the influence of quinia, the effect was retained by repeated smaller doses, until the paroxysm ceased to return; then the medicine was gradually withdrawn. By this gradual withdrawal, unpleasant tinnitus aurium, impaired vision, and other nervous symptoms, were prevented.

Where a patient did not bear the sulphate of quinia well, the valerianate was substituted.

Dr. Conn has not detailed the treatment adopted by him. This is the more to be regretted because different forms of malarial fever seem to have prevailed to a very great extent in his vicinity, and observations made by him could not fail to be interesting to the profession.

Dr. Armstrong, of Liberty, never bleeds if he can help it. Arrests vomiting by small doses of calomel. To reduce febrile heat, &c., he gives ipecac. and nitre, with cooling drinks, and sponges freely with cold water. To relieve headache, cups over the eyes. To "correct the system," pills of calomel and rhubarb, gamboge, aloes, and ipecac. at bedtime. When fever is somewhat subdued, he gives quinia and piperine united. Uses some tonic medicine during convalescence.

Dr. McNelley does not mention his treatment.

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Table intended to show at one Glance the Colour, Age, and Sex of those reported as suffering from Bilious, Intermittent, Remittent, and Continued Fevers, with the Mortality and Average Duration of Treatment.

REPORTED FROM		Colour.	CASES ATTENDED.							FATAL CASES.							Average days' attendance.	REPORTED BY
			Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.	Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.		
			M.	F.	M.	F.	M.	F.		M.	F.	M.	F.	M.	F.			
Grundy Co., Ten.	Intermittent	W.	10	7	50	20	20	7	114	...	...	...	...	...	...	...	...	
	"	B.	3	4	12	4	...	...	23	...	...	...	...	...	...	...	6	Dr. Conn,* Pelham, Grundy Co., Ten.
	Remittent	W.	4	6	4	5	...	...	19	1	...	...	...	...	...	1	...	
	"	B.	2	3	5	3	...	...	13	...	1	...	...	...	...	1	7	
	Continued	W.	...	...	4	2	...	...	6	...	...	...	...	...	...	...	...	
	"	B.	...	...	1	2	...	...	3	...	...	...	...	...	...	...	18	
Casey Co., Ky.		W.	19	35	18	28	8	5	113	...	...	...	1	1	...	2	14	Dr. Armstrong Brownsville, Ky.
"		B.	2	2	1	...	...	...	5	...	...	...	...	...	...	...	...	
Oldham Co.		W.	1	3	6	...	4	...	14	...	...	...	...	...	...	...	5	Dr. Swain, Ballardsville.
Lincoln Co., Ten.	Intermittent	W.	3	8	7	10	5	6	39	...	...	...	...	...	...	...	5	
	"	B.	1	...	1	...	...	...	2	...	...	...	...	...	...	...	...	Dr. McNelley,* Fayetteville.
	Remittent	W.	2	4	3	5	2	1	17	...	...	...	...	...	...	...	...	
	"	B.	...	3	2	1	...	...	6	...	...	...	...	...	...	...	8	
	Inf. remittent	W.	3	2	...	...	...	...	5	...	...	...	...	...	...	...	2	
	"	B.	3	...	...	...	...	...	3	...	...	...	...	...	...	...	...	
Bourbon Co.	Bilious inter.	W.	1	...	1	2	...	...	4	...	...	...	...	...	...	...	...	
	"	B.	...	...	...	1	1	...	2	...	...	...	...	...	...	...	...	Drs. Ray, Paris.
	Remittent	W.	3	2	9	4	2	...	20	...	...	...	...	...	...	...	...	
	"	B.	1	3	2	3	...	...	9	...	...	...	...	...	...	...	...	

Here would seem to be the most suitable place to introduce an account of an irregular form of fever which prevailed at Memphis, late in the fall; which the reporter, Dr. Grant, is scarcely willing to class with paroxysmal fevers. For the purpose of putting this fever in the most distinct view, it is judged proper to present it separately, and in the words of the writer.

AN ACCOUNT OF AN ANOMALOUS FORM OF FEVER, WHICH PREVAILED AT MEMPHIS, TENN., LATE IN THE FALL OF 1852.—At a late period of the sickness, last fall, I encountered a few cases of (to me) an anomalous form of fever, as did some others of my professional friends, which I confess myself at a loss where rightly to place in the nosology of febrile diseases. These cases came under observation at a time when scattering cases of cholera were occurring, and it may be were the production of the commingled influences of the malarious and choleraic poisons. Be this as it may, the symptoms, as well as

* No symptoms or treatment given.

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the failure of the treatment so efficacious in the purely paroxysmal fevers, serve to place it at a remote relationship, if indeed it has any affinity to these diseases. As I made short notes, at the time of its prevalence, of the leading phenomena which attended it, I will transcribe from them such prominent facts as I think may prove interesting to you, and may be of sufficient value to allude to in your forthcoming report.

The cases of fever, above referred to, were met with during the first and second weeks of November, being the week before, and the week after, the first frost of autumn.

Its early cessation after the commencement of cold weather, would seem to indicate its connection with *malaria;* but still it differed, in the malignity of its symptoms, from the well-marked cases of remittent fever, prevailing at the same time.

The attack, in most of the cases, was not preceded by any marked indisposition. Chilly sensations, or a well-marked cold stage, of short duration, ushered in the disease. The reaction which followed was attended by intense heat of the surface; eyes more or less injected; circumscribed flushing of the cheeks; pain in the back; headache; pulse ranging in different cases from 90 to 120 beats in the minute, and continuing throughout the entire course of the attack feeble and unresisting; the tongue at the commencement showed little evidence of departure from a normal appearance, but soon began to exhibit a line of dry and elevated papillæ, extending back along its centre, which continued to widen, of a dark brown color, exhibiting a notable contrast with the more or less smooth and red tip and edges of that organ. The thirst for cold drinks was intense. There was tenderness on pressure over the epigastrium. Nausea, and frequent vomiting of pale green fluids were troublesome symptoms. The bowels were not moved except by medicine, when the dejections were quite fluid at first, and of a pale green color. The urinary secretion was small in quantity, and highly colored, giving off a strong ammoniacal odour. Hemorrhage from the nose was common; in a few it occurred both from the nose and bowels; and in one case blood flowed from these and likewise from the uterus, in considerable quantity two weeks after the last menstrual period. Sleeplessness and jactitation were absent in no instance. Little or no delirium present; intellect clear. No eruptions discoverable. Great prostration of the vital powers present in all. The duration of the fever was from five to fifteen days, most of the cases being convalescent during the first week. It had, like those called continued, its morning periods of

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partial remission, but they were not the clearly marked remissions of the paroxysmal fevers prevailing at the time.

Taking into consideration the unpromising symptoms just detailed, the disease yielded more readily to treatment than would appear probable. The gastro-intestinal mucous membrane, which, as has been already mentioned, was so exempt from implication in the remittent fevers of the season, was now the part most ostensibly and seriously involved in the morbid manifestations attendant on this form of disease, call it by whatever name we may.

The remedy which in our hands appeared to exercise the greatest influence over the symptoms was *mercury*. After the bowels were evacuated, our reliance was placed on small doses of calomel and opium, repeated as often as the urgency of the symptoms in different cases required, and steadily persevered in until the disease yielded, which always took place on the first appearance of the constitutional effects of the medicine. As adjuvants, we employed local depletion and counter-irritants to the epigastrium. Iced lemonade, or pure ice water, or powdered ice, were freely allowed, and contributed not a little, we feel assured, in allaying gastric distress, and adding to the comforts of the sick. The great and infallible remedy of the treatment of periodic fevers did not succeed in the disease under notice, though a full and fair trial was made with it in several instances. Even as a tonic, after the subsidence of the disease, quinia did not appear to answer so well as the mineral acids.

The quantity of blood lost by some of the patients in this disease, and the manner in which the apparently prostrated vital energies sustained themselves under the loss, were truly astonishing. So far from being really injurious, we believe that the hemorrhage from the bowels was of signal service in relieving the conjested capillaries of the intestinal mucous membrane, as no death occurred where this condition was present. To restrain the hemorrhage within the bounds requisite for the safety of the patient, as well as to impart the salutary influence which it is generally believed to exert over the diseases of the mucous membrane, the spirits of turpentine was administered in small and repeated doses, with most signal advantage in the cases we are considering.

It is a fact worthy of note, that, so far as we have been enabled to learn the subsequent history of these cases, not an instance of relapse has supervened since recovery, now four months after the attack. Whether this circumstance, in connection with the unusual and malignant character of the symptoms attendant on the fever last

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described, should give just cause to dread the future in this city, we leave to the decision of those who are better informed than we profess to be in the etiology of febrile diseases.

Typhoid Fever occurred as an epidemic in two portions of Harrison County, Kentucky: 1, on Indian creek, the time of prevalence is not noted; 2, at Colemansville, it began about the 25th of Sept. In Oldham, the disease appeared throughout the year, but could not be said to be epidemic at any time. At Memphis, it prevailed most in June, July, August, October, and November. In Scott, it could scarcely be said to be epidemic, yet a few families suffered severely.

Persons most liable.—In Indian Creek, Harrison County, of 21 patients seen by Dr. Dille, 13 were males, 8 females. From the report, it is presumed that 20 were white, 1 black, though it is not so stated; 2 were 15, 3 were 20, 2 were 40, and 1 was 64 years old.

Of seventy cases treated by Dr. Hawkins, 67—51 males and 16 females—were white, and 3—1 male and 2 females—were black.

Of thirty-two cases, treated by Dr. Conn, of Grundy, Tenn., 25—18 males and 7 females—were white; 7—6 males and 1 female—were black.

Of thirteen cases, treated by Dr. Swain, 10—6 males and 4 females—were white; 3—2 females and 1 female—were black.

Of twenty cases by Dr. Sutton, of Scott county, 15—7 males and 8 females—were white; 5—3 males and 2 females—were black.

Cause.—The Committee have little to say upon this head, in addition to what was said in the report of last year. It may be remarked, however, that in a particular portion of Georgetown, Kentucky, there were quite a number of cases in September and early in October. These cases all occurred in a short time. This portion of the town is situated on a southern slope, at the foot of which, in wet weather, is a small branch; usually, however, there is little or no water in it. In the course of this branch, through one of the squares of the town, a ditch has been dug. During the summer, the sides of this ditch had been overgrown with rank weeds, which, before the fever began, had fallen into the ditch, so as to obstruct it to a considerable extent, and retain any trash which might be washed down it. One death occurred in the house to which this garden belonged. The portion of the town which suffered most is situated north-east of the ditch above mentioned, so that the south-west wind would bring any effluvium arising from the ditch to that quarter of the town. The fever commenced in the corner house nearest the ditch, in which house were several cases; the other cases were beyond and less directly

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exposed to this source of exhalation. With the exception of this quarter of Georgetown, most of the cases occurring in Scott County were in particular families, several cases occurring in one family (either simultaneously or more or less scattered), whilst the other families in the neighbourhood escaped entirely.

It is generally believed that typhoid fever and bilious fever do not prevail in the same neighbourhood, and depend in fact upon different causes. Dr. Grant declares that typhoid fever is becoming much more frequent in Memphis, without the remittent fever being at all less so.

Prophylaxis.—Dr. Dille believes the disease propagated by breach of hygienic rules and by contagion, and therefore advises great care and cleanliness in regard to the body of the patient, his bed and body-clothing, ventilation of his room, removing the carpets from the floor, free use of chlorinated lime about the room, bed, &c.

Symptoms detailed are very much those mentioned as characterizing this disease in the report of last year. By Drs. Swain and Dille, a dry hacking cough is described as particularly frequent. Drs. Dille and Hawkins saw an unusual number of cases of free hemorrhage from the bowels. Dr. D. saw seven (out of 21) cases in which the hemorrhage continued from four to eight days, two of which were fatal. [Cases attended with hemorrhage from the bowels rarely terminate fatally in this neighbourhood—W. L. S.] Dr. H. does not give the number of cases in which the hemorrhage occurred; but speaks of it as a common symptom, thus: "In about fifteen or twenty days the bowels poured out a large quantity of pure blood." Dr. D. speaks also of the countenance being more dusky or purplish than in other fevers. He saw no rose spots or sudamina; but about the twelfth or fourteenth day "profuse perspiration came on at regular periods in the day, and continued about four hours," in cases about to terminate favourably. Dr. Hawkins found "soreness throughout the system a very distressing symptom." He also observed in some cases a stage of prostration, from which it was extremely difficult to rouse the patient, came on about the eighth or tenth day. This stage was not perceived by the patient, but the nurse would be aware of it by feeling the toes, and especially the pulse, which many times could scarcely be perceived. Dr. Swain found profuse perspiration in the morning. In Scott County, Dr. Sutton found one case attended by a free efflorescence of erysipelas.

Treatment.—Dr. Hawkins avoids all medicines of a prostrating tendency, as bleeding, emetics, active purgatives, &c. He gives

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small quantities of blue mass for six or eight days, and then nothing, generally, but a mild purge. Calomel he has found too active. Ten or fifteen drops of spirits of turpentine, once or twice a day. When nausea is present, he gives warm salt and water, to throw off the contents of the stomach. When there is heat and dryness of the skin, he sponges with weak lye. This he has found particularly serviceable in reducing the heat and quieting restlessness. For pains in the stomach or bowels, or upon the occurrence of hemorrhage, he applies a blister to the abdomen. After hemorrhage, does nothing more than give sugar of lead for a day or two, and some mucilaginous drinks, and mild, unirritating diet.

Dr. Dille drew sixteen ounces of blood from the arm in *one case*, where there was a strong sanguineous determination of blood to the brain, which had a happy effect. The most successful mode of treatment was to give an emetic of ipecacuanha at the commencement, which was followed by calomel and Dover's powder. He used refrigerant diaphoretics from the first, of which citrate of potash, as an effervescent mixture, was the best. Kept the surface cool by cold or lukewarm ablutions. Allowed ice and cold drinks. For severe headache, cold water or ice topically. For hemorrhage from the bowels, acetate of lead and opium. To correct the nervous system, subsultus, &c., spirits of nitric ether, camphor water, and opium. In the latter stages of the disease, especially when there was some venous congestion which went off with free perspiration, he gave two-grain doses of quinia every three or four hours, until a general warmth was produced. When the symptoms still continued obstinate, he resorted to blue mass and opium, spirits of turpentine, &c.

Dr. Swain never saw a case requiring bloodletting. Tried six-grain doses of opium. Although he believed the opium was beneficial in all cases but one, yet he failed to obtain the fortunate results which followed the opium treatment by Dr. Henry, of Illinois. He meets the symptoms as they arise. Diarrhœa, with opium in some form; the cold stage, if any, with quinia; gives guarded doses of calomel when the liver demands it; moves the bowels with oil and turpentine, when necessary; suspends delirium and wakefulness with morphia; the sweating stage by sponging with tepid vinegar and water, with marked benefit.

"With our knowledge of the disease, I think, the less amount of medicine we get along with the better for all." In this sentiment, Drs. Hawkins and Dille agree, as indeed do our physicians generally.

Dr. Grant, viewing the disease as having a definite course to run,

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does not so much attempt to arrest it, as to watch; if any secondary complications arise, to control them, so as to keep the disease in as simple, uncomplicated form as possible, remove any local disturbances which may arise, and to guard carefully the *period of crisis*, which is almost sure to come sooner or later, by suitable stimulants. Yet he has been induced to try the "abortive treatment," and given sulphate of quinia in doses varying from a few grains to half a drachm, and repeated them until he became satisfied that they only lessened the frequency of the pulse in some cases; whilst, in all, they increased the restlessness, sleeplessness, tinnitus aurium, and deafness symptomatic of the disease.

In closing his remarks upon the treatment of typhoid fever, Dr. Grant says: "Judging from symptoms observed during life, which we have carefully endeavoured to watch and to analyze, and also from the appearances seen in a few cases examined in our presence after death, we are led to believe that pathological disorganizations of important viscera, proceeding from inflammation and its results, are rarely the causes of death in this disease, as it has existed among us. Our patients in typhoid fever, we would say, *die from a deficiency of the vitalizing principle in the blood, which imparts to the nervous system the power requisite to sustain and keep in action the organic functions.*"

In Scott County, Ky., the treatment did not vary materially from that pursued the preceding year. In the case attended by an eruption of erysipelas, the application of collodion over the eruption seemed to remove it. The removal of the efflorescence, however, had no effect on the course or termination of the disease.

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Table intended to show at one View the Colour, Age, and Sex of those returned as suffering from Typhoid Fever, with the Mortality and Average Days' Attendance.

REPORTED FROM	Colour.	CASES ATTENDED.							FATAL CASES.							Average days' attendance.	REPORTED BY
		Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.	Under 15 yrs.		15 to 40 yrs.		Over 40 yrs.		Total.		
		M.	F.	M.	F.	M.	F.		M.	F.	M.	F.	M.	F.			
Pelham, Ten.*	W.	4	1	14	6	...	...	25	...	...	...	...	...	...	...	...	J. V. W. Conn,
"	B.	3	...	3	1	...	...	7	...	...	1	...	...	...	1	...	Pelham, Ten.
Edmonson, Ky.,*	W.	...	...	15	10	17	2	44	...	...	...	...	...	...	5	30	Dr. J. L. Ford, Brownsville.
Harrison, Ky.†	...	...	...	...	...	...	...	70	...	...	...	...	...	...	5	...	Dr. Hawkins, Colemansville.
"	W.	...	1	13	5	...	1	20	...	...	1	...	...	1‡	2	...	Dr. Dille, Country.
"	B.	...	1	...	...	...	...	1	...	...	...	1	...	...	1	...	
Oldham Co.	W.	3	...	3	2	...	2	10	...	...	...	...	...	...	...	...	Dr. Swain, Ballardsville.
"	B.	1	...	1	1	...	...	3	1	...	...	...	...	...	1	30	
Scott Co., Ky.	W.	3	2	4	6	...	...	15	...	...	...	2§	...	...	2	...	W. L. Sutton,
"	B.	1	1	1	1	1	...	5	...	...	1	...	...	...	1‖	26¶	Georgetown.
Lincoln Co., Ten.	W.	1	...	2	1	...	...	4	...	...	1	...	...	...	1	...	Dr. McNelley,
"	B.	2	...	...	...	...	...	6	...	...	...	...	...	...	...	28	Fayetteville, Te.
Bourbon Co.	W.	2	1	3	2	1	...	9	...	...	...	...	...	...	...	...	Drs. Ray,
"	B.	3	4	4	...	...	...	11	...	1	...	1	...	...	2	...	Paris.**

In conclusion, it is judged not altogether inappropriate to introduce the concluding remarks of the very interesting communication on the diseases of Memphis, contributed by Dr. George R. Grant, of that city.

It has been believed, and asserted by high authority, that phthisis pulmonalis is not only not so prevalent in malarious as in non-malarious regions, but also that by a residence in localities confessedly abounding with malaria, the consumptive invalid might even hope and expect a radical cure. An inspection of the table herewith

* No symptoms or treatment given.

† Returns do not admit of the sexes, colour, and ages being arranged with certainty.

‡ This lady was aged sixty-four years.

§ Both these were delicate females, advanced in the disease when first seen. One of them had a large erysipelatous eruption on the shoulders and chest.

‖ This negro man was seen but once. He recovered so as to walk abroad. He was known to go into an apple orchard; after which he became suddenly worse, and died in about twenty-four or thirty hours. It is not known what were the symptoms of his relapse or the cause.

¶ If one case which lasted sixty-two days be taken from the list, the average will be twenty and a half days' attendance.

** One consultation case.

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sent will show, as far as it goes, that facts are not in accordance with this opinion; that disease is undeniably on the increase in Memphis, where malaria is almost as abundant as in the "Pontine Marshes."

Nor is that all. An examination of the mortuary table not only shows that *consumption* is the only disease which has consigned victims to our cemeteries, in every month of the past year, but it likewise informs us that precisely *one-half* of the 54 deaths, reported to have died by it, occurred from July to October, inclusive, the very season of the year when malaria is in most abundance, and is exerting its power to the fullest extent.

No one will run the risk of making himself so justly subject to ridicule as to say that these are *strangers*, who have come hither to spend their *summers* on account of the healthfulness of the place.

As far, therefore, as the facts furnished by *one* malarious locality can be depended on, it may be safely asserted that an exemption from pulmonary consumption is not attainable by a residence in a sickly place, much less ought a cure to be expected under such circumstances.

By reference to the mortuary table of Memphis, for 1851, furnished last year, it will be seen that those dying of consumption are not returned separately, as they are for 1852, but all diseases of the respiratory system are included under one head, and give a mortality of 89. If we add together those dying of consumption, croup, pleurisy, and pneumonia, in 1852, we shall have 88. It is, therefore, reasonable to conclude that the deaths from consumption in each year were not very different in number, thereby strengthening the conclusions of Dr. Grant.

In speaking of the influence of climate on scrofula, the following language was used, in 1846:* "The opinion that the removal of scrofulous, and especially phthisical patients to a southern climate, is very favourable to convalescence, has been so common that the idea advanced here will by many be considered as absurd. Many things, however, equally absurd in appearance, have been by strict investigation proved to be true." After referring to the *Med.-Chirurg. Review*, vol. xxx. pp. 52–59, for the results obtained from reports from the medical department of the British army to Parliament, the

* Essay on Scrofula, by W. L. Sutton, M. D. To which the Medical Society of Tennessee awarded the prize, May, 1846. See *Western Journ. Med. and Surg.* vol. vi. new series, p. 386.

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following opinion of the reviewer is quoted: "This, we should think, may open the eyes of some physicians, who think a hot climate a panacea for diseases of the lungs."

We would refer, also, in reference to this subject, to the authority of Dr. E. H. Barton, of New Orleans. His remarks, corroborative of the same general fact, will be found in his report, contained in the fifth volume of the *Transactions of the American Medical Association*, page 579.

It will be remembered by many members of the American Medical Association, that the late Professor Drake, in offering a paper on *The Connection between Climate and Pulmonary Consumption*, remarked that the facts obtained by extensive inquiries and observations, had led to results very widely different from what he had anticipated. What those results were is not known to this Committee. Probably they coincided with those obtained by Dr. Grant. At any rate, it is deeply regretted that unnecessary scruples on the part of Professor Drake prevented that paper from appearing in the *Transactions*.

W. L. SUTTON,
THOMAS LIPSCOMB,
E. B. HASKINS,
F. A. RAMSEY,
A. EVANS.

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ON

ACUTE AND CHRONIC DISEASES

OF THE

NECK OF THE UTERUS.

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ON

ACUTE AND CHRONIC DISEASES

OF THE

NECK OF THE UTERUS.

BY CHARLES D. MEIGS,

OF PHILADELPHIA.

THE Committee on the subject of Acute and Chronic Diseases of the Neck of the Uterus, after carefully considering the ends proposed, and wishing not to exceed the limits assigned by their appointment, beg leave to report a plain statement of the subject as it has appeared to them; with the addition of such explanations, illustrations, and advices, concerning treatment, as may, perhaps, best subserve the ends of the call.

The Committee have not understood that they are commanded to present a complete summary of whatsoever has, of late years, been proposed by numerous writers upon this topic, and have therefore prepared such a report as they venture to hope may, to some, prove useful as a help to clinical practice in an important and extensive department of medical and surgical treatment.

Clinical experience coincides with daily common observation to convince the Committee, that it is, in general, held to be a difficult thing to comprehend the various disorders of the womb, and appreciate their peculiar reaction upon the female constitution. Notwithstanding the learned and elegant works we possess, it does appear that this class of maladies, as to the clinical duties connected with their management, are not so well understood by physicians, in general, as they ought to be; and, that this is a chief reason why many of the cases tend, at last, to fall under the care of certain individuals instead of remaining in charge of the family physician, usually first consulted in relation to the complaints. This tendency to specialization may be regarded as an evil in the profession, and an unneces-

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sary one, since a little consideration should convince us that this class of diseases presents to the practitioner nothing more essentially intractable, nor anything more inscrutable, than the ordinary affections submitted for counsel. There would be less propensity to bring them into the state of a specialty in practice, if the treatises now in our possession were as clear in the description and illustration, and as precise in the indication and relation of methods, as they ought to be. Far from this—we have to regret that many of the drawings and colored prints serve rather to obscure, than enlighten the path of the practitioner. It needs only to glance, for example, at the plates to Madame Boivin's work, to see that this remark is quite just, while the more elegant volume of Dr. Hooper affords as little aid—the engravings, however beautiful, exhibiting for us only the ravages of diseases ending in death, and not those curable forms and stages —which are far more worthy of investigation, as guiding us in all our attempts to cure.

It has seemed to the Committee, that, before entering upon the examination and description of the special cases, they are called upon for a few remarks concerning methods of proceeding—the usefulness and propriety of which have, by some, been sharply called in question, while others have contended that they are indispensable to correct diagnostication and treatment.

We allude to the method introduced by M. Récamier, Physician to the Hôtel Dieu, at Paris. That celebrated professor, in his work entitled *Recherches sur le Traitement du Cancer*, 2 vols. 8vo. Paris, 1829, says, at page 317, t. i. :—

"Dès l'année 1801, consulté fréquemment pour des maladies organiques de l'utérus, je commençai à traiter les ulcères du vagin et du museau de tanche, de la même manière que ceux de la gorge, qui m'en donnérent l'idée. Un tub d'étain du volume du doigt, et de quatre ou cinq pouces de long, servait à ces pansements, dans lesquels les surfaces ulcérées étoient touchées avec un pinceau de charpie trempé dans du miel rosat, ou dans du rob de Daucus, seul ou associé avec du collyre de Lanfranc, du laudanum de Rousseau, ou de l'extrait d'opium."

M. Récamier further informs us, that, in 1812, M. Bayle inquired of him concerning his methods, and that the facts he communicated to that author were made public in the *Dict. des Sci. Méd.*, art. Cancer, which notice is found in tom. iii. p. 604, as follows:—

"Déjà M. Récamier est parvenu à porter differentes substances médicamenteuses immédiatement sur l'ulcère, à l'aide d'un pinceau

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conduit dans un tube de gomme élastique ou de métal qui écarte les parois du vagin, et embrasse dans son orifice supérieure toute la partie saillante du col de l'utérus. Ces essais, conduits avec prudence, conduiront peutêtre un jour à quelque méthode de traitement, plus efficace que celles aux quelles nous sommes encore réduits."

The great success, in treatment, that attended M. Récamier's practice at the Hôtel Dieu and the city generally, led to the introduction of *his* method—the surgical method of curing the disorders of the cervix; and the question now is, whether the advantages resulting from it are sufficient to counterbalance certain evils supposed to have grown out of its introduction. These evils are said to consist in the great mortification which any sensible woman must experience, if subjected to a metroscopic examination; some laxity of the moral sense likely to grow out of such proceedings; the physical injury, by contusion or laceration, sometimes attending the operation; and the mischievous effects of drugs and instruments, to the employment of which it leads. Further—it is asserted to be unnecessary, since the exploration by touching, as it is called, reveals sufficiently the nature of diseases attacking the cervix uteri.

The Committee will not here discuss the moral differences between a diagnosis made by the operation of touching, and a metroscopic one. No one will deny that the delicacy of those relations that exist between medical people and their female patients opposes, in a variety of cases, an insuperable barrier against any successful treatment, and medical men, it is presumed, must (often) prefer to adopt an empirical method, or—to use a common phrase—to guess at the seat, nature, and phasis of the disease, as well as the remedies for it, rather than resign the woman to hopeless distress, on the one hand, or, on the other, urgently insist upon her waiver of that fastidious *bienséance*, which ought to a certain extent to rule her professional relations with the medical adviser.

Even to examine by touching, does, in many parts of the United States, appear to be a revolting proposition; and there are many persons who prefer to make journeys of hundreds of miles, in order that such an inquiry, supposing it indispensable, may be instituted by a stranger, and far from home.

Some examination is, nevertheless, often indispensable to correct information as to the sanitary wants of the patient; and physicians cannot always rely upon the historical relation of the patient herself, or her friends, who assuredly cannot impart knowledge which they do not themselves possess, and can by no means acquire.

It may be farther averred, that the touch alone does not surely

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impart this knowledge; for as much as, a tint or colour is often the clearest exponent of a morbid state, it becomes sometimes an imperious necessity to ascertain it by the metroscope. The question is, then, not whether the instrument informs us better, but whether, without its aid, we can at all learn our therapeutical or surgical duty to the subject.

The objection as to danger of injury, and also as to the temptation to make use of too violent topical remedies, is a question as to the good sense and prudence, or conscientiousness of the medical man—subjects not for us to discuss. Yet, we may adventure the remark, that no member of this house will deny, that, in our profession in these United States, there exists a great majority of gentlemen, competently educated; men skilled in their art; men of probity, and having elevated views in morals and religion; and, as to life and conversation, irreproachable.

If the whole class of cases under consideration should be confided to such persons as these, little objection would be made to their professional opinions or practice. There is conceded to them, by common consent, as well as by governmental authority, a "*jus docendi scribendi atque exercendi ubi rite vocati fuerint;*" and the public esteem them to be warranted in all their acts, done within the metes and bounds of their professional privileges. They have no authority to violate those rules, and arrogate privileges beyond; and they do so at their own peril.

The question seems to be, then, still more narrowed down; and we should determine what is, and what is not, strictly within the limits of our duty and prerogatives—things to be resolved, not by formal council, but by the physician himself, upon the spur of the occasion. We must not repudiate, and, by wholesale reprobation, condemn and annul a good method of cure, merely because incompetent or wicked persons may abuse it to their own gain. To do so, would be to amerce and punish the sick and the miserable for faults of our brotherhood.

Lastly.—Even if it be true that some abuses, disgraceful to medicine, do exist, we are persuaded that such discreditable examples must be very rare among a class so generally acknowledged for wisdom, virtue, and politeness, as the great body of the United States' physicians. Wherefore the Committee do not find themselves called upon to discountenance and reprobate the prudent and necessary employment of the modern methods and instruments, invented to improve the means both of diagnosis and surgical treatment of diseases of the cervix uteri; conceiving that this is a matter to be left

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to the conscience and judgment of their brethren, whenever the occasion may arise.

The reactions of the reproductive organs upon the other members of the animal economy are known to be both diverse and comprehensive; nor is it difficult to perceive that this should be the case, seeing the great importance of their functions. One could hardly suppose, indeed, that a system of anatomical tissues, representing the complement of the vital powers, could be an indifferent in their constitutional reaction. The powerful sway they exert upon not only the physical, but the psychical condition and nature of the woman, might persuade us that changes in their vital status, even such as are inappreciable except by the understanding, should greatly affect both the body and mind of the woman.

The reproductive force can never, in fact, be an indifferent, as regards the health and perfection of the woman—who is an imperfect being, until she has, at full puberty, acquired this complementary force, and who begins to decline from her perfection as soon as she lays it down at the change of life. During all the period in which she possesses this force, even any slight modifications in it affect the direction, as well as intensity, of many others of her physiological actions.

It is probable that the hysteric malady, which assumes such diverse forms, and comprehends in its range, so many separate and distant organs, depends essentially upon a vital *status* of the reproductive viscera, often so slight as to escape all sensible detection. It is, at least, certain that the sexual diseases do give rise to severe distress in distant regions of the body; and, while they can incommode, vex, and even destroy, by degenerating the womb, the ovaries, or other parts, they can likewise subvert the health, and destroy the life of the woman, by perverting the strength and soundness of other vital portions—the radiating point of the mischief being, in the mean time, concealed in the depths of the pelvis, and wholly masked by the constitutional disorders thus set on foot by them.

A dark and mysterious veil hides from us many of the laws that grow out of the intimate relation and mutual dependency existing between the conservative or generic forces of animals, and their reproductive or genetic powers. Many stumbling-blocks in the path of the practitioner would be taken away, if these laws and relations could be fully understood; and we should then be able to take more precise indications, and adopt more positive methods of treatment.

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Possessing full anatomical and physiological information upon those organs and forces, we might raise up in the mind a true IDEAL of them, which would serve as a standard or scale by which to measure and judge every aberration of form, power, or place, in the instances brought before us.

And it is proper, indeed, that the physician should endeavour to acquire the perfect IDEA of the organ whose state he is about to determine. This determination he only can make, who hath already erected his ideal standard, as now proposed. Otherwise, he cannot but frequently err in his diagnosis. But he who hath ever at hand in his scientific store, a perfect *ideal* of the healthy organs, shall scarcely err, since, in every diagnostication, he will strictly compare the real with the perfect ideal, or standard, and, from the deviations and aberrations, deduce a perfect knowledge of the case before him.

In order to raise up such an ideal or standard of the uterus, it is necessary to analyze its component textures, and arrange them; for example, into a series, as: 1. The serous tissue. 2. The mucous tissue. 3. The sanguiferous; 4, absorbent; 5, the muscular tissue. 6. The Nervous substance. 7. The glandules and follicles. 8. The areolar texture.

These are the only things in the uterus that can be subjects of disease, whether severally or conjointly. When they are combined in the just ratio of mass crasis and vital force, they represent the form and substance of the healthy womb, and, we might add, its faculties also; whereas, every deviation from such normal ratio of the elementary tissues, represents a certain disease of that organ.

Equable augmentation of the elementary tissues constitutes hypertrophy of the uterus—which may be either local or general.

Hypertrophy of the womb may be physiological or pathological.

The term physiological hypertrophy here applies to those equable augmentations of the elements of the uterus, that take place in pregnancy, in which the womb acquires, in the course of 280 days, a great volume, and a weight of 25 to 30 ounces. In the non-gravid state its length is about $2\frac{1}{4}$ inches: in pregnancy it is 12 inches in length. The non-gravid weight is $2\frac{1}{2}$ ounces, whereas it, not rarely, weighs two pounds, at term. These great changes occurring in mass, while inconsiderable changes in the ratio of elements take place, can only be the result of a physiological hypertrophization—and it is well to make such a reflection, seeing that, the womb recovers its non-gravid form, substance and faculties, in about thirty days after the termination of pregnancy.

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In this process of normal hypertrophy, the several elementary textures are evolved in their equal generical ratios; and the same kind of equable development takes place in all the true cases of pathological hypertrophy.

As the womb readily recovers from any physiological hypertrophy in about thirty days after the removal of the cause, so, in like manner, it recovers from its pathological hypertrophy soon after the removal of the cause. Such cause may be a polypus developed within the cavity; chronic inflammation of the cervix or os uteri; dislocated states of the uterus, in retroversion, or anteversion; or any circumstance that should provoke the organ to grow inordinately, but equably as to its elements.

Unequable development of the several elements, converts the womb into a tumour, degenerates it, and renders it incurable by medicines; whereas, the state of pathological hypertrophy is to be recovered from upon removal of the cause.

If, in a womb, the areolar or the vascular, or the nervous element, should be augmented several hundred per cent. beyond its normal ratio, the rest of the tissues preserving their quantitative relations to each other, the viscus would be changed into a hæmatoma, a cephaloma, &c., &c., according to the nature of the case or chief constituent element of such tumour.

Unhappily, the womb is, by nature, peculiarly subject to such changes. It may pass unharmed through the hypertrophizations and recoveries of many consecutive pregnancies, because of its innate aptitude to recover its non-gravid form and substance; a reflection this that affords us the greatest encouragement, when called on to undertake the treatment of cases of enlarged womb. So true is this, that, in all cases, where we can clearly diagnosticate a uterine hypertrophy, we may found on it a happy prognosis, provided we may also reasonably expect to remove the cause of the excessive growth, *e. g.*, if the cause should prove to be a mere retroversion, we may count upon a cure if we can reposit the womb and maintain it in its proper attitude in the pelvis. This is, in fact, all that we shall be called upon to do; for, this being done, the organ hastens to recover its non-gravid form and substance, as after a delivery at term, or after an abortion.

A perfect ideal of the normal womb, one fit to serve as a standard, or scale of comparison or measurement for cases, should comprise, in addition to a notion of its elements, one of its form, volume, place, posture in that place, sensibility, resistance, complexion, and all its powers as well as its anatomical relations or connections.

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A great help to the formation of this ideal standard, may be found in drawings and engravings. And here we beg leave to submit a figure (Fig. 1) copied from nature. The womb, which was that of a young unmarried woman, was 2½ inches in length, by 1¾ in width at the widest part. Its weight might be 2½ ounces. The specimen, a recent one, was presented to us by Dr. Addinell Hewson, of Philadelphia. We regard this uterus as a fair standard specimen.

In regard to this drawing, and indeed all the figures made use of as illustrating points treated of in this report, we owe an apology for their want of artistic excellence. It is clearly out of the question that we should show these things to artists, as well as that no artist could represent them from mere description. We did try that experiment, which failed. We were then compelled to become artists in self-defence, and succeeded, as is here to be seen, in making very inelegant, but very faithful representations of the subjects of the sketches. As to their fidelity we have no other assurance to give than such as we may refer to as professional experience; and we feel assured that all those physicians who have been obliged to acquire this painful experience must concur in the opinion that our drawings are very truthful.

The figure, it is therefore believed, gives a fair average representation of the form and size of the human womb. The chirurgical neck projects within the remains of the vagina, to the normal depth; and the aperture, or os uteri, preserves its appearance as a transverse sulcus.

Fig. 2 represents an antero-posterior section, and exhibits the left symmetrical half of the womb; showing the thickness of the walls, in this direction; the size and shape of the cavity; as well as those of the canal of the neck; the two lips, anterior and posterior; and the length of the vaginal neck or surgical neck.

Fig. 3 is a transverse section of the womb, exhibiting the posterior half, with the shape and size of the cavity; as well as the canal of the cervix, and the orifice as seen in this way.

Fig. 4 is designed to give an idea of the womb's place, and position or altitude in the pelvis. It is on a scale of half an inch to the inch; and shows how far below the plane of the superior strait (see dotted line) the fundus uteri should be found.

Most of the published drawings have erroneously exhibited the fundus uteri either as high as the plane of the strait or even elevated somewhat above it, which is incorrect, and leads to considerable errors in diagnosis.

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W. Dreser Del.

T. Sinclair's lith. Phila.

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Fig. 1.

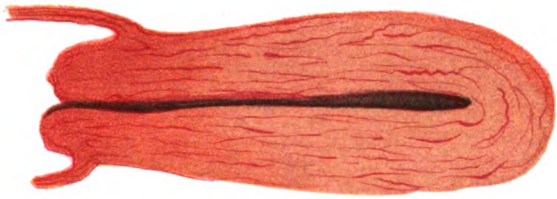
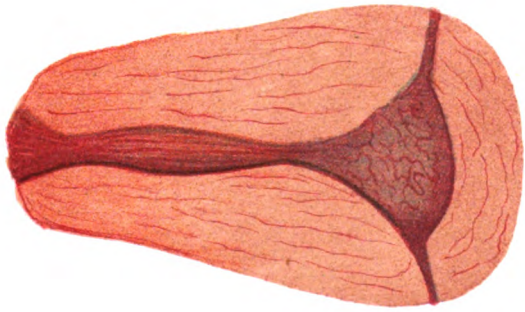


Fig. 2.



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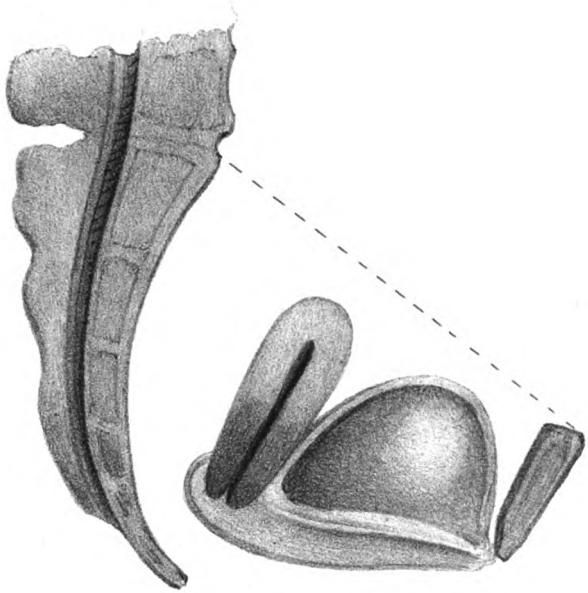

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Considering that the plane of the ischium is 3½ inches long, and that the mean length of a normal womb is above 2½ inches, the figure may serve to give a correct idea of standard as to place and altitude. Since the diagnosticator, by touching the os tincæ, at once recognizes the place in the excavation where it is found, as whether high, or low, and he also determines by palpation at the hypogastrium, whether the fundus is too much elevated or no; and so he judges of the length and volume of the womb he is examining.

The anatomical relations of the womb are those which unite it with the bladder, in front; the vagina inferiorly; the broad and the round ligaments, ovaries, and Fallopian tubes at the sides and angles; while posteriorly, it has no anatomical relations whatever; that surface being there completely invested with a peritoneal covering.

The standard should comprise a true idea of the os tincæ or aperture of the canal, many deviations from the standard being observable in practice; all of which may afford useful lights in diagnosis, prognosis, and practice. Certain changes of its form may be regarded as pathognomonic.

The sensibility of the uterus to pressure or contact; its resistance, and flexibility, ought also to form part of the ideal; and lastly the colour or tint—as observed in the metroscope—as whether pale, rose-tinted, raspberry-red, violaceous; or even of a hue approaching to that of melanotic tissues, constitutes an important item in the aggregate of characteristics.

Experience has shown that many instances of bad health in women are to be attributed to acute or chronic ailments of the womb, where no topical pain or other sign of local lesion is complained of.

Thus some of the examples of perverted innervation exhibited in torpor of the bowels, chronic tympanitis, frequent attacks of spasm, and general convulsion; cephalalgia, palpitation of the heart, pain in the back, loins, and lower extremities; fluor albus, and aggravated hydræmia, take their origin from disease of the cervix uteri. In so far, therefore, as these affections spring from acute and chronic disease of the neck of the uterus, they might be assumed to come within the scope of inquiries assigned to the Committee; but since an elaborate statement of all those subjects would fill a volume rather than a report to this Association, we shall confine our remarks within narrower limits.

Among the number of sexual disorders, few are more frequently complained of than leucorrhœa, and it seems probable that most

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women have occasion, at some period of life, to complain of it. In general, it gives little inconvenience, is of short duration, and ceases spontaneously; not requiring for its cure the advice of physicians. Perhaps in a thousand cases of moderate fluor albus, the physician would be consulted for one or two only. This, however, is not always the case, for in certain exceptions the secretion becomes very great, and is attended with debility, pain in the back and loins, a sense of bearing down or weight in the pelvis, with heat in the passages, which passes into insufferable pruritus. Instances in which the profluvium is very great, will be rarely met with in practice; and not many of the subjects are found to require the use of the usual guard-napkin, which could not be dispensed with if the secretion were so abundant as is by some supposed. We have heard of cases of leucorrhœal discharges to the amount of eight ounces per day, but probably such examples occur not more than once or twice in a long career of medical practice; one in which the secretion should equal a table-spoonful per day, even, would be very troublesome, as well as uncommon, the majority of the patients complaining only of an inconvenient humidity of the parts.

Where the discharge in leucorrhœa consists only of vaginal products, it may safely be assumed that such leucorrhœa is of very little consequence as to the woman's general health. We speak here of the moderate cases. Indeed, affections of the vagina may, for the most part, be considered to interest very little the female constitution. Such a statement as this might excite surprise, in those who are accustomed to hear of the very debilitating effects of fluor albus; and it becomes necessary to qualify the statement, by showing that, in those cases where the health has appeared greatly to suffer, the essential malady is not in the vagina, but in the neck of the uterus itself.

Patients do not, in general, make any discrimination between the several different appearances presented in fluor albus; because they are satisfied to believe that excessive humidity of the genital mucous surface is fluor albus, or vaginal leucorrhœa.

While our own observation convinces us that profuse vaginal secretion is rarely met with in practice, we are equally convinced that some of the patients are rendered too moist by excessive activity of Duverney's glands, and, that the most mischievous of leucorrhœal evacuations is that which comes from the canal of the cervix of the womb.

The muciparous glands of the vagina furnish either a thin watery

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mucus, or one of a creamy consistence, which, in other instances, is almost butyraceous, or concrete.

The excretion from the follicles and glands of the canal of the neck, is always gluey or albuminous, and resembles fresh white of eggs; and, when the patient, in describing the disorder, informs us that she discovers a slimy mucus, and especially if it appears at intervals of once a day, or oftener, we may take for granted that she labours under inflammation of the neck of the womb. There is no apparatus in the vagina itself for the secretion of this albumen; but the abundant follicles of the canal of the cervix are devoted solely to such production—when the cervix is chronically inflamed, that production is much increased; to that degree, indeed, as to impart a character of sliminess to the discharges, and make it resemble *albumen ovi*.

It very often happens, to observe this transparent viscid mucus to be wholly unaccompanied by any vaginal excretion; the substance coming away from the os uteri, and escaping upon the napkins, without the least admixture. In using the metroscope, the same albuminous matter is observed to be oozing forth from the orifice of the womb only, and so tenacious that it cannot be wiped away with the sponge.

Those physicians who have attended women in labour, are familiar with the viscous excretion from the neck of the womb, which generally discharges considerable quantities of it, while the os is undergoing dilatation. In a state of ordinary health, the production—which is equal merely to the physiological demand—is so inconsiderable, that the albumen is not to be observed; but any augmented vital activity in the cervix may make its presence manifest.

It has already been remarked that subjects of leucorrhœa complain of it as a weakening disorder; nor can it be denied that this albuminous fluor from the cervix mostly coincides with a sense of great lassitude and debility—not because of the wastage of the material, but because of the disturbing effect in the general economy, produced by even slight modifications of the health of the uterus.

Physicians consulted for these leucorrhœal affections, ought, therefore, to discriminate between the different kinds of discharges. When the excretion is deemed to proceed from the muciparous apparatus of the vagina only, it may suffice to make astringent injections, whether of mineral or vegetable sorts. In case such remedies prove unavailing, the vital activity of the mucous body may be changed by solutions of argent nitrat., of feeble strength—and one might confidently look for a cure under such prescription. Yet, in fact, our or-

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dinances for those disorders are notoriously unsuccessful; and the reason is, that the vaginal leucorrhœa is either a rare, or a trifling complaint, and we vainly attempt to cure—by these methods—a disorder of the cervix whose existence we do not suspect, and which would not yield to the method if we did.

We repeat, that the serious cases are cases of disease of the cervix—and a vaginal injection for inflammation of the canal of the neck, is, simply, ridiculous. The albuminous leucorrhœa is a sign of inflammation of the cervix, in which is included the canal, with its copious muciparous apparatus. It is as much a surgical disorder as an ulcer of the leg, or an anthrax, or conjunctivitis. When the surgical disorder is cured, the sign disappears. Hence we desire to express the opinion, that such leucorrhœas are to be held as acute or chronic inflammations of the canal of the neck, and to be treated accordingly.

The most essential element of a successful practice exists in a positive diagnosis. It will not do for us to believe, or suspect, or infer, this or that—we must guard the interests of the patient by knowing it is this, or that, or the other form and stage of a disease.

The historical or descriptive account delivered by the patient, or her friends, cannot serve as guide for us; nor can t h e y compare the actual state of the suffering organ with that perfect ideal standard, by which w e ought to compare, weigh, and measure, everything brought before us for judgment.

It must ever be a very trying occasion, that of a sensitive and delicate woman, who is brought into such a strait as to require a physical examination of those parts which naturally shun exposure. The medical man, who has even a common share of sensibility, will always, therefore, defer this last resort as long as possible, while he makes use of empirical treatment—if, haply, he might thus be enabled, to effect a cure without the waiver, on her part, of those honorable scruples which deserve from him the most perfect respect.

Of all the means of making a physical diagnosis, touching is the easiest and the least distressing to a woman of sensibility, bad though it be. By touching, we can determine the place, the volume, the resistance, the sensibility, the smoothness or roughness, the patulousness, and other characteristics of the organ.

Where the touch gives sufficient information, let the Touch suffice; but, if any doubts as to the wants of the case remain, then a metroscopic examination should be made. If declined—let the consequences rest with the patient; the physician is absolved from blame.

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There are a great many sorts of metroscopes now in use—some consisting of a tube, as originally proposed by Récamier, but composed of glass, or silver, or ivory, &c.; and some consisting of a bivalve, trivalve, or quadrivalve speculum uteri. Each of which is preferred, according to the taste or caprice of the practitioner.

The most reliable metroscope, probably, is a slightly conical tube of silver, six inches and a half in length. The uterine extremity should be one inch in diameter, bevelled with an angle of thirty-five or forty degrees. The outer, or larger extremity, should be one inch and a half in diameter. The silver should be highly polished, with the edges of the bevelled end rolled and rendered blunt, lest they might catch in the folds of the membrane, or even wound the cervix.

An olive-shaped piece of wood, secured in a steel handle, and made to fit accurately in the smaller extremity of the cone, serves to conduct it without pain to the bottom of the vagina, whereupon it is withdrawn, in order that the surgical neck may engage in the opening, and thus enable the surgeon to discern any marks of disease there.

The light passing down the tube ought to be as clear as possible, and the inner surface of the metroscope ought not to have a very high polish, lest, serving as a reflector, it should pour a flood of chromatized light on the parts, and thus give rise to the greatest misapprehension of their real condition. It would be better to have the inner surface painted with black, in order that no reflection from the walls should deceive us, and lead to error.

In using the glass reflecting speculum, covered with Japan varnish on the outside, a great deal of prismatic light finds its way to the part, and gives the idea of red inflammation or engorgement of the mucous body, which is only corrected by a circumduction of the instrument. This could never happen in a blackened and dull-coloured tube.

An instrument on the plan above proposed can disclose the physical appearances of any square inch of the inner walls of the vagina; for by merely rotating the tube on its axis, and directing the bevelled end this way or that, we may at leisure observe any part of the cervix or vagina.

The bivalve and quadrivalve speculum are sometimes convenient; but they as often annoy us and obstruct the research by allowing folds of the vagina to fall in between the parted blades, and thus completely hide the parts sought to be studied. They are by no means suitable for the application of the cautery, whether liquid or

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solid; and no one could conveniently make use of leeches by means of them. In any case, when the multivalve speculum is applicable, M. Recamier's tube is more so, on which account, we prefer it altogether.

Plate 4 represents a Récamier metroscope, with its guide. This instrument is of silver, and bevelled in a way to facilitate the operation, in engaging the os tincæ in the open end of the tube. It has not any handle, which, moreover, is both useless and troublesome, by catching in the bed-linen or napkins with which the patient is always enveloped on such occasions.

With a Récamier tube, one can readily observe any, even slight modifications of form or surface, hue, abrasion, vegetation, druses or botryoidal excrescences. Small polypi, jutting from the canal of the neck, any fissures or rhagades, ulcerations, cauliflower degeneration, or open carcinoma, &c. &c., that shall have given rise to the symptoms may be thus seen. The same occasion may be seized to make such surgical dressings as may be indicated, and the sacrifice of sensibility in this way made by the sufferer, is almost sure to be rewarded by a speedy cure of any curable disorder, which, but for such sacrifice, would continue to make her unhappy, leading, by a perpetual progress of deterioration, to ruined health, and, finally, to the last refuge of the miserable, which is the tomb.

A diagnosis by the metroscope should be made in the daylight, and in presence of a third person.

The patient should lie on the back, near the side or foot of the bed; the head, and not the shoulders, resting upon a pillow. A sheet or spread is to cover the person; the margin of the covering to descend nearly to the floor.

The knees should be much flexed, and the feet near to the trunk of the body.

Before proceeding to adjust the instrument, an examination should be made by touching, to ascertain the precise position of the uterus, and the place occupied by the cervix; the sensibility, resistance, volume, &c., of the neck, as well as the state of the aperture of the womb.

There should be provided a speculum-forceps, and some small bits of moistened sponge, which, being held in the forceps, serve to absorb or remove any mucus or slime, or sanguineous excretion. The most convenient speculum-forceps that can be got is probably a bullet forceps; such as the one proposed by Professor Gibson.

The speculum-forceps of Charriére is far less convenient.

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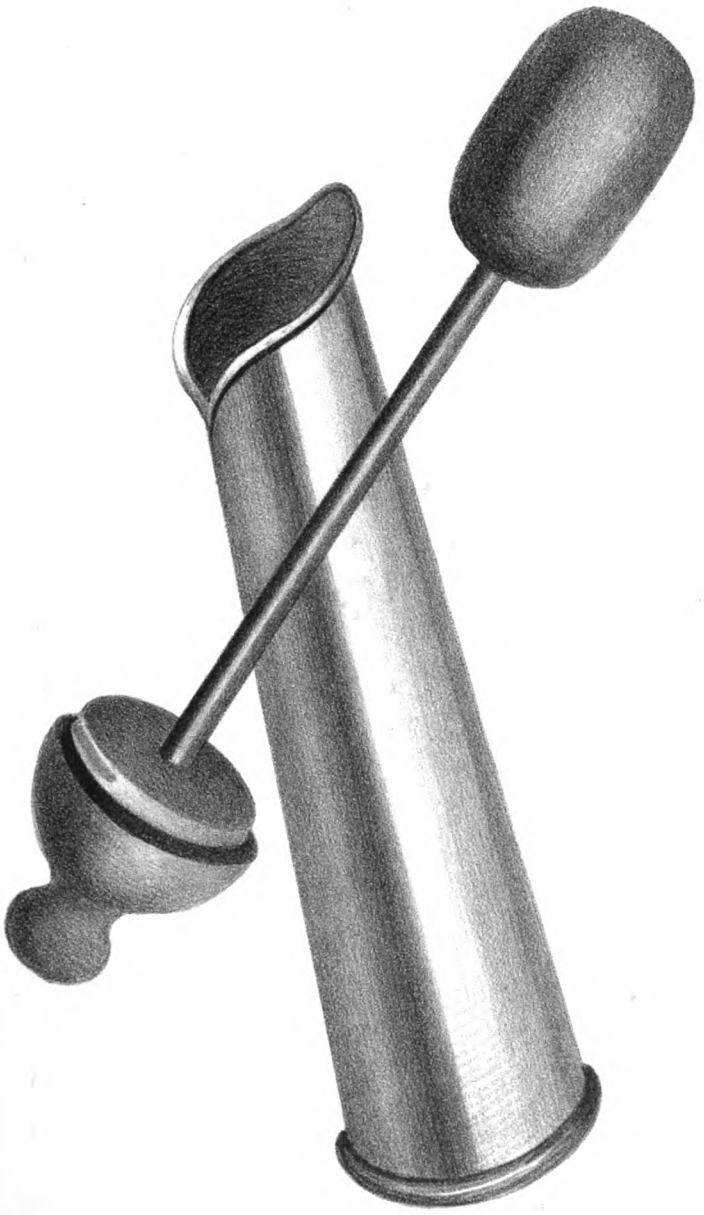

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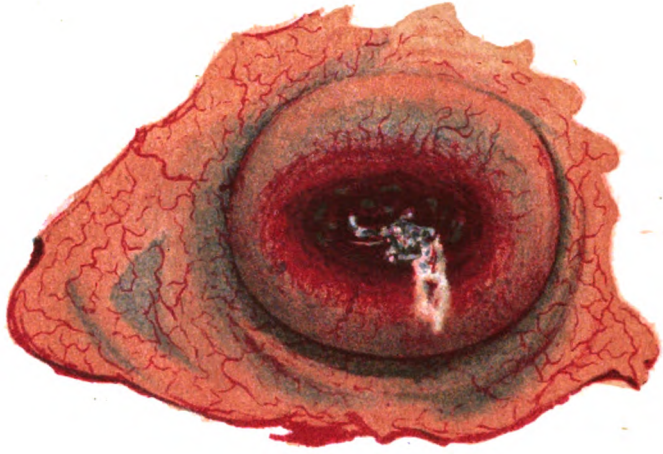

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If the metroscope should now be adjusted beneath the coverings, and if the margin of the sheet should be carefully wrapped around the cylinder, it is easy to conceal, in this way, the entire person of the patient, while the light, falling down the tube, serves to reveal any existing signs of disease of the parts to be examined.

If these inquiries are instituted for the purpose of determining the precise cause, and learning the proper treatment of a troublesome leucorrhœa, especially one characterized by the albuminous discharges heretofore mentioned, we shall rarely fail to observe a positive inflammation of the cervix and os uteri, or to notice a certain quantity of transparent viscid phlegm, oozing slowly away, or tamponing the orifice of the canal of the cervix.

One or both of the lips of the womb will be found tumid, softened, granulated, or botryoidal in appearance, and of a uniform red; or else drusy as to the surface, and presenting some resemblance to the surface and colour of a ripe raspberry; whence it has been the custom of some to speak of this form of inflammation as *inflammation framboisée*, a French word that very aptly expresses the idea of it.

Annexed is a drawing (*vide* Plate 5), by which we hope to impart a correct idea of one of the forms assumed in inflammation of the cervix uteri. It was drawn soon after the inspection of a case that was under the care of the reporter. The lady, about forty years of age, was the mother of several children, of whom the youngest was between three and four years old.

She had long complained of weakness, pain, and dragging sensation in the back and loins, a bearing down at the hypogaster, and a leucorrhœa, which she supposed to be the whole malady. Her fastidious delicacy induced her, for many months, to conceal her uneasiness; nor did she at last apply for counsel, until her health was very much reduced, and her appearance greatly changed. She had acquired a deep sallow tint of the skin; had anorexia; lowness of spirits; intestinal torpor; slight dysuria, and a constant fluor albus.

It was not until after presenting clearly to her comprehension a statement of the motives for it, that she reluctantly consented to allow a metroscopic examination to be instituted; and it manifestly appears that none other could have possibly revealed the real nature of her position and its danger.

The os tincæ of a healthy woman barely suffers the passage of a small female catheter into the canal of the cervix. In this case, the ends of two fingers could be introduced nearly half an inch. The

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cervix, of course, must be much enlarged to make so great an aperture. But the drawing, which is believed to be as correct as any daguerreotype could be, will show better than words the appearance it presented.

Plate 5, which may be compared with Plate 1, will show how enormously this chirurgical neck and this os uteri exceed in magnitude the one we have presented as a sample of the ideal or standard uterus.

The cervix, although so greatly enlarged, and as red as the tint in the drawing, was not exceedingly sensitive. Its resistance was neither too hard nor too soft, as is the case in the hypertrophies. The os had the shape or form here expressed. It was enlarged, but not deformed. Had it been deformed, that circumstance alone would decide as to its being not hypertrophy; because, it would prove that the development or growth was non-equable.

The orifice or aperture of the canal was occupied with a viscid transparent slime, which was the albuminous product of sur-excited muciparous glands and follicles, which abundantly line the canal of the cervix. Its appearance is pretty well represented in the drawing.

Inspecting the interior of the canal of the neck, as far as it could be observed, the red tint seen on the lips gradually passed into the dark, almost black hue, which, in the figure, approaches the colour of melanosed tissues, and it was of so black a colour as to lead the reporter at first to fear that it was due to some process of sloughing. He had never, in any other case, noticed anything similar.

This case was treated, by the reporter, by means of antiphlogistic contacts of the nitrate of silver pencil. The number of those contacts, which were reiterated with intervals of from five to seven days, did not exceed six or eight, and as the inflammation vanished before the therapeutical power of the remedy, the womb returned rapidly to its form and dimensions; just as it would do, if, being enlarged by gestation, it should be set at liberty, by an abortion, to return to its non-gravid condition. We shall shortly explain the meaning and intention of the phraseology we have employed, as to antiphlogistic applications of the argent. nitratum.

The ideal womb is two and a quarter inches long. This specimen was so much augmented in size that, while the os tincæ was low down in the excavation, the fundus was distinctly felt above the

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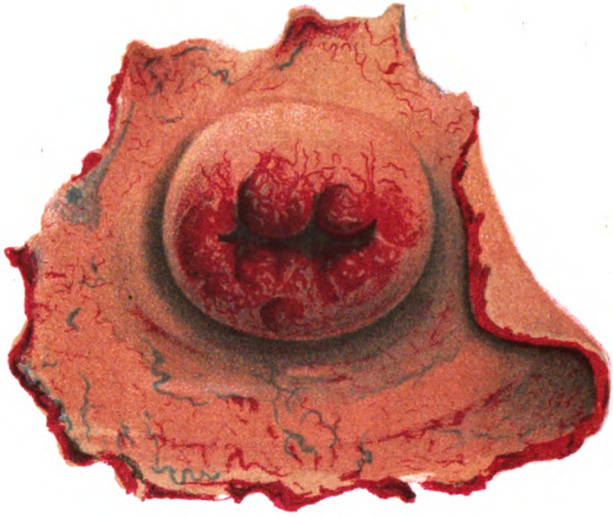

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plane of the superior strait, being at least one inch higher than that.

It might, perhaps, be a desirable thing to know, if possible, why it happens that, in certain cases of inflamed cervix, the surface of the lips of the womb should be smooth or even, as is seen in the present specimen; while, in others, the inflamed superficies become uneven, or tuberculated, or drusy. To show the difference here alluded to, the Committee beg to ask attention to the annexed figure (Plate 6), which exhibits the uneven surface in question.

This drawing is also copied from nature, and represents an appearance very commonly met with in practice. The chirurgical neck is manifestly enlarged, which does not imply thereby swelling of the texture, but rather, a condition of hypertrophic growth or development. Here, as in almost all the examples, the hypertrophy proceeds from the provocation contained in a positively inflamed state of the corpus mucosum uteri.

Such an inflammation is virtually an augmented vital force, which stimulates the whole organ, and compels it to obey its natural law, which is that it shall wax or increase in substance, equably under certain stimulation. This is the power which enables it to go through the vast but equable mutations of its form and substance in gestation.

One might well deem, from inspection of such a specimen, that the vital status of the corpus mucosum must be dangerously exalted. It exhibits many tubercular elevations, which, however, are soft and velvety to the touch. There was no ulceration; on the contrary, the entire surface was covered with its pavimented epithelium, which was so delicate as easily to be broken by a sponge, or by imprudent, awkward manipulation with the tube. Madame Boivin seems to have conceived the idea that a tuberculated surface like this may possibly be the expression of a fresh or inchoate stage of cauliflower degeneration, and the thought is worthy of attention.

The patient from whom this drawing was taken, was rapidly cured, the irregularities of the surface subsiding to the ordinary smooth level, and all the excess of magnitude, sensibility, and colour of the parts, being taken away, chiefly by contacts of the nitrate crayon—in which surely resides a true antiphlogistic therapeutical force, as we shall hereafter endeavour to show and explain at greater length.

Out of a considerable number of drawings, faithfully representing this appearance of disease in the subject, the Committee have selected these two as sufficient to explain or illustrate the physical lesions the practitioner may expect to meet with; not wishing to load

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their report with a useless array of specimens. They will, however, add one more of this class (Plate 7), taken from nature, in a patient who had, for a long time, and in vain, been subjected to treatment by nitrate of silver cauterizations. It also presented the appearance, like that shown in Plate 5, of a collection of albuminous mucus in the orifice. As here, so it often happens to observe this mucus-production to be greatly augmented; whereas, in other samples closely resembling it, in other patients, this peculiar sign is either not seen at all, or in indifferent quantity.

It may well be supposed that variable degrees of vital excitement in the muciparous apparatus, might, for one individual, or now, cause excessive production, while at another time, or in another case, it should give rise to no excess in the albuminous production, and thus we may justly infer that, this albuminous discharge is not invariably present as a pathognomonic characteristic; but on the contrary, the inflammation is the thing to be considered, treated, and cured, if we would really restore the patient's health. When the albuminous discharge is seen, it is, however, always pathognomonic of the cervical inflammation.

The Committee respectfully submit that, these illustrations are sufficient to confirm their assertion, precedently made, that diagnostication by M. Récamier's method is indispensable for the correct information of the medical attendant. Not only do they show how absolute such a diagnosis may be; but they must convince that diagnosis by touching alone, could not convey to the mind so concise a notion of the surgical and medical wants of the sufferer; while they, further, manifest the facility, convenience, and exactitude, with which surgical dressings and other treatment may be made by the metroscopic method.

Whosoever should examine these illustrations might be expected confidently to infer that so considerable an inflammation as either of them represents, would be likely, not only to give rise to the constitutional disorders attendant upon many examples of leucorrhœa; but he would understand why, along with the albuminous fluor, there might arise an excessive vaginal secretion, which, nevertheless, would be but one of the accidents of the principal case.

Daily experience convinces that multitudes of sick women are treated by professional and other persons, for affections like those here portrayed (without the least inkling of the truth), with vaginal injections of various astringent and other solutions and infusions,

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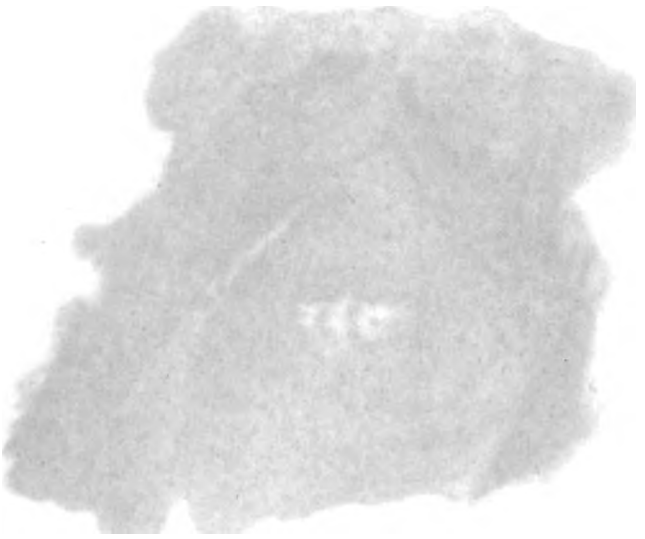

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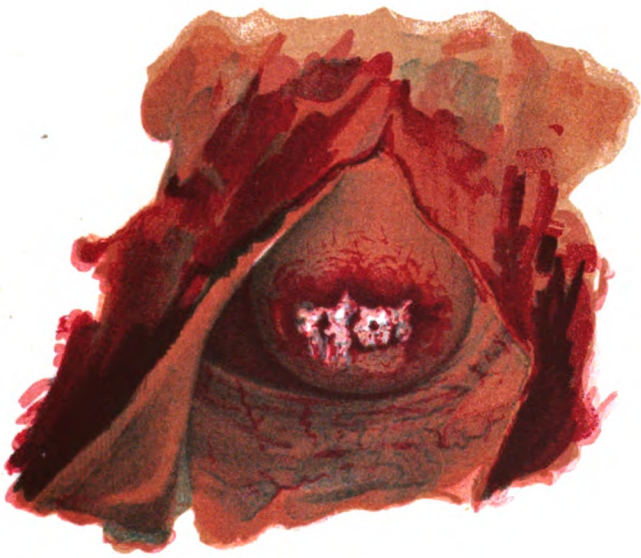
Plate 7



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which rarely produce any good effect upon the health; though they serve, in many, to add to the mischief.

It would certainly be a considerable step in the progress of clinical medicine, were it possible, to clear up these obscurities, and banish from the art the mere senilities which so much discredit it; but this can only be done by means of perfect diagnostications.

Physicians, in order that they may make proper ministrations require—not belief, but knowledge of their cases. Where perfect knowledge of the case can be obtained, there will be the most perfect administration; there will be at least, the meliorations; and, in so far as remedies can succeed, there will be the most successful results or cures.

A medical man, ordering injections for such disorders as are here described, will assuredly be baffled, since no such injections can be supposed to pass beyond the limits of the vagina itself. They cannot penetrate into the canal of the cervix. They bedew only the mammillary parts of the uterus; while they bathe the entire of the vaginal walls. It is the custom of many practitioners, to order vaginal injections, composed of solutions of argent. nitrat. of various strength. Few of them, we are persuaded, can say they have found much advantage to the patients from this operation. If the Committee are to be justified in asserting that the major part of these cases consist, essentially, in disease of the cervix itself, and not of the vagina, which is only secondarily or symptomatically involved in the disorder; then such a practice is either useless or pernicious. If such a solution is strong enough it is too strong; and if it be too dilute it is indifferent in the treatment, or merely nugatory.

The patient requires a remedy for the raspberry-coloured inflammation of the neck—one fitted to bring to a close a train of perverted and exalted vital force of an organ, whose disturbance proves to be one of the most considerable disturbers of the constitutional health. The cure of the real disease, the radiating point of disturbance, cures the leucorrhœa, as well as all the balance of the perversions.

But, it is proper for us to exhibit other appearances assumed by the inflamed cervix uteri, lest a false direction be given to opinions upon such special cases as may come up in one's practice. We have already exhibited several modifications of the cervical surfaces observed in different specimens, and we here call attention to one represented in the annexed figure (Plate 8, fig. 1), one that is not quite

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so common as the *inflammation framboisée*, yet so common, as to be often met with in practice.

The appearances here exhibited (Plate 8, fig. 1) were those observed in the case of a lady from a distant city, who was addressed for treatment, to the reporter, by a distinguished member of this Association. She had been for a considerable time under his own medical care, after having undergone a great variety of surgical and medical treatment by others, but without any useful effect.

She complained of pain in the interior of the pelvis; disordered menstruation; which was both menorrhagic and painful; as well as irregular in its returns. She had dysuria; pain in the middle of the sacrum, and tenesmus: she was excessively hydræmical and desponding; and had capricious appetite; while acidity and torpid bowels accompanied the affections above enumerated, and, to crown all the rest, she was frequently attacked with what might be called crispations, or rather spasms and convulsions, simulating closely certain epileptic forms. The nervous system was in a state of such sur excitement, that a word, a look, an emotion, or any motion of the limbs, often served to render her quite insensible from an attack resembling a slight form of eclampsia puerperalis.

She had borne three children, of whom she had, within a year, lost two by scarlet fever; and that event, by the moral shock it occasioned, had greatly aggravated all her pre-existing complaints.

The womb was retroverted; the fundus resting in the Douglass culde-sac, while the os was near to the symphysis pubis.

Upon exposing the chirurgical neck to a beam of daylight, by means of the Récamier tube, the os proved to be inflamed, with the margin of the anterior lip tinted red, just as is here shown; while the lower lip was covered by a disk of inflamed corpus-mucosum, of a square shape; the whole of this square patch was evidently a raised surface. One might almost venture to call it a *molluscum* on the cervix. It is frequently met with in clinical medicine; and we think always exhibits this peculiar character. Its frequency, in practice, we repeat, is considerable.

In reasoning upon such a case, it would seem superfluous to look beyond facts, like these as here stated, for a rationale of the constitutional and local phenomena. The healthiest young married woman is liable, almost immediately after conception, to be affected by those disturbing radiations of the uterine or hysteric force that are developed by the new vital states of pregnancy, and she may be seized with distressing nausea, or frequent vomiting, or profuse salivation, with

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Fig 1

Fig. 2.

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anorexia, hydræmia, hysterical passion, and a thousand perversions, both of the reason and the senses, as well as the organic functions.

The hysteric malady, with its incoherent train of vital manifestations, may arise out of a basis of reproductive irritation, even less than that of incipient gestation; we really are not called upon, then, to look farther for causes of the constitutional symptoms of our case than to those morbid states of the womb that are here set forth in the figure 9.

As to the treatment, the fundus uteri was lifted up out of its false position, the cervix was thrust back to its place near the sacrum (see the ideal standard (Plate 3), and retained in it, by means of an annular pessary, whose sacral segment rested in the posterior vaginal culde-sac, and its pubal segment against the pubis. Nothing could now retrovert it again.

To take the womb thus out of its dislocated position would, alone, go far towards insuring its recovery; and it will be presently contended that, many of the uterine diseases, even great hypertrophies, require no farther treatment; but being thus delivered over again under dominion of their natural or generical laws, they hasten to recover their true generic substance and form, as after parturition. In this instance, however, the disk of inflamed tissue was treated by antiphlogistic contacts of argent. nitrat., by which it was cured; and thus provision was made for the early removal of another cause of the constitutional disorders, besides the displacement.

Having already spoken of contacts of the nitrate of silver as antiphlogistic contacts, it seems proper now to explain our meaning in the use of that phraseology.

It is undeniable that a pencil of nitrate of silver applied to a soft, moist, living tissue, and held long in contact with it, will disorganize the tissue, and so prove to be a destructive contact. It is equally undeniable that a contact may be effected with such rapidity and lightness as to prove ineffective or indifferent, while there is another mode or force which does resolve inflammation with great certainty; and this is the antiphlogistic contact above spoken of. We therefore feel warranted to speak of such use of the nitrate of silver as being either destructive, or indifferent, or antiphlogistic contacts; and experience confirms the propriety of the classification; for, we meet with numerous examples of treatment that conclusively prove it is not the mere treatment by escharotics that is so successful, but the use of them in such a way as to provide for their due operation as antiphlogistics and not as destructives. Certain women, who are in vain treated for these cervix inflammations for months in succession

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by contacts of nitrate of silver, recover their health very speedily upon a few such touches, lightly made—that is, made with due regard to the resolvent or antiphlogistic power of the drug. It is not enough, therefore, in studying this subject, for the practitioner to resolve upon the treatment by a method of escharotics or cauterizations, as it is erroneously expressed. Far from it—what he requires is, to create for himself an IDEAL of his operation, so that, when about to perform it, he may predetermine what it is he hath to do, and whether the contact he is going to make shall be a destructive or an antiphlogistic one. It might equally well be called a resolvent one. He who fails to set up before him this IDEAL of his duty and purpose will be apt to fail in the cure, or his cure will be a chance-medley, and not the product of a rightly reasoned purpose and a prudent act.

This absence of precision in the design and act frequently occasions the greatest and most dangerous aggravation, and the most poignant sufferings—which, in our estimation, is wicked and abominable. They are disgraceful to the Art and the artist at once.

Believing these remarks to be just and clinically important, it is desirable they should arrest the attention of every practitioner in this line.

After the foregoing observations, we will now aver that by means of antiphlogistic contacts of the nitrate crayon it is possible to cure, and that speedily, most of the inflammations, and their accidents met with in this humble department of clinical medicine or surgery; for it is to be remembered that few of them are attended with any, the least degree of ulceration, as we have before asserted.

It is a common opinion, and it is generally agreed to say so, that these are ulcers on the womb; and there are people who seem never to fail to discover an ulcer upon making an examination with the speculum matricis. An immense experience in a populous metropolis—an experience greatly increased by the resort of numerous invalids from the country, and from the different United States, enables us, with confidence, to declare that, an ulceration of the womb is among the rarest of disorders. We repeat the expression of our opinion, that these disorders and *framboisée* inflammations and hypertrophies of the cervix, have been misinterpreted and accounted as ulcerations, which they were not, the superficies being covered with a delicate epithelium, yet so very delicate as readily to give way and suffer abrasion under improvident unskilful manipulation with the tube or the sponge.

A proper antiphlogistic and resolvent contact of the crayon ought

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not to destroy even this delicate epithelium; but rather to make it more firm and dense, and so, planish as it were, the unevenness down to the normal surface level. In this way, we may compel the drusy or tubercular eminences to sink down again to their place, and give, by solidifying the epithelium, a firm physical delimitary support to the before debilitated capillaries that rise up in the form of a soft molluscum.

Our patient was thus treated. She was also advised to take abundantly a nutritious diet, with a copious allowance of Bordeaux wine and water; to swallow, soon after each daily meal, two grains of iron revived by hydrogen, and made into a pill with clarified honey; to get into the open air, on foot, as much and as long as possible, and, being a person inclined to follow the instructions, she soon recovered a good state of health.

As incidental to this part of our explanations, it may be proper to remark that, the state of the canal of the cervix so often referred to, is, in all probability, one of the ordinary causes of sterility. In examining these structures after death, we have observed the whole cylinder of the canal of the cervix, to be filled or tamponed, so to speak, with a plug of viscid lymph, so obstructing the passage as to render it apparently impossible that any spermzoon could obtain access to the uterine cavity. Certain it is, that some sterile women are always affected with this excessive albuminous mucous production. We have met with instances of unrelieved sterility in women enjoying the most robust health, with the sole exception of this vexation, which never gave any pain, nor modified the mensual phenomena in the least. Many women, who had temporarily suspended the usual succession of their gestations, apparently in consequence of this derangement of the health, have again conceived after the cure of the albuminous leucorrhœa; or rather, the inflammation of which it was the sign and consequence.

Surprise has often been expressed on observing that married women, after years of sterile cohabitation, have suddenly become fruitful. In these instances, the want of fruitfulness could not depend on failure of the ovulations; may it not be that the spontaneous cure of a protracted and subacute inflammation of the kind herein treated of may have restored the health, and so given power to take away the woman's reproach?

Continuing the plan adopted in this report, we now observe that other forms of cervical inflammation, than those already described, will present themselves to the observation of the clinical practitioner;

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and we submit the accompanying drawing, taken from the life, in a case which proved unexpectedly rebellious to treatment, though it was subdued at last.

This person, a lady of small stature and delicately formed, of a sanguine choleric temperament, had given birth in rather too rapid succession to six living children, and experienced, in the third lying-in, a very dangerous attack of childbed fever; and, a few years later, subsequently to the birth of her sixth child, suffered from a severe crural phlebitis, or milk-leg. The health had been less firm than usual after her recovery from the phlebitis, although she was not afterwards, on account of her valetudinary state, confined to the house.

After, for a long time, patiently enduring severe pains, referred, in chief, to points in the neighborhood of Poupart's ligament of the left side, with distressing sensations in the range of the external obturator nerve, bearing-down feeling, backache, debility, loss of appetite, irregular action of the bowels, and dysmenorrhœal symptoms, she asked for advice. There was inconsiderable fluor albus, no positive dysury, nor other sign of uterine deviation or displacement. Exercise on foot invariably increased the pain, and the jarring of carriage-springs was highly annoying. She looked dispirited and wan.

Touching the chirurgical neck, it was found to be excessively solid, with the posterior lip not a little longer than its fellow. The touch gave, from the resistance, the idea of scirrhous induration; and was so painful, even on slight pressure, as to be very unwillingly borne, and then, not without some exclamations. The left angle of the os was sulcated—as if it had recovered imperfectly of some foregoing laceration in labour. Pain excited by this pressure was felt not only at the point of contact, but severely in the groin and inner side of the thigh.

The lady submitted, under advice, to an examination by the metroscope, and it disclosed the form, size, and hue, portrayed in the figure (Plate 8, fig. 2). A long familiarity with maladies of this class, did not prevent Reporter from having repeated misgivings as to the result of this attack; which again and again seemed to take upon it so many threatening characteristics of carcinoma, and resisted so obstinately the most careful, reasoned, and persevering treatment, that, even now, he is led to doubt if the case was not strongly tinctured with the malignant nature in question.

It was treated with antiphlogistic contacts of the nitrate crayon,

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with emollient injections of flaxseed mucilage, with anodyne enemata, containing, each, forty drops of tinct. opii, at bedtime, with repeated application of Swedish leeches to the cervix; with a regulated diet, occasional baths, a soluble state of the bowels; much rest on the sofa, and the use of hydriodate of potassa, combined with comp. syr. sarsa.

Of several hundred cases of inflamed cervix (at various times, for many years) under treatment, this appeared to be the most irresolvable. Yet it began at last to give way, and slowly yielded—so completely, that the patient was advised to take a course of the waters at Saratoga; from whence she returned in good health, after an absence of a few weeks. Since that time she has passed through a healthy gestation, and has given birth to a robust infant with the easiest of all her labours. During the dilating processes of the labour, the whole cervix was found remarkably healthful. The recovery was fortunate; and she has very good reason to rejoice that M. Récamier has taught the profession not only how to make a physical diagnosis, but also how to treat these dreadful cases. We repeat here, that an inspection of our drawing will communicate an idea of the physical appearances, wellnigh as correct as the direct observation of the inflamed cervix itself. It cannot well be believed that such a form of inflammation as this could be made amenable to a treatment by mere vaginal injection, by alterative doses of mass hydrarg., by preparations of iodine and tartar of antimony, or any constitutional medication. It is true that, in some instances, the topical applications may have been either indifferent, or positively injurious; and it must ever happen, that even where one forms in his mind a correct ideal of the therapeutical purpose and act, he may err in the realization of it. It can only be said, that the treatment rarely gave rise to other pain than what was caused, sometimes, by pressure of the instrument. But every sensible pressure with the index finger gave as much. Be this as it may, the ultimate final resolution of a detestable inflamed induration of the cervix, and her complete recovery, furnish proof, that the method (if not a safe, or a desirable one) is, at least, in some very unpromising instances, crowned with the happiest success.

The Committee having already set forth, in a preceding part of this report, their views in relation to hypertrophied states of the womb, and the constitutional proneness of that organ to undergo hypertrophic development, they will now beg to call attention to some

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of the consequences of uterine irritation, that differ, at least in form, from those already adverted to, and illustrated.

Women, who complain of intrapelvic pain, and other disorders of the lower part of the body, are inclined, in general, to account for these incommodities by referring them to, what they call, womb-complaints. This term is so vague and imprecise, that the pronouncing of it rarely excites any clear precise idea. Among these cases of womb-complaints, probably none are so common as those which depend upon retroversion of the womb. So frequently does this affection occur in the course of one's medical practice, that one almost acquires a tendency in every case, before examination, to suspect it has some dependency upon *retroversio uteri*. Reporter has long been fully convinced that retroversion of the womb constitutes seventy-five per cent. of all cases of sexual disorders, of a gravity sufficient to require appeal to medical advice. This may, at first, appear to be an exaggerated estimate; but it will probably be found sustained by statistical experience.

Although the Committee, appointed to report on Acute and Chronic Diseases of the Neck of the Womb, have respectfully considered what are the limits assigned to them, they cannot but suppose that these cases of retroversion do so far interest the state of the cervix, as to bring them, also, within the scope of their inquiries.

An inspection of Plate 3 shows how it must happen, that alternating states of fulness or voidness of the urinary bladder must interest the uterus, as to its place. An overfilled bladder of urine, by thrusting the fundus backwards towards the sacrum, puts violently on the stretch the ligamenta rotunda; and, as the uterus is, to a certain extent, rigid and inflexible, it follows, that if the fundus is thrust back so as to stretch the round ligaments, the cervix must come forward, straining at the same time the utero-sacral folds—for the womb moves by the way of see-saw—in being retroverted.

A great many women and young girls suffer themselves to acquire a bad habit of retaining the urine until a large quantity is accumulated within the bladder. Thirty ounces of liquid, in the urinary bladder, makes a mass as big as a quart measure, and it cannot ut thrust the uterus injuriously backwards, causing the neck to see-saw at the same time forward, and approach the pubis.

A long habit of this kind comes at last to ruin the uterine ligaments; so that the fundus, being wholly overset backwards, sinks down into the Douglass cul-de-sac, while the os takes a permanent place near the symphysis pubis; and this is retroversion. It is even

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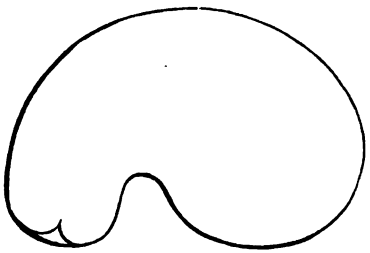
sometimes a congenital malposition, as shown by Morgagni and others.

But, let it be remembered that the womb is constitutionally prone to set off on a race of hypertrophic development, and we may understand how it shall be readily provoked to commence a process of hypertrophization, by the awkward, unnatural, and irritating posture it acquires in retroversion.

We have met with some of these cases in which the womb grew so much that the fundus was jammed against the hollow of the sacrum, and the os against the symphysis, or on the soft parts above it, compressing the bladder most mischievously, but not at all bending the womb itself.

In other specimens, we find the uterus bent like a retort, as in the outline figure annexed. The angulation, in some of the examples, is



almost acute; so that the caliber of the canal of the neck is seriously diminished, and its function as an efferent duct for the menstrua, in a measure hindered; whence we have distressing dysmenorrhœa.

Medical men, called upon to diagnosticate in such cases, should be very careful, first to establish in their minds the ideal, or standard, by which to compare; otherwise they will be apt, upon finding the whole pelvis occupied, or blocked up with an immense and immovable solid mass, to conceive of it as a tumour; whereas it may prove in fact to be no tumour, but only an overset and enormously hypertrophied uterus.

We have met with repeated examples of such misinterpretation of the case, women being supposed to labour under tumours within the pelvis, whereas the supposed tumours were nothing more than the posterior aspect of the corpus and fundus uteri, turned over and touched through the posterior membranous wall of the vagina; and wholly disappearing as soon as the womb could be properly reposited.

A young lady was brought to Reporter from a distant State, being

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accompanied by her attending physician. In this case, a drawing was made, after the most careful examination, of which we subjoin a copy—Plate 9.

The figure is one-fourth less than the natural size, and is made to exhibit a cross section of the pelvis conducted through the pubis and sacrum, with the fifth lumbar vertebra. In making this exploration, it was easy to verify the diagnostic by hypogastric palpation, and by examination conducted both by the rectum and the vagina. The cervix was bent, as in the figure, and the body of the uterus enlarged and turned over deep into the recto-vaginal cul-de-sac. The entire mass was so packed and immovably fixed in the excavation as to make it impossible for one to raise it by any degree of upward pressure with the fingers, and the canal was too much curved to admit of correcting the deviation by using Professor Simpson's sound. In general respects, the patient's health, being not very greatly affected, she was advised to return to her home and undergo attempts to reposit the organ by the use of caoutchouc bottles filled with curled hair, a method proposed by M. Hervez de Chegoin. The result has not been communicated to us, farther than to inform us that the lady's health is improved.

We have stated this case, because the diagram was made after due reflection, at the time of our consultation, and because we rely upon its accuracy as a representation of not infrequent cases of disorders of the cervical portion of the uterus. Certainly, we have met with a considerable number of analogous forms of disordered womb for many years past.

To reposit the uterus, and maintain it so, would appear to be the chief indication in such a case; since that alone would be to place it in a condition to obey again its normal or generical law of form and substance. We repeat that a considerable number of instances, in which the womb was greatly augmented in every dimension, in consequence of the irritation superinduced by retroversion, have been treated with the happiest success by Reporter, on this principle.

To show how great is the change wrought in the hypertrophied uterus by such reposition, we annex two figures (Plate 10, figs. 1 and 2).

These drawings represent one and the same uterus, Fig. 1, being a view of it when the hypertrophy was at the highest stage; and Fig. 2, when it had entirely disappeared.

The patient was a woman 39 or 40 years of age; her children were all grown up, except the youngest, a daughter, some 13 or 14 years old.

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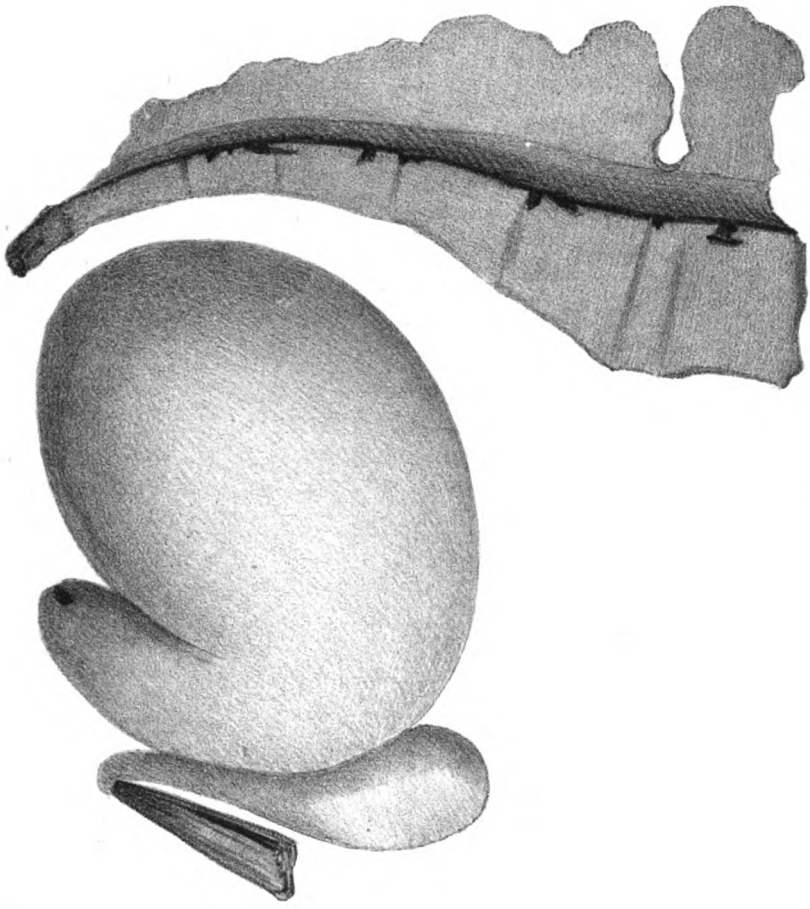

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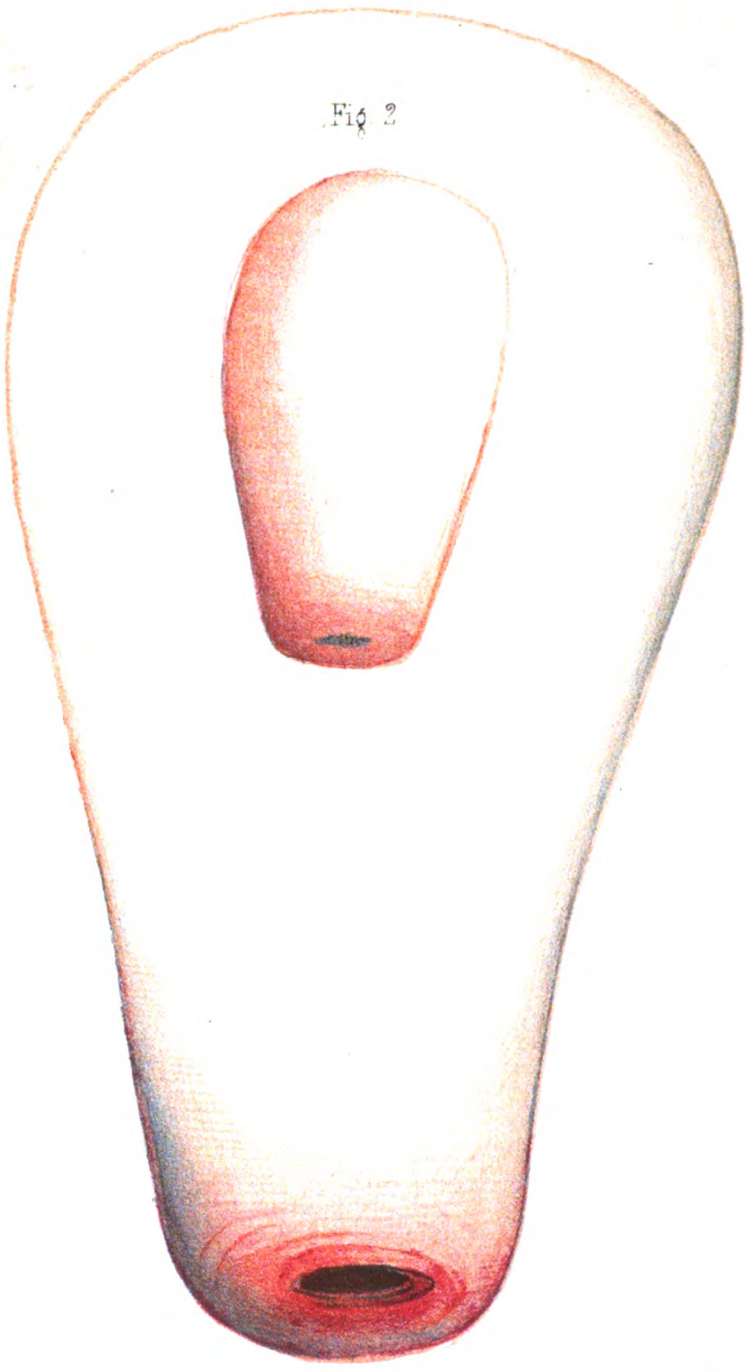
Fig. 1.

Plate 10.

Fig. 2



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When first called to the case, the belly was tumid, and to a considerable degree tense and sonorous on percussion, but, with the integuments, so strictly drawn over the abdominal contents as to prevent any satisfactory conclusion concerning the state and nature of the substance giving rise to the so great distension.

There was dysury and pelvic fulness, with tenesmus; the menstrua absent, causing some suspicion, on her part, of existing gestation.

Touching disclosed a retroversion. The pelvic cavity was so full as to seem packed. The os uteri was higher than the top of the symphysis pubis, and permanently there. The substance of the womb could be traced down and backwards to the hollow of the sacrum.

This womb was reposited, completely, and with considerable relief. Subsequently, after severe catharsis, it became possible to trace its outline in the belly, and Professor Simpson's womb-sound was used, in order to clear up the diagnosis. Reporter slowly and cautiously passed the sound into the os uteri, and it advanced without giving pain or encountering any obstruction, until the probe-point of it was arrested by coming in contact with the vault of the fundus. The sound had gone six and a half inches into the cavity, which proves that the Fig. 1, Plate 10, is correct as to its length. We are answerable that the volume of the chirurgical neck and the orifice are also rightly illustrated, while the transverse diameter is as near the truth as we could make it by the most careful measurement, spanning it with the thumb and fingers through the abdominal walls, relaxed after the cathartic operations.

The rectification of the position did not, however, save the life of the unfortunate patient, who proved to be also affected with a colloid degeneration of the ovary. This, which became an enormous mass of disease, spread its ravages far and wide within the peritoneal cavity, and she died at the end of six months from the time here referred to, after repeated paracentesis abdominis. Upon making a *post-mortem* examination, the uterus was found to be of the size and form represented upon the Fig. 2, Plate 10.

The case appears to us to present points of great interest, since it shows that a vast increase of the volume of the womb, unconnected with conception and gestation, is no more inconsistent with recovery of the non-gravid form and size than a similar recovery after healthy pregnancy. The measurement, made with Simpson's sound, and the opportunity to determine the appearances of the organ, after the death of the lady from colloid cancer, rendered it an extremely favourable example for illustrating the views set forth in this report on the

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subject of uterine hypertrophy. It ought not to be forgotten that this womb admitted of the introduction of six and a half inches of Simpson's womb-sound soon after it was relieved of its retroversion, and that the lady perished, some six months later, with degeneration of the ovary and other parts. If, therefore, the hypertrophy of the womb had had any dependency upon the ovarian disease, we should not expect to find it reduced back to its normal size; whence we infer that, to relieve it from its dislocated position, was the very thing necessary for its cure.

Note.—Since writing the above, we have concluded to present a drawing (Plate 13, fig. 1), exhibiting the appearance of a case of hypertrophy of the cervix uteri, which came under care of reporter. It was the case of a maiden lady of some 85 years of age. It is believed that this picture may serve to communicate a correct notion of the physical characteristics of the chirurgical neck in the instance referred to. This patient recovered under resolvent contacts of the nitrate crayon, and the exhibition of internal remedies of an alterative kind. Probably such considerable augmentation as this of the cylindrical portion of the womb, yet not involving a state of general hypertrophy of the organ, will not be very often met with in practice. Still, cases of the kind are of sufficient frequency to give interest to the description of such a great change in the volume of the neck of the womb. In this instance, and in others much resembling it, we have not been accustomed to detect signs of positive inflammation in the part. Nevertheless, it would be reasonable, from the resolving or deobstruent power of the nitrate contacts in positively inflamed cervix, to infer a similar availability in mere hypertrophic irritation; and practical experience justifies such an inference.

Another drawing (Plate 11), herewith presented, shows the appearance of a cervix and os examined by the reporter. The patient was a resident of a neighbouring State, who, some months previous to her visit to Philadelphia, was visited, at her residence, in consultation. The womb, at that time, was completely retroverted, and so much enlarged as to fill the excavation, pretty much as it is filled by the presenting part in a labour. It was, at the time, immovable by the hand, but was afterwards slowly raised by means of the caoutchouc bags or pessaries of Hervez: vid. *Trans. Acad., Roy. de Med.*, tom. II. p. 319. She was considered to be dangerously ill; but began to recover when the womb was replaced and relieved. After acquiring a tolerable degree of health and strength, she proceeded to Philadelphia, complaining of bearing-down pains and intropelvic distress, with catamenial disorder, attributable to the remaining and uncured disorders of the uterus.

The metroscope showed the os tincæ and cervix as they seem in the figure in which we have represented the organ foreshortened. The unhealthy state of the cervix was attested, not only by its augmented size, the tubercular elevations of the margins of the orifice,

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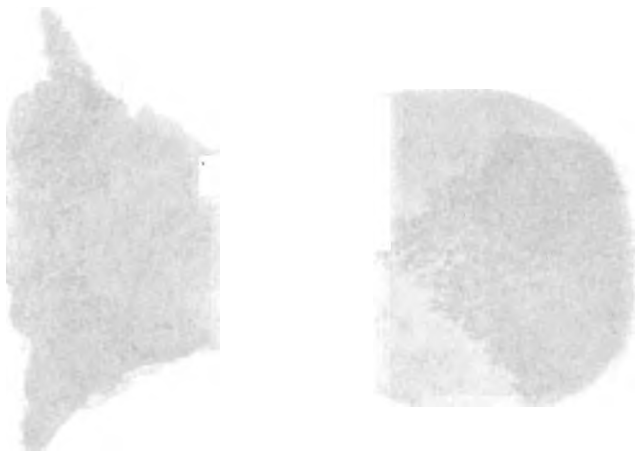

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Fig. 1.

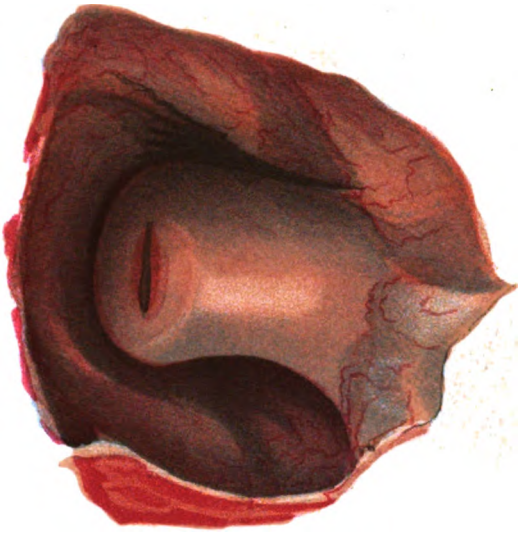
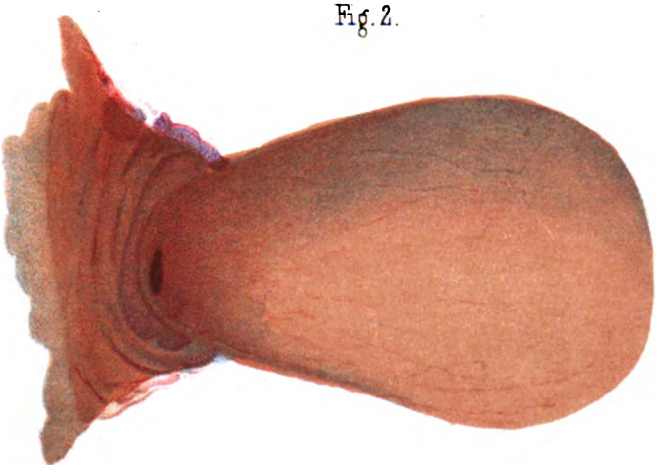


Fig. 2.



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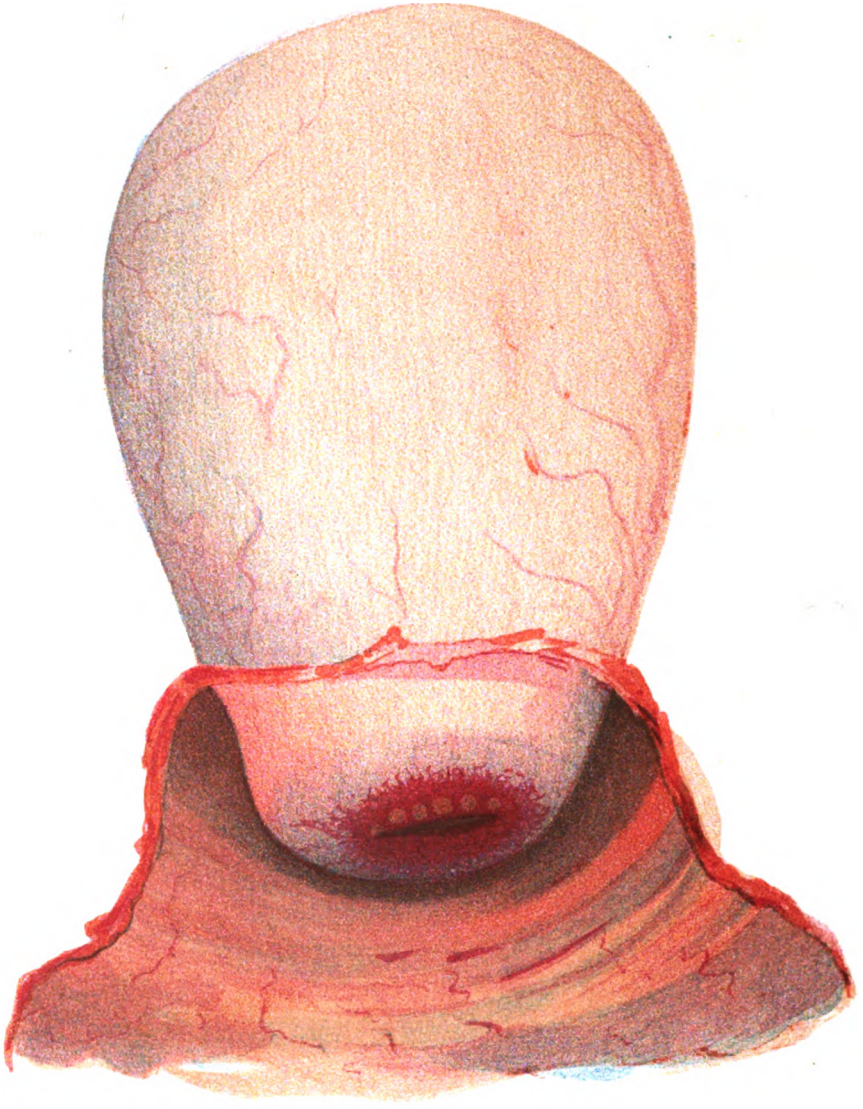

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and the red inflammation, but also by the viscid albuminous dis-

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and the red inflammation, but also by the viscid albuminous discharges from the canal of the neck.

The treatment consisted, mainly, in the use of antiphlogistic impressions made by the nitrate of silver pencil. The inflammation of the mucous body being cured, the disordered womb returned to its accustomed obedience to its generical law of form and substance, and recovered its normal magnitude.

An inspection of this specimen of disorder, remaining after a very great hypertrophy had been reduced, strengthens the inference hereinbefore expressed, that the precise appearances manifested by the os and cervix, in inflammation, are accidental—being either drusy, tuberculated, or smooth and even in surface; and now we present a case of hypertrophy, of which Plate 12 is a just and fair exponent of the appearance, as it was examined by us, both by touching and by the Recamier speculum, as well as by M. Joubert de Lamballe's ivory metroscope.

The os uteri was of this size, and the cervix could not be embraced within the aperture of the Joubert instrument. The neck of the womb, as far as it could be explored by thrusting the fingers upwards all round the neck, in the vaginal cul-de-sac, was of this shape, and flaring out at this rate. The fundus, which was readily detected in the hypogaster, was as high and as ample as is here seen.

The tint of the inflammation of the os tincæ, is carefully reproduced in the drawing.

The patient had been in apparent danger of imminent death a few weeks before this sketch was taken, and from causes connected with this uterine disease. She is the mother of a numerous family.

Some leechings of the chirurgical neck, and repeated applications of nitrate of silver to it, served to cure the cervical inflammation, and the womb returned to the size shown in Plate 13, fig. 2. The patient was soon restored to health, after several years of the greatest inconvenience and many most painful and alarming attacks endured before this treatment began.

No man, much experienced in the treatment of uterine hypertrophies, can have failed to meet with persons in whom, to examine by touching, was to find the pelvis filled up solid, so to speak, with the os uteri close to the pubis. We have met with not a few such instances; and it has happened more than once, that we have been so much disheartened upon a first examination, as to be prompted to make a diagnosis of incurable disease.

In one case, a lady from the distant South, and in the most

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wretched health, had the excavation so filled up with a hypertrophied and solidified mass, that there was scarcely space sufficient to allow an introduction of the index finger betwixt this mass and the floor of the pelvis upon which it rested. The neck and os were near the symphysis. It was a case of complete immobility of the uterus, which seemed as big as a fœtal head in a labour at term. By means of a Para gum-elastic bottle, a very small one, stuffed with mattress-hair, we made an oviform pessary, a little bigger than the thumb. This was forced into the vagina, and retained by an outer compress. In a few days, one somewhat larger was substituted. The elasticity or spring of the caoutchouc bottle steadily pressed the mass upwards, and the size of the balls being occasionally increased, we had the satisfaction to find, after no very protracted service, that the intropelvic tissues had acquired a natural character, and the lady returned to her country in very good health, which she still enjoys.

We might relate a great number of cases to show that upon removing the causes of augmented volume of the womb, it falls speedily into its generical habits of nutrition; but we abstain, considering that the declaration of this doctrine, indeed, with a few illustrations and explanations, ought to suffice to present the matter clearly to the apprehension of any of our intelligent colleagues.

It is, by many, very confidently supposed that the use of escharotics in these cervical inflammations will speedily suffice to restore the health of the patient; and those who are most accustomed to use them, are, perhaps, more thoroughly convinced than any other persons, of their great efficacy. It will not, however, always answer to make the application to the mammillary part of the cervix alone, because the inflammation is, in some of the examples, found to attack with greater or less severity the corpus mucosum and the mucous follicles of the *canalis cervicis*. Here it is proper to apply the nitrate by means of Lallemand's port-caustic, or by means of a fitch pencil, which is composed of hairs so rigid that one can readily pass it, loaded with a solution, some distance up the canal. It would be dangerous to inject such solution with a syringe whose canula should be introduced within the os. But we must meet with cases, at first to all appearance tractable, that will, in the end, be found to foil all our most patient and well-reasoned efforts to cure them.

One makes up a diagnosis upon the means of diagnosis—there is no other way. One cannot, therefore, positively declare what is the state of the tissues that compose the walls of the canal of the neck, and it may, and does happen that those tissues undergo changes which

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Plate 14

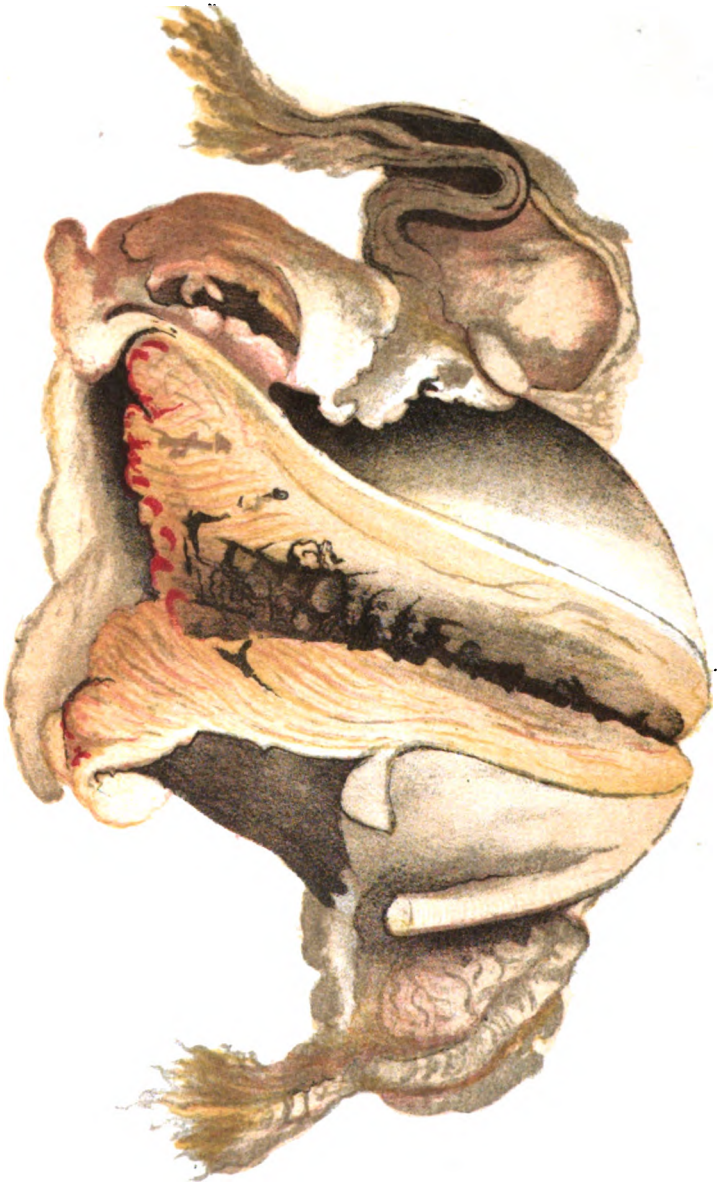

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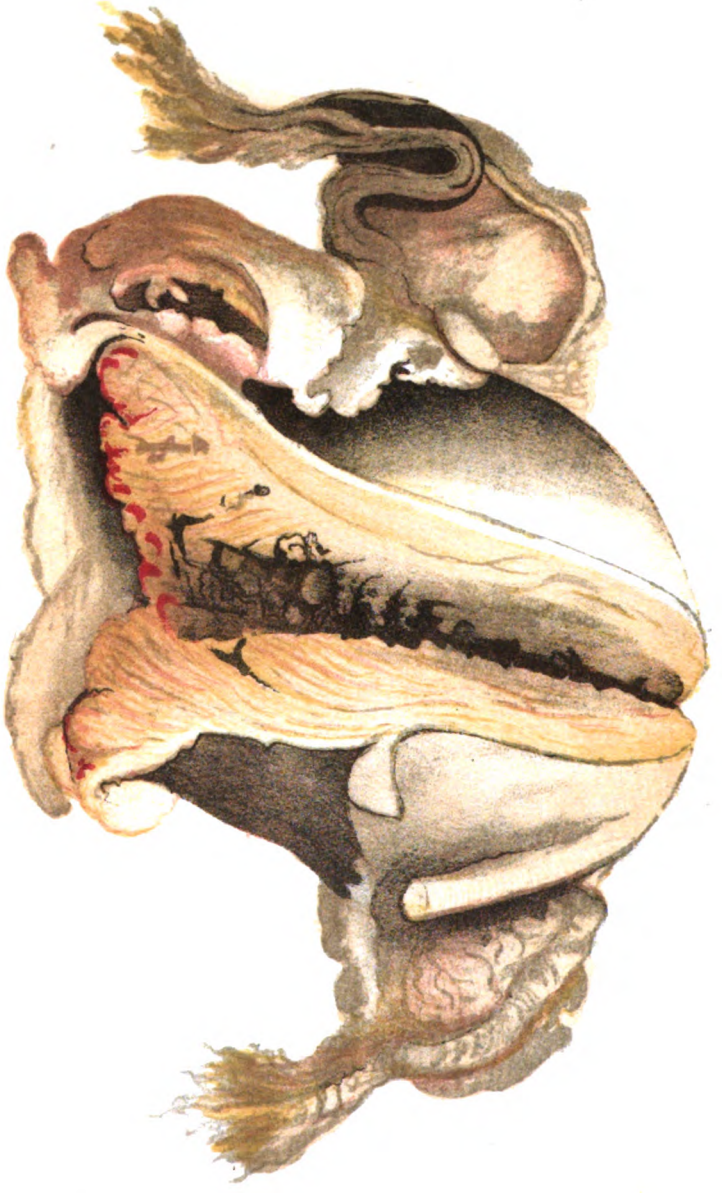
Plate 14



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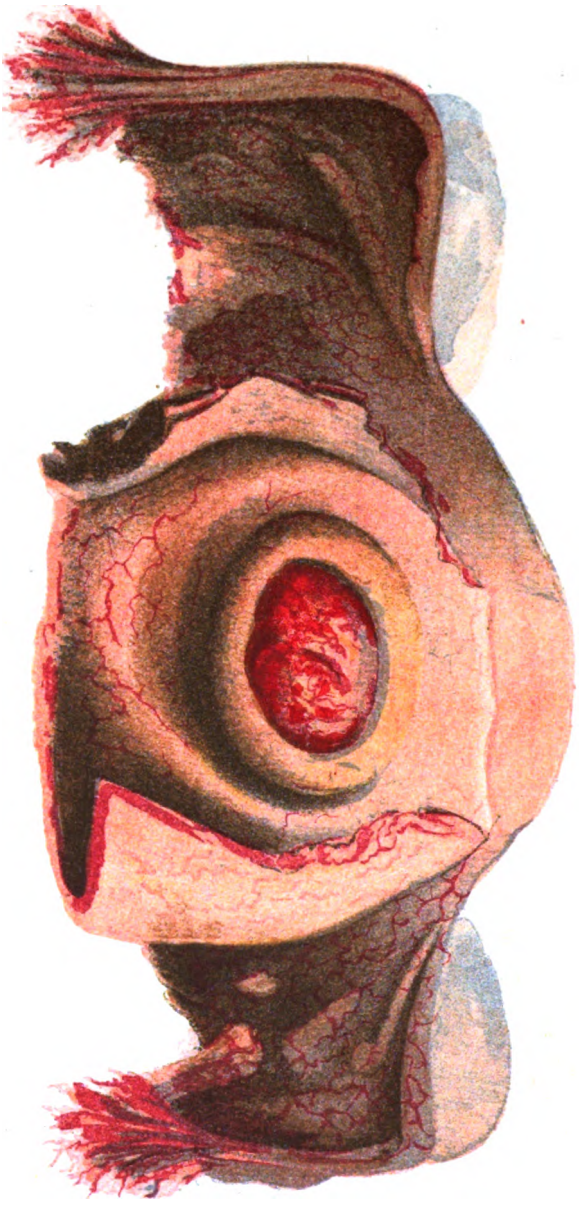

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render a cure by the means herein treated of out of the question. All the parts that come into view might be apparently little changed, while other parts beyond the reach of sight and touch should be in a very different condition. Here is a drawing taken from a specimen in the reporter's collection, that illustrates this point. (Plate 14.)

This figure answers, correctly, as to its scale, to the preparation from which it was drawn; and shows how considerably the womb was enlarged at the time of the woman's death; upon looking at the specimen, after closing the incision, we might be easily misled, so far as to think such a case would, in life, be amenable to treatment; but on cutting it open by an incision from the fundus down to the cavity and canal, and to the os, it is seen that the interior is irremediably diseased, the substance of the walls being converted into a sort of pennicillated structure, of which, the ends of the pencils exhibit small tubercular elevations all along the inner wall.

A casual examination by touching, or even a careful metroscopy could not be expected to clear up such a diagnosis as this one; and all attempts to restore a healthful crasis and form to such degenerated tissues, by leeches, by escharotics, either on the outside of the neck or within the canal, must have failed. We know nothing farther of this case—it having came into our possession by the politeness of a stranger. Nevertheless, it is a specimen most useful in the study and minute research so desirable, as to these recondite sexual maladies. We have recently witnessed the death of a fine woman, who perished under this precise form of disorder immensely aggravated.

Changes taking place in parts of the texture of the inner wall of the cervix must be as various as accident could make them. Such a pennicillated degeneration as the above, being one form, it is not difficult to conceive of other modifications as producing either fibrous or cellular polypus, or the more unmanageable forms of Hæmatomatous degeneration, of which we have met with not a few examples in our practice, and we shall now introduce into our Report the Figure (Plate 15) representing a case which was for a considerable length of time under treatment without any useful influence being produced therefrom.

The womb was carefully examined, with a view to determine its dimensions and probable weight. It was not more than four inches in its longest diameter. The os was of an oval shape, and the edges or lip was much condensed—to such a degree, indeed, as to communicate, by touching, the idea of scirrhous induration. Projecting very little beyond the plane of the orifice was a hæmatomatous mass, that evidently sprung from the right side of the wall of the canalis cervi-

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cis by a broad origin. This bleeding tumour is well represented in the drawing, in which the chirurgical neck, foreshortened, is seen to look like a bourrelet or ring projecting in the vagina, which is opened.

The hemorrhages, here, were on some occasions very threatening. There was little encouragement to be taken from the suggestion to remove the fungus by strangulation—a step forbidden, also, by the delicate state of the woman's general health.

Without indulging idle hopes of effecting a cure, and with clear understanding that no promises to that effect were given, or any flattering hopes held out, the case was conducted for a few months under very unfavourable circumstances, by means of nitrate cauterizations, destructive as to the hæmatome, but antiphlogistic as to the cervical ring. They, perhaps, cannot be said to have done any good; unless, indeed, we might attribute to them the diminished frequency and force of the hemorrhages, and, perhaps, also some positive reduction in the fungus itself.

Circumstances rendered it inexpedient to continue these attempts at amelioration, which were unattended with pain or any other sanitary inconvenience. The treatment was repeated about once in every seven days, from a date in May, 1852, until the close of autumn. Certainly no aggravation was noticeable up to the final period of these attempts; and the patient went away, supposing herself to be somewhat improved in general health.

As a general conclusion, a physician might venture, in such cases, to pronounce them incurable.

Women often complain of irregular mensual returns, saying, that while the legitimate periods are duly and healthfully observed, they are, in addition, vexed with an occasional show after any considerable exertion. Women are apt to regard every sanguineous discharge from the genitalia as menstrual discharge.

All those who recognize the truth that menstruation is one of the phenomena of ovulation, will at once suspect that such irregular *markings* must arise from some cause other than the physiological hyperæmia of the ovulative act. And, in fact, it is mostly found, upon due inquiry, that the patient has the raspberry-coloured inflammation of the cervix, already described; or she has certain small *vivaces*, or bunches of red cellular and capillary tissue, which are either found peeping out at the plane level of a somewhat patulous os tincæ, or jutting quite forth out of and beyond it, and attached by a delicate peduncle or footstalk.

No one could expect to cure such a disorder as this by any sort of

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above proposed, and with satisfactory results. It seems needless to introduce them here.

NOTE.—Upon second thought, we have, by way of NOTE, introduced the figure (Plate 17), showing the appearance of the excrescences protruding like ripe currants from the *os tincæ*, as mentioned on the opposite page. In this case, under treatment in the spring of 1853, there was hypertrophy of the womb to such degree, as to allow the fundus to be felt quite two and a half inches higher than the plane of the superior strait.

In continuation of these clinical details, we now remark, that the lady from whom the drawing (Plate 16, fig. 2) was taken, complained of menorrhagia gradually increasing in violence, and settling at length into an incessant drainage, under which she became excessively weakened and pallid. She had borne no child for some sixteen years, and, like a majority of women similarly affected, was confident that her trouble arose from what they call change of life. She refused, during some eighteen months, to submit to a physical diagnosis, and became, at length, so dreadfully affected with hydræmia, that her life seemed to be in great danger. She was repeatedly informed that the word change of life, as used in general, conveys no distinct idea, and that her issue of blood must arise from some organic fault.

At length the blood, from continual waste of the solid constituents of it, became so dilute, so hydræmic, that she could not walk across the carpet without bringing on palpitation, nor ascend the stairs but with caution, and always with much difficulty. Seeing that she had no other hope of amendment, she submitted to an examination, whereupon the small pedunculated vascular excrescence, or polypus, was discovered, as shown in Plate 16, fig. 2. It was immediately twisted off. The hemorrhages never returned afterwards, and she has had good health these now many years.

This example and statement suffice to fulfil our purpose in this relation, which is the reason why we detail no other similar instances —which we could do, drawing on the stores of our personal experience in practice.

It is of the utmost importance in the practice of physic and surgery, to make absolutely correct diagnostications, since all our prognosis, as well as treatment, which is contained within the diagnosis depends upon being right in the beginning.

This opinion will be admitted to be just by all those who belong not to the empirical schools of our art, but, on the contrary, adhere to the rationalists in Medicine.

There is a great liability, even among the most experienced men,

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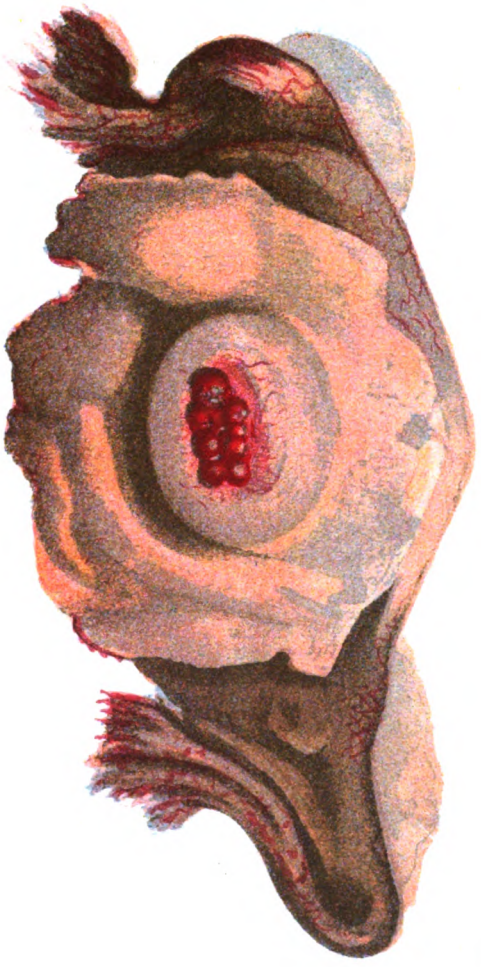

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to be led into ludicrous or even fatal mistakes in the diagnostication of the so-called diseases of the cervix.

A lady came to Philadelphia complaining of uterine disease, that had baffled her physicians at home, and applied for advice to an expert. Upon some consideration, Reporter was invited to attend in consultation, and dissented from the opinion that was entertained by the attendant.

Patient had been affected with some form of violent intro-pelvic inflammation, which, we believe, had been regarded as metritis, by a misapprehension of its seat. After protracted and great suffering, and after several operations with the knife—the precise nature of which we do not know—she was observed to have the vagina so much diminished in length, that her relations of a marital nature became impossible. She came to Philadelphia, as before mentioned. The canal of the vagina was about an inch in depth, and no more. At the bottom of this cul de sac was a firm substance, that was mistaken for the chirurgical neck, and the delicate aperture in it for the os uteri.

To the touch, such a case presents the greatest similitude to the mammillary projection of the womb, and is, therefore, a great stumbling-block in the way of diagnosis. The medical gentleman, whom I met, insisted that it *was* the os uteri, and that adhesion of the vagina to the whole exterior vaginal cervix gave it the present appearance, as at *a* in the diagram on the next page.

Dissenting from this opinion, we averred with equal confidence that the womb could not be touched, nor even approached, and that the so-called os was nothing more than one of the apertures in a long vaginal stricture, and that the womb itself was not at all implicated in the disorder, save in so far as it was imprisoned above this impracticable stricture, amounting nearly to atresia vaginæ. See (*b*) in the diagram.

Some months subsequent to these discussions, the lady returned to Philadelphia, and placed herself under the sole direction of the Reporter—and went away cured.

On the next page we have given an outline drawing, or diagram, which shows how readily one might mistake the thickened bourrelet (*a*) for a vaginal cervix and os uteri; whereas, in truth, the os uteri is above, and in the free part of the vagina marked (*b*).

To look at the diagram, it is easy to perceive the truth as to such a case, while, in practice, the diagnostication had, and has deceived many an experienced and able physician.

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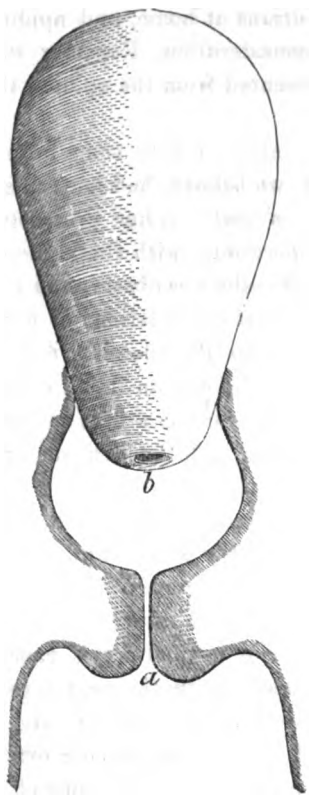
We have now under treatment a case precisely analogous, within a woman about fifty-five years of age, which was handed over to us by



an eminent practitioner, who had treated it for some time under the conviction, that what he touched, as in the figure (at *a*), was the os tincæ, in a state of cancerous induration. And it ought to be observed, that, in all these instances coming under our notice, we have found the part so very solid and scirrhous, as to lead to fears that it could not be amenable to any resolution. It is well known that some urethral strictures do become almost callous.

When called to examine the state of the parts in this case, we were, at first, startled to find such an appearance of malignant disease; but a little reflection enabled us to doubt of the fact. This doubt was easily resolved by passing the probe point of Simpson's sound along and quite through the lengthy stricture, into the free and ample vaginal cavity above. That this was done was verified by

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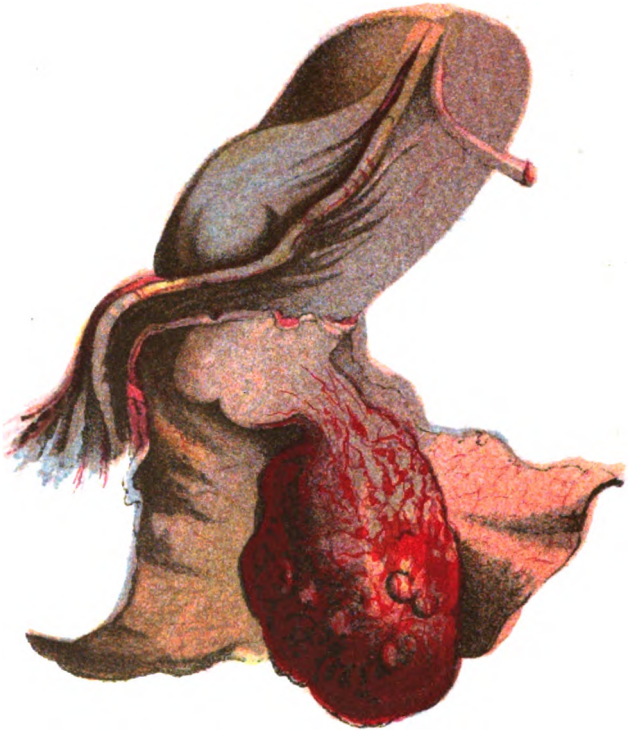

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an examination made through the walls of the rectum, in doing which, the probe point, in the free cavity, was recognized, as well as the shaft of the sound lying in the stricture, and closely embraced by it. The same method of making this diagnosis was employed in the first case described in this connection; and it is one always to be resorted to and relied upon with implicit confidence.

Having stated these cases, which are not affections of the cervix uteri, we hope to be absolved from any charge of transcending the limits assigned to us, inasmuch as we have deemed it proper to array all the means of differential diagnosis that might haply serve to present misapprehension in the cases we have been charged to treat of, and shall now ask liberty to refer to a work entitled "Woman, her Diseases and Remedies," page 106, for the history of a case whose circumstances are forcibly recalled to memory by the above observations.

Hitherto, we have not, in this Report, said anything in regard to the malignant degenerations of the texture, so often met with in inquiring into diseases of the neck of the uterus.

To report upon all the forms of malignant chronic, or acute diseases of the cervix would require a considerable volume, and it is not to be supposed that such a detail would fail to exhaust the patience of the Association. The figure (Plate 18), which we have in this connection presented, is designed to illustrate one form of chronic disease of the neck of the womb, which, in the case at least from which the figure has been carefully designed, was found susceptible of a radical cure.

The patient was a very robust and stout married woman, aged about 30 years, less or more. She had children. Complaining of frequent attacks of irregular menstrua. She appealed for advice to a physician, who, giving an unfavourable prognosis, so excited her fears and the anxiety of her family, that reporter was invited to form an opinion on the case.

The vagina contained a substance of the size and shape of the red mass which in Plate 18 is seen springing from the anterior aspect of the vaginal cervix. To the touch, it communicated the impression always produced by touching the tissues in scirrhous or carcinoma uteri. It had the same resistance and roughness or grater-like feel that belong to cancer of the womb or vagina. The gentlest touch caused it to bleed. It was not sensitive so as to be intolerant of pressure.

The posterior aspect of the cervix gave, by the touch, the idea of normal tissue; and this was likewise the case as to the lips of the os

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tincæ; and indeed, all the accessible parts of the womb seemed to be perfectly sound and healthy, save only where the peduncle, or root of this malignant mass, arose from the anterior aspect of the neck. The vagina was everywhere quite healthy. An inspection of the figure must produce the same impressions as were excited in reporter's mind by the physical diagnosis.

It would, in such a case, evidently be useless to apply any dressings to the surface of the tumour, which must be supposed to be all of one quality and vital temper, from the base to the apex. It would be a mere waste of time, therefore, to act upon the surface of it only. But if the diseased state in fact, proceeded from the base alone, on which it rested and depended for its existence, then one might hope that, in eradicating the very base itself, would be found a sure remedy for an otherwise fatal extension of the degeneration.

It was, accordingly, proposed that the mass should be removed, either with a uvula scissors, or by means of a ligature. The ligature being preferred, was applied, and the mass came away after a few days.

It was also proposed that, as soon as the tumour should come away, the base or root should be destroyed by actual cautery, or by acid nitrate of mercury. The latter was adopted. Every vestige of diseased structure was thus removed, and the lady has now, for three years, enjoyed good health; having experienced very little inconvenience during the whole process of cure.

The question naturally here arises, whether this was a malignant tumour or no. We are aware that some persons will be likely to regard its curability as evidence of its non-malignant nature; but, as for us, we consider that such a mass, being left to work out its own complete development, could not fail, in the progress of it, to involve in destruction larger and still larger portions of the uterine tissue, and at last bring the life of the patient to its premature close. If the tumour was not malignant, it would become so if uncured.

It would have been, perhaps, hopeless to attempt the cure, had the examination not given the most flattering assurances that the diseased mass sprung from a rather superficial portion of the cervix, as was ascertained by the touch. We doubt not that, in numerous instances of parts wholly changed like this, cures have been and will be effected by removing all changed texture—exposing sound surfaces, which, being filled by healthy granulation, restore completely, or almost completely, both form and function to the before diseased parts.

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Fig 1

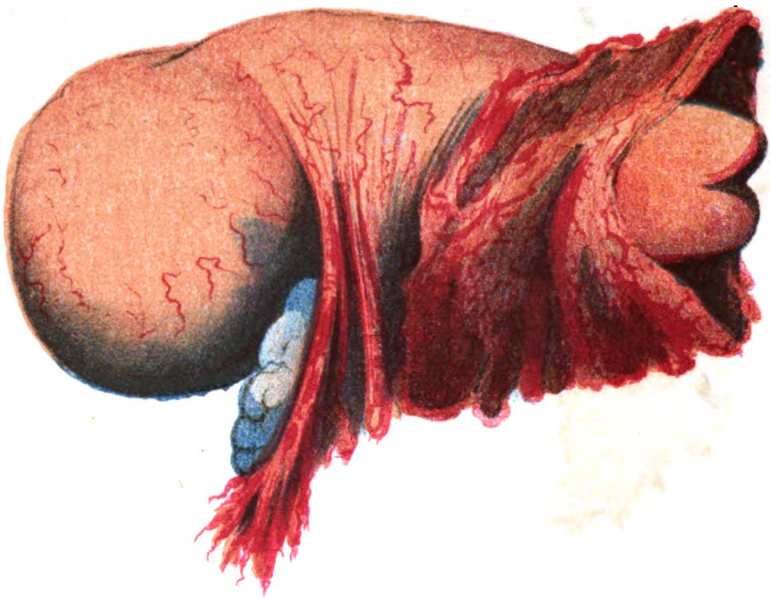

Fig. 2.

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We shall next beg leave to call attention to a form of diseased cervix, of which, during a very long and copious experience in practice, we have met with but one solitary example. And even this is rendered less useful, perhaps, by coincident circumstances of the patient, that serve to lessen the value of the case as furnishing experimental results of the treatment. The lady was near fifty years of age, had borne several children, but none since seven or eight years. She was in an advanced stage of pulmonary tuberculosis, and evidently destined, at an early day, to die with pulmonary consumption.

Certain symptoms, proceeding from intro-pelvic disturbance, and causing great distress, led to the opinion that the whole case was aggravated by some form of uterine irritation; and as she was anxious to be relieved, and willing to undergo an examination, it was found, on making one by the touch, that the vaginal neck and os were both enlarged and painful. The mouth of the womb was much more patulous than it ought to be, and a lump was perceived upon the anterior aspect of the chirurgical neck. On exposing the point by means of a Récamier tube, there was seen a bright elastic tumour of a translucent appearance, which is represented in Plate 19, fig. 2.

The lady, even in her low state of health, was convinced that she was pregnant; but the womb could not be felt through the hypogastric integuments. Whence it was inferred that the organ was of the form and dimensions given in Plate 19, fig. 1.

Professor Mütter opened this hygroma with a sharp-pointed, narrow bistoury, and there issued from it a few drops of colloid fluid, but as transparent as albumen ovi; after which, came away a few drops of blood, whereupon the tumour, or hygroma, collapsed, and the lady, whose consumption made continual progress, had little or no farther trouble from this special cause; a few touches of the pencil of argent. nitrat. having dissipated the inflammation and engorgement of the womb.

A few months later, she died, exhausted with hectic—the lungs being utterly disorganized by softened tubercles, and numerous vomicæ.

A *post-mortem* examination being allowed, it was found that the hygroma had not filled again, but left a bluish spot or blain. The os was still a good deal open, but the neck was little enlarged. Plate 19, fig. 1, shows a profile view of this uterus, which is deemed well worthy of observation, particularly because the fundus was converted into a tumour, which is seen like a subrotund swelling at the left hand end of the figure, and somewhat separated from the general mass of the

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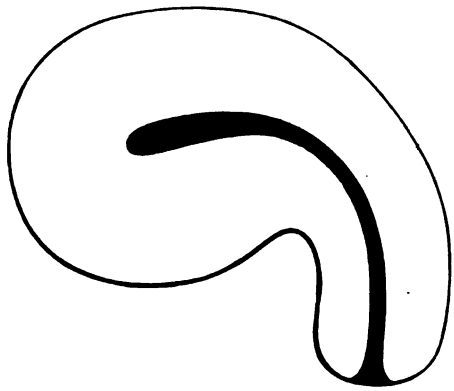
even more than that. Retroversion, with an unbent womb, is a mere accident or chirurgical disorder; whereas retroversion, with a bent or angulated uterus, is a case in which the womb has itself become diseased. It is a case in which by the nutrition of one segment or half of the womb it has grown smaller or larger than the other symmetrical half or segment; for, it is clear, if both the anterior and posterior halves should grow or be developed *pari passu*, the organ could be



only straight, and could be by no means crooked or bent, as in the above diagram. The quantity of the convex must greatly exceed the quantity of the concave half; otherwise, the womb could not be bent, but must remain straight or fusiform.

In treating these retroversions, therefore, we must expect to meet with greater difficulty and delay in the case, if we have an angulated organ to rectify, as well as a case of retroversion. We do not mean to say that the methods are different, since there is but one good method for all of them, and that is a treatment by the pessarium of a proper form.

And here we may beg leave to notice what we regard as a great oversight on the part of practitioners, in regard to one of the chiefest sources of embarrassment and ultimate disappointment of the cure. The circumstance here alluded to is this. In retroversions of some considerable duration, the anterior columna of the vagina has become shortened, and in consequence of its being without any antagonistic or opposing force, allowed so to coacervate its material as to be indisposed afterwards to remain in the state of extension natural to it.

We believe that all living soft parts have an indefeasible tendency to consolidation, when not prevented by an antagonizing power.

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This depends, indeed, on what Bichat announces as the *contractilité par défaut d'extension.* If the os uteri should be brought close to the symphysis pubis, and kept there for some months or years, it is clear that if any one should cause it to remove backwards again to its place three and a half inches away from the crown of the arch, and then let it go, it would leap back again to the vicinity of the pubis, being drawn thither by the anterior column of the vagina, which contracts like a bit of stretched caoutchouc. Now, this is the circumstance which we complained of as being too much overlooked by our physicians in their intentions as to a cure. They are well enough aware that when the womb is overset, or retroverted, it is useless to reposit it, and then abandon it to itself, since it always falls over again with the first motion, or effort to stoop, or to cough, &c. They have used pessaries of every possible shape, and they have been much annoyed to find their patients worse instead of being better for their use.

An immense use has been made of the globe-pessary; and certainly, in a simple prolapsus uteri, it answers admirably. But, it does not answer well for the cases of retroversion; because the morbidly condensed and elastic anterior columna of the vagina, slowly contracting in order to recover a state of rest, pulls the os uteri forwards over and above the convex upper surface of the ball, and draws it again close up to the symphysis, the fundus meanwhile sinking down backwards into the peritoneal cul-de-sac between the rectum and the vagina. In this case, the woman is greatly annoyed, having both a pessary and a retroversion to contend with.

Many years ago, reporter was in attendance on an aged woman, who had long suffered from prolapsus uteri. She had been taught, by a nurse, to construct a pessary, by making a whalebone ring, some three inches in diameter. This ring being wrapped or *served* with bobbin, was dipped in melted wax, until a sufficient coating of wax was given. In this way, she made an elastic annular pessary, by means of which she was enabled to pass through many years of an advanced age, without any inconvenience from a procidentia which always threatened to return upon the removal of the support.

The globe-pessary of Dr. Physick, and the concavo-convex disk, or Dewees's pessary, have been much employed in Philadelphia, and throughout the United States generally, as might be expected from the eminent position and repute of both those gentlemen.

The globe of Dr. Physick is liable to the objection above stated, as a remedy for retroversion; and Dewees's disk is equally obnoxious

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at last taken away, the woman is found to be cured; for the ligam. utero-sacralia are now condensed again, and the anterior columna vaginalis has regained its ductility.

The method of constructing annular pessaries of watch-spring, as above set forth, was detailed at a meeting of the College of Physicians of Philadelphia, by the reporter.

At a subséquent meeting of that College, one of the Fellows thereof, Dr. Charles Evans, reported an improvement, consisting in coating the watch-spring served annules with gutta percha dissolved in chloroform; and this fortunate idea has rendered the instrument so perfect that we may suppose it to be all that could be wished for as an apparatus for the purpose of treating retroversions.

Reporter has long used annules made of absolutely pure silver cylinders, bent into the circle and gilt with fine gilding. Such an instrument is excellent, and is so pliable that it may be crushed into an elliptical form before its introduction, and then, by a little dexterity, opened again into the circular form while lying within the vagina. This is perhaps the most perfect of these instruments, yet difficult to use and to obtain.

A reference to Plate 20 shows that, if the posterior segment of such an annulus is lodged within the cul-de-sac of the vagina that is found behind the neck of the womb, while the anterior segment reposes upon the tissues just above and behind the crown of the pubal arch, it is absolutely impossible for the retroversion to take place; and it is to be believed that, in a space of time, greater or less according to a variety of circumstances, all the tissues concerned in keeping the uterus in its place will recover their tone, by virtue of the physiological force called by Bichat *contractilité par défaut d'extension*. It could not be otherwise. As to the external apparatus called utero-abdominal supporters, which we have the mortification to see prescribed and applauded by physicians and surgeons, even eminent persons in our class, we can but express again our conviction of their utter uselessness, as well as the liability of those who use them to aggravation of all the evils for which they are the pretended remedies.

We have here placed a drawing (Plate 21), taken from the museum of Jefferson Medical College, at Philadelphia, which represents one of the specimens there of polypus of the womb; and we have felt constrained to present some remarks upon cases of this kind, inasmuch as they greatly interest the cervix of the uterus, whose acute and chronic diseases we are commanded to describe. And herein we do not feel

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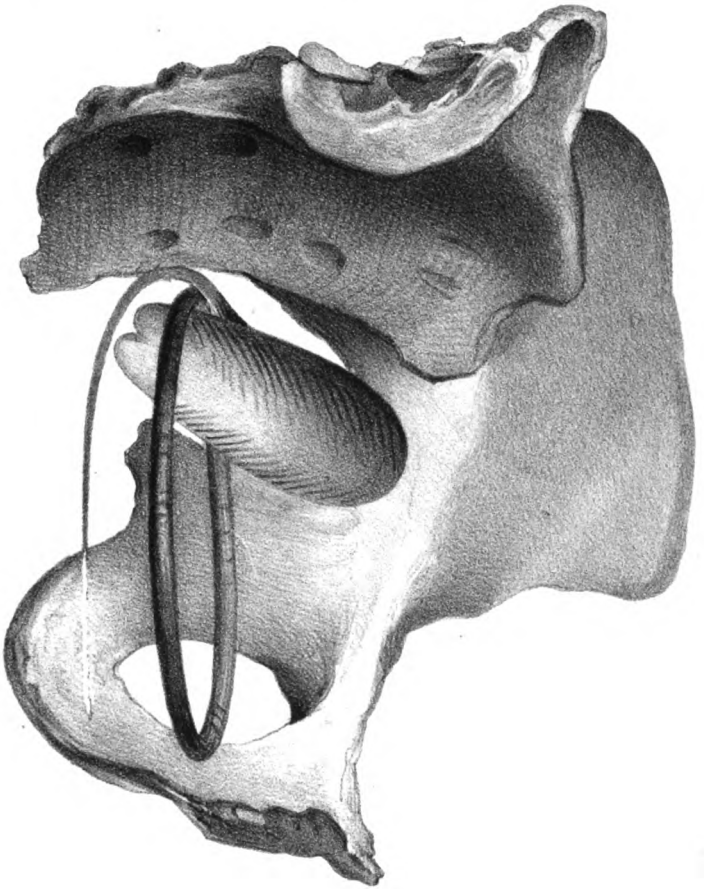

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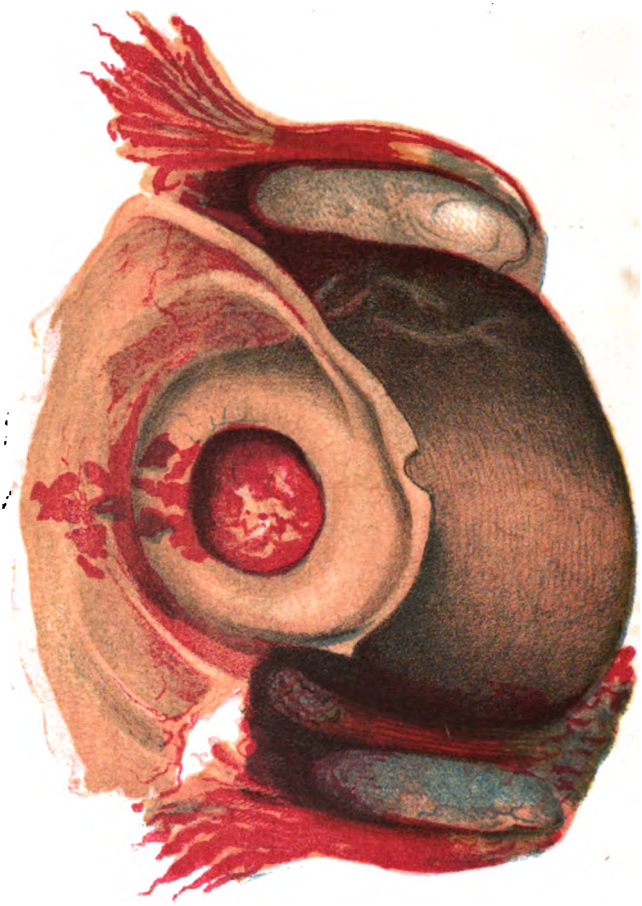

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called upon to go into a full account of the various uterine polypi. Let it suffice to say that they consist of tumours, hard or soft, which rise up from the surface, whether exterior or interior, of the child-bearing organ. Many of them, however, being developed in the very substance of the organ, and invading greater or lesser portions of its structure.

These growing polypi depend upon non-equable development of certain of the elementary tissues of the womb, which, being produced out of all normal rate, may attain a great magnitude or mass. They vary from a few grains in weight, like the currant-shaped excrescence in Plate 16, fig. 1, or the bunches in Plate 16, fig. 2, or the more considerable tumour projecting from the os, in Plate 21. We possess specimens of the size of an ostrich egg, and one that was at least six inches in longitudinal by five in the conjugate diameter. We were present at the examination of a specimen, which was, at the time, weighed by the late Professor Wm. E. Horner, and found, with the womb which contained it, to weigh thirty-two pounds. The degenerated development-force, by means of which a fibrous polypus rises up on the surface of the womb, or within its substance, may, in some cases, extend so far throughout the whole limits of the uterus as to apparently convert the whole substance of it into the material or nature of polypus. We possess a specimen, in which it would be difficult to discriminate between the character of the large mass, into which the whole womb has become converted, and the substance of the large polypi just now mentioned as in our possession.

But lest we should be tempted to carry this investigation far beyond the limits proper for this report, we shall confine ourselves to remarks upon the interior polypus, or polypus of the cavity of the womb.

A tumour of this sort, which, at the commencement, might be supposed to be no bigger than a split pea, growing, by constant nutrition of its mass, soon comes to distend the cavity which it occupies, and that cavity must expand upon the same principles as those which regulate the hypertrophyzation or evolution of the womb in true pregnancy. If the polypus should take its origin within the true cavity of the uterus, the fundus and corpus uteri would, of course, first expand for the accommodation of the growing polypus, just as happens in the early stages of a gestation.

Under these circumstances, the form and size of the neck of the womb do not undergo any change, and an examination per vaginam would not enable a physician to pronounce with assurance that any

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change whatever had taken place in the form and volume of the organ.

The term expansion does not mean merely a stretching of the walls—as when a bladder is blown, but it means a regular physiological hypertrophy, or growth. Now, when the womb grows, it must grow generically, and will, in such case, preserve its generical character as to form; for the idea of genus and species cannot but include ideas of both form and substance, of superficies and contents; when the womb, or parts of it degenerate, then the form goes in the most convenient direction qua data portâ.

A similar uncertainty as to the existence or non-existence of pregnancy ever attends the vaginal examination in the early stages. But in proportion as the polypus on the ovum becomes larger, so must the cervical portion of the womb grow shorter, until, at last, the cylindroidal neck acquires the shape of a cone, or rather a conoidal form; for a polypus, though it can grow indefinitely, and come to be of an enormous size at last, must ever mould itself upon the internal walls of the womb. The polypus, therefore, cannot so alter the shape of the womb as to give it a figure other than such as naturally belongs to it, and characterizes it whether gravid or not gravid.

Sooner or later, in any case of polypus uteri, it may be expected that the lower segment of the tumour shall appear at the os uteri, and gradually dilating it more and more, open it at last so widely that the contractility of the fundus and body shall be able to thrust the tumour out into the vagina.

In the Figure (Plate 21) it is seen that the polypus begins to show itself very plainly at the opening mouth of the womb. But, with the cone of the cervix, and the circle of the os so thick and strong as here shown, it could not be that the polypus shall very soon be expelled from the uterine cavity.

It might be a question whether the diagnosis of such a tumour is to be absolutely relied upon, seeing that it so closely resembles the case of hæmatoma or bleeding fungus, represented at Plate 15 of this report. Yet it is not difficult, either with the index finger alone or with a womb-sound, to ascertain that the fungus at Plate 15 is a hæmatoma springing directly from the inner wall of the canalis cervicis, while the other tumour (Plate 21) is a real uterine polypus rising from a portion of the wall of the true cavity.

The shape assumed by the womb in a natural pregnancy is well known; but, in all cases where the womb contains a true polypus of the cavity, it in like manner preserves its natural form. But, if a

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fibrous or other degeneration of its texture takes place in the substance of the womb, making one or many fibrous tumours, the womb loses its normal form, and becomes lumpy or botryoidal.

The examination of such cases should, hence, always be so conducted as to lead to a knowledge of the form actually possessed by the enlarged organ. If that form be normal, or generical, then we infer that it is occasioned by the presence of something moulded into shape by the womb itself, and that it is a polypus. If, on the other hand, the generical form is not preserved, we may infer the existence of some morbid growth outside of the cavity, but within the very substance or walls of the womb.

The uterus is destined, normally, to discharge a quantity of blood from its cavity with every periodical ovulation, and the custom of women is one so regularly observed, that it gives to the mind a tendency to regard every discharge of blood from the organ as a menstrual discharge. Hence, when women find themselves bleeding too often, too copiously, or in a way too greatly prolonged, it is their habit to consider the deviation as a fault of their *courses*. They do not, in general, consider that blood may issue from such a sanguine organ, without the discharge having any relation whatever to their mensual act; and they usually speak of all such profluvia as disorders of the menstruation. A medical man, on the contrary, ought to possess an ideal standard, by which to compare every such case, and he must at once perceive that such a state cannot possibly be a mensual state, but must be related to some other quality and faculty than those of ovulation and menstruation.

Enough has been already said in this report to show that morbid changes in the corpus mucosum of the womb may serve to explain the frequent reappearance of sanguine discharges from the genitalia. Such discharges as these, however, are not for the most part copious and wasting.

It is true, such discharges as these are most apt to coincide with the hyperæmia of the ovulation, or to have relation to some mental or physical shock, or to the sexual insult, &c. &c.

In cases like that portrayed at Plate 21, the profluvium of blood is likely to be independent of any periodical ovulation, and, indeed, in some instances, is never wholly absent, while the subject of it is liable to sudden enormous effusions of blood, generally greatest at the mensual periods. We have elsewhere related a case, in which the woman was never, during six years and a half, without bloody issue from the genitalia; an issue which arose from a polypus passed into

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the vagina. So many striking instances of this either frequent or constant hemorrhage have occurred to us in practice, that we spontaneously, as it were, adopt it as *prima facie* evidence of the presence of a uterine polypus, when we discover such frequent, or copious, or constant sanguine evacuations.

A polypus, like that in Plate 21, whose lower segment peeps out from the opening os, and whose sides are compressed or strictured by the firm walls of the cervix uteri, could hardly do otherwise than bleed more or less day and night, since the stricturing cervix must necessarily keep up a molimen hæmorrhagicum in the uncovered and uncompressed superficies of the tumour.

It is not always, however, the polypus alone that has begun to project beyond the stricturing circle of the os that bleeds. Many wombs are kept in a state of hyperæmia and hemorrhagical fulness by the presence and pressure of the polypus, and such cases are marked by the most violent floodings. Now, when we come to inquire diagnostically into such cases, and find, upon touching, that the os tincæ is unmodified, and the cylindrical neck unchanged, all that we can do is to infer, for we cannot know, that the hemorrhages are caused by a polypus in the cavity, whose existence, however, we can only conjecture or infer; and especially where the polypus is still so small as not greatly to magnify the womb and make its increase perceptible to the touch in the surpubal region.

There can scarcely be found, in the whole range of medical duties, a more difficult case of diagnosis than this. Time alone can solve the problem; and then only by protruding the mass into the cervix, or out into the vagina, whereupon all doubt is at an end.

In numerous instances, our inferential diagnosis has been verified by immediate examination, or by after events. Yet we have met with samples of such hemorrhage impressing us with a sort of inward conviction that they could only arise from polypus, but which proved to be a mere hypothesis, destitute of any foundation of truth.

We have dwelt thus long on this subject, chiefly out of an anxious desire to put our friends and brethren on guard against making too hurried a diagnosis, and also as a preface to the drawing, Plate 22.

The annexed figure, Plate 22, was drawn from the specimen at the time a *post-mortem* examination was made to ascertain the cause of a fatal uterine hemorrhage.

The woman, several years married, had borne no children. During two or three years, she had been under the medical care of the reporter, who, observing her to be occasionally seized with frightful

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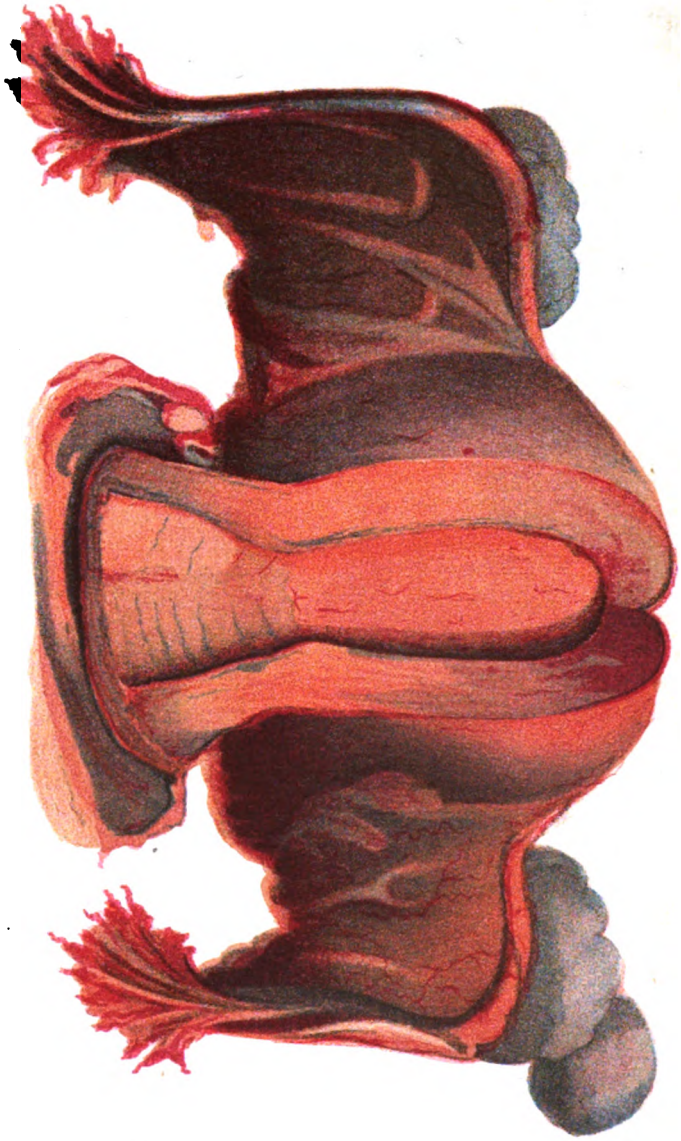

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uterine hemorrhages that left her always excessively hydræmical, could not avoid the conclusion that a small polypus concealed within the cavity of the womb, but undiscernible by any physical examination, would at some future time be thrust out into the vagina, so as to allow of its oblation. Repeated careful examination left the same impression upon the mind.

Meanwhile, when the floodings should be too wasting, she was advised to resort to rest, to hæmastatics, and, above all, to the tamponing of the vagina.

Having removed her residence to a greater distance, she took counsel of another physician, and for more than a year occasionally suffered from attacks of very alarming floodings. Upon the last of these occasions, the Reporter was called in consultation with the attending physician, and hastening to the rendezvous, found she had expired some four or five minutes before he arrived. It was with him and the attending physician anxiously desired, that the question of the existence, or non-existence, of a polypus uteri should, in this case, be settled, and the figure (Plate 22) exhibits a very correct view of the cavity and walls of the womb. There was nothing upon the lining membrane that should throw any light upon the strange and most obstinate hemorrhages, unless, indeed, the magnitude of the cavity be so regarded; and its size is truly given, and may be compared with our figure, Plate 2, fig. 2. But, upon the left ovary was a cystical tumour, which is shown in the figure. Yet no one can assign such a cyst as a sufficient cause for those irregular and enormous uterine evacuations.

Reporter has, at this moment, under treatment the case of a maiden lady, about forty-three years of age, in which the hemorrhages are immense and irregular—sometimes very alarming. In this instance, he can by no means detect any, the least, modification of the form, size, resistance, or natural place of the womb; and, it is probable that no man can say whether these distressing attacks arise from polypus, or no.

It would be a most useful thing to know some certain methods by which to distinguish between habitual violent floodings without polypus uteri, and cases in which the polypus does exist, but cannot be come at. One can scarcely, in medical practice, be put to greater loss for some certainty in discrimination, than in such cases as these.

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The great length of this paper renders it necessary to bring it to a close. There is a considerable number of affections to which the cervix uteri is liable, which it would be easy to discuss; and, in regard to the so-called malignant disorders of the part, it would be gratifying to us to describe and illustrate their various appearances, and suggest methods for their treatment. But we refrain, out of consideration for this Association—which may well expect to receive communications much more worthy to occupy their attention than anything that could be said upon the humble topics with which your Committee have been charged.

In closing this report, the Committee take the occasion to renew the expression of their sincere desire that the class of cases herein treated of may receive a greater share of attention from their brethren in the practice; so that they may less frequently than heretofore be found to accumulate in the large cities and towns, or in the hands of specialists. They are convinced that, as such disorders require no greater amount of particular information or dexterity than other surgical and constitutional maladies, it is within the ability, and is the duty of the brethren generally, to conduct them safely and gently to a satisfactory cure. In our opinion, it would be much more creditable that such skill and discrimination should belong generally to the whole profession, than for one or two individuals in a district, or in a great metropolis, as London or Paris, to acquire a notoriety, which, to say the least of it, is scarcely enviable; since, among the humiliating services that physicians and surgeons are by their vocation obliged to render to the distressed, none can be more revolting to the sensitive mind than these.

It might, perhaps with consoling truth, be added, that few professional ministrations turn aside with greater certainty and celerity the attacks of disease, or arrest the shafts aimed at the existence of our clients.

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AN INQUIRY

INTO THE

NATURE OF TYPHOIDAL FEVERS.

BASED UPON A CONSIDERATION OF

THEIR HISTORY AND PATHOLOGY.

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AN INQUIRY

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THEIR HISTORY AND PATHOLOGY.

SUBMITTED BY

HENRY F. CAMPBELL,

CHAIRMAN OF THE COMMITTEE ON TYPHOID FEVER.

AN analysis of the symptoms of typhoid fever, with a view to its correct pathology, is embarrassed by the difficulty which presents itself in the selection of those phenomena and events which are really characteristic of the disease; for in the immense body of recorded observations with which the investigator is presented, there are comparatively but few which belong essentially to typhoid fever. In the following paper, it has been our object to give a brief but careful summary of its most prominent features, illustrating them when necessary by cases from the latest and most approved authorities, from a consideration of which our deductions have been made.

We have in no instance referred to our own observations of cases, for it is obvious to our mind that any views of disease founded mainly upon rational induction, would be less liable to fallacy, when based on the recorded, careful observations of others, than when they are merely the interpretation of our own cases, always liable to preconceived opinions and foregone conclusions, which have ever proved most effectual and deplorable barriers to correct observation. It is true that, in the plan here adopted, the same objection to a great extent will be found to exist; but by this method we are freed from the fallacies which would be apt to arise from a personal record, with

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a view to establish a doctrine, and will have to contend only with those which must necessarily attach to the interpretation of data, the record of which has in many instances been intended to corroborate views entirely dissimilar and often opposed to our own.

Typhoid fever has prevailed so widely and oftentimes with such great fatality that there are few diseases which have excited such general interest with the etiologist and pathologist. Its symptoms and phenomena have been repeatedly noted with the utmost accuracy, and the recorder of the present day must bound his aspirations, and be content, by simply *verifying* the observations of those who have preceded him. But with the analyst the case is far different; being in search of no new phenomenon, not professing to add anything to what are already cognate facts, in relation to the disease, his object being merely to interpret these facts from data founded upon them, repeated observation and record by others but strengthen and confirm his positions, and afford him a wider field and more abundant material from which to draw his conclusions.

History.—There are certain traits upon which nearly all writers have agreed as being usually characteristic of typhoid fever. In the first place there is always more or less prostration, with an impairment of the functions of the nervous system. Secondly, there is *fever*, which is continuous, but sometimes variable in its degree. Thirdly, in the vast majority of cases there is more or less diarrhœa, generally of an obstinate but passive character; and fourthly, its most constant pathological appearances have been found to be those which refer to the abdominal viscera, there being very uniformly some special alteration in the follicles studding principally the mucous membrane of the mesenteric portion of the intestinal canal known as the glands of Peyer and Brunner.

In addition to the above, which may be considered as almost essential characteristics of the disease, there are a great variety of symptoms described by almost every author as very constantly attendant upon it, and by many considered among its essential attributes. These, although very numerous, will be found generally more or less referable to one or other of three classes of symptoms, the combination of which appears to constitute the disease, viz. disturbances of the *nervous system*, of the *circulation*, and of the *digestive organs*—and so far as practicable, we will thus classify them in our summary. The symptoms pertaining to the *cerebro spinal system*, commonly denominated the nervous symptoms of typhoid fever, consist

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in wandering pains in various parts of the body; cephalalgia, pain in the back, loins, and extremities, more or less delirium, vigilance, somnolence, dulness of hearing, impaired vision, soreness of the muscles, and occasionally subsultus tendinum, together with prostration of muscular power.

In relation to this class of symptoms our reference to statistics has been attended with various results; we find that they have generally been noted with a view to institute comparisons between the two types, typhoid and typhus, and the general result in regard to them, so far as we have been able to notice, has been; that nervous symptoms of one or other character are present in a large proportion of *typhoid* cases, but they do not generally form such prominent features in this as in the typhus type, where their existence is almost invariable, and of a character much more alarming; so that, of the two diseases, the common term "*nervous fever*" would be more applicable to typhus.

DISTURBANCES OF THE CIRCULATION are perhaps the most invariably present of the three classes of symptoms. Of these, we will notice the abnormal states of the pulse, abnormal temperature of the skin, and the capillary congestions frequently observed on the surface of the body.

The *pulse* is almost invariably more or less accelerated in both typhoid and typhus fever. In character, it is irritable, weak, and nervous, showing a remarkable contrast to the full, open, *sthenic* pulse, characterizing the paroxysms of periodic fever. Its frequency is far less than in the latter case, but it is nearly uniform; and although occasionally subject to variations, this general feature of unintermitting continuousness is so constant, that it is from this probably more than from any other feature that these types have acquired the general name of *continued fever*. The average mean of the pulse, according to the accurate observations of Dr. Flint, is $95._{\frac{7}{8}}$ in typhoid cases, while it was found by him to be $105._{\frac{4}{8}}$ in the typhus type, which I think will be found not to vary materially from the observations of others. His tables also show that irregularity and feebleness are much more common in typhus than in typhoid fever.

In relation to frequency he remarks: "There can be no doubt, if the pulse exceeds 120, except for a transient period, that the case is of dangerous severity; and the danger increases in more than geometrical ratio, if it rises above this point."

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Temperature of Surface.—There is, as in other forms of fever, more or less heat of surface, in continued fever, which varies from a little above the natural warmth to what is known technically as biting heat, *calor mordax;* but these aberrations are so common in every form of fever, that we cannot attach much importance to them in a pathological inquiry like the present.

Sweating and Moisture, we find more common in typhoid than in typhus fever. In the former, cases vary much in regard to these symptoms; sometimes the skin will remain usually dry, having at intervals sudden profuse exudations of sweat, soon becoming dry again. In other cases, the surface will be kept in a soft and gentle moisture during the whole disease; while, again, the most profuse colliquative sweats will shrivel the surface and rapidly exhaust the strength of the patient. Upon the whole, there is a greater tendency to moisture than to dryness of surface in typhoid fever; while in the typhus type, decidedly the reverse is found to obtain.

"It would be interesting," says Dr. Flint, "to acertain upon what ulterior conditions sweating is dependent. With reference to this point of inquiry, it would be important to establish any relations subsisting between this event and other phenomena belonging to the natural history of the disease. I have examined the histories of several of the cases characterized by sweating, in order to discover a clue to some connection of this kind, but without success. It occurs not only at different periods of the disease, but apparently irrespective of the circulation, the temperature of the skin, and other symptoms. The antecedent morbid condition or conditions upon which it depends, and the circumstances involved in its production are unknown. All that can be said of its causation is, that it is an event incident to the progress of continued fever, belonging in the category with other incidental events, such as acceleration of the pulse, coating of the tongue, somnolency, &c., all of which are not more rationally explicable than it."

Capillary Congestions.—Among the ordinary appearances of continued fever, there is no one which will strike the attention of the clinical observer more forcibly than the peculiar, diffused congestions on various parts of the cutaneous surface; and though the symptom is easily recalled to our minds by a bare mention of it, yet strange to say, it has been almost uniformly omitted by reporters until quite recently. Even Louis, with all his vigilance, seems to have allowed

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this symptom to escape his attention. Dr. Gerhard, of Philadelphia, appears to have made the earliest and most distinct reference to it, in his account of the epidemic typhus fever, in Philadelphia, in 1836. Published in the *American Journal of Medical Sciences*, and also in *Graves's and Gerhard's Clinical Medicine.*

"A constant symptom," says Dr. G., "observed in every case of typhus fever, was a dull livid red hue of countenance, extending nearly over its whole surface. Sometimes this colour approached a purple. It coincided with a strong, dark red suffusion of the capillary vessels of the conjunctiva, which appeared at the same time with it; but it usually disappeared at an earlier stage than the injection of the eyes. The *conjunctiva never* presented the bright tinge or the brilliant aspect observed in acute inflammatory diseases of the brain, or eye itself."

Dr. William Jenner, of University College, London, has also given recently his observations on this subject, in his work on typhus and typhoid fever. He remarks: "The conjunctivæ were more or less intensely injected in twenty-five cases; and in all of those in which the opportunity occurred of observing the date of the first appearance of the increased vascularity, it began during the second week. * * * In eleven of the cases the pupils were contracted. The expression was dull, and the bloodvessels had a dark red tinge, instead of their scarlet hue. The suffusion of the face and eyes was so constant and so well marked, in the fully formed disease, that it served almost as a pathognomonic sign. It was generally most evident in patients of a full habit of body. Towards the close of the disease, the leaden colour was gradually changed into a dull ashen tint, which remained until the entire recovery of the patient. The colour of the face when flushed (in typhus) was dusky red, and never pink as the cheeks were in cases of typhoid fever."

But it is to Dr. Austin Flint, to whose admirably accurate and faithful reports we have already referred, and upon which we must continue to draw for reliable data in this inquiry, that is due the credit of the fullest statistical account of this trait of the typhoid affection.

Inasmuch as we consider this symptom, as well as his observations in relation to it, somewhat important to the establishment of the views we have taken in regard to the pathology of the disease, we will give them somewhat *in extenso.* He remarks that these alterations of colour were evidently due to capillary congestion. They were always more marked on the cheeks than elsewhere, and

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sometimes were chiefly observable in that situation. In many instances, they extended entirely over the body. He describes the redness of the face as resembling closely the appearance which the surface presents after exposure to cold, the explanation probably being the same in either instance, viz: "*retarded circulation of the blood in the capillary vessels.*"* On pressure with the finger, the redness disappears, and returns when the pressure is removed. The redness returned less quickly in proportion to its lividity or duskiness, showing that the colour is an indication of the degree in which the forces of the circulation residing in the capillary system are depressed.

In the two types of continued fever, Dr. Flint found this symptom to differ very much; being much more frequent in the typhus type than in the typhoid, and also of a much darker and more livid hue in the former. In typhoid, it was seldom more than red, whereas in typhus, it was more frequently dusky or even livid than otherwise; showing that, whatever cause acted in its production, this cause existed in greater force in typhus than in typhoid fever.

"It may be suspected," remarks Dr. F., "that the capillary congestion of the face and surface generally, is dependent on the complication of pulmonary disease, in consequence of which, the free transmission of the blood through the lungs is prevented, and its aëration compromised. With reference to this point, I have compared the pulmonary symptoms with the symptoms now under consideration, in all the cases, and find that this connection does not uniformly exist. Several of the cases in which the congestive redness and dusky tint of the face was marked, presented pneumonic complication, denoted by cough and expectoration, accelerated respiration, and occasionally by the physical signs of inflammation of the lungs; but these symptoms and signs were absent in other cases in which the appearances of the face were the same; and the lungs evinced notable disorder and inflammation in cases in which no capillary congestion is recorded in the notes. To speak with greater exactness of the cases of typhus, congestive redness of the face and marked disturbance of the respiration were associated in nine cases. The congestive redness was present without notable disturbed respiration in two cases.

"Thus it appears that the condition of the capillaries is not attributable to the morbid condition of the lungs, or to derangement of the functional activity of these latter organs. It is worthy of note,

* Austin Flint's First Report, p. 29.

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however, that in typhus, in a large proportion of cases, capillary congestion is associated with pulmonary affection; while in typhoid the *absence* of such a connection appears to be the rule. It is highly probable that both pulmonary disorder and congestion of the surface, may be the effects of the same prior morbid condition. It is not likely that the capillary congestion in continued fever is limited to the surface when it is present in the latter situation. Internal parts, could they be observed, might be expected to present similar appearances. The lungs, as is well ascertained, are liable to become congested during the febrile career, and the form of pneumonitis which usually occurs as a complication of this fever, called pseudo pneumonitis, is usually supposed to result chiefly *from the passive engorgement* of these organs. This complication, however, is thought to be far more liable to occur in typhus than in typhoid fever. The morbid condition, then, whatever it may be, which determines the congestive redness of the skin is the occasion of congestions elsewhere. . . . In congestions pervading the capillary system of the general circulation, the lungs are especially prone to become involved. This is probably the fact in cases of typhoid as well as typhus, in which congestive redness of the surface is observed, but not in a degree sufficient, generally, to lead to a notable pulmonary complication or marked disorder of the respiration. The latter consequence, it would be expected, should much oftener be present in typhus, inasmuch as in this type, not only is capillary congestion present in a much larger number of cases, but it exists in a much greater degree."

Conjunctival Redness.—Very nearly allied to capillary congestions of the general cutaneous surface of the body are those of the conjunctiva and other external mucous surfaces—and indeed by many they are classed with them, as occurring at the same time and apparently as a part of the same set of symptoms. Dr. Flint, however, has shown by his records that congestions and suffusions of the conjunctiva are by no means always associated with those of the general surface, but occur independent of them: "not only capillary congestion of the skin does not exist in all cases of congestive appearance of the conjunctiva, but the latter may be absent in cases in which the former is present." He farther remarks that this does not depend on cephalalgia. This, however, does not prove that they may not both be the effects of the same pathological condition—

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circumstances which we are not able to appreciate occasioning the manifestation of either exclusive of the other.

After a careful estimate of his cases and their various features under comparison, Dr. Flint inquires what is probably the *prior condition* upon which these two congestions in common depend, or in other words, what is the common proximate cause? He considers that it depends upon the morbid condition of the blood itself, and does *not* relate to the state of the nervo-muscular forces influencing its circulation in the capillary vessels. In strict accordance with his excellent statistical method, he reasons thus from the fact, that if the cause resided in the vessels and not in the blood, inasmuch as the "pulse is the thermometer of the forces presiding over the general circulation," we should expect to find the circulatory forces compromised by the disease in proportion as the pulse was found feeble, weak, and irregular; but as his records have been attended with a different result, hence his conclusion varies from what would at first appear most rational. It will be our duty, a little later in this inquiry, to refer again to these views, and to endeavour to ascertain whether the present state of our knowledge of the physiology of the capillary circulation, will not relieve us from the necessity of regarding the condition of the heart's action as only the arbitrary criterion of the capillary circulation.

Cutaneous Eruptions.—This class of symptoms being present in both the typhoid and typhus types of continued fevers, but under entirely different forms, it will be found convenient for the purposes of our analysis to institute a comparison of their prevailing characters in the two diseases. It will be found on reference to authors, that they very generally agree that in the majority of cases of typhoid fever, there is an eruption characterized by well-defined spots of a rose colour, varying in size from that of a pin-head to a quarter of an inch in diameter. Louis observed them in twenty-six out of thirty-six of his fatal cases,* and Dr. Jackson's observations, in the Massachusetts General Hospital, have been attended with similar results, viz. about two-thirds of the cases presented the eruption.† Dr. Hale, as quoted by Dr. Bartlett, found the rose

* Louis, Researches on Typhoid Fever.

† In passing, we would make the remark, though of course it has no bearing upon the object of this inquiry, that, according to our observations and those of many physicians with whom we have conferred in relation to the eruption of typhoid fever, in those cases which occur in the Southern and Middle States, even where the disease

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spots in one hundred and seventy-seven out of one hundred and ninety-seven cases. Dr. Flint reports thirty-seven out of forty-eight cases in private and hospital practice. His description of the eruption is as follows: "It is generally limited to the chest and abdomen, but occasionally extends to the extremities. It may be copious, particularly over the chest and abdomen, but oftener it is the reverse, the spots being few in number. The eruption is of a rose-red colour (rose spots, *taches roses*), the spots are oval, appearing somewhat elevated. The redness momentarily disappearing on pressure."

In Dr. Jenner's description, we shall perhaps find all that is necessary to characterize this concomitant of typhoid fever: "The spots were slightly elevated. To detect the elevation, the finger had to be passed very delicately over the surface, as they had none of the hardness of the papulæ of lichen or of the first day's eruption of smallpox. Their apices were never acuminated, never flat, but invariably rounded, their bases gradually passing into the level of the surrounding cuticle. * * * They were circular, and of a bright rose-colour, the latter fading insensibly into the natural hue of the skin around. They never possessed a well-defined margin. *They disappeared completely on pressure*, resuming their characteristic appearances as soon as the pressure was removed; and this was true from first to last, from their first eruption to their last trace. They left no stain of the cuticle behind; *they never passed into anything resembling petechiæ;* the characters they presented on their first appearance continued till they vanished. Their ordinary size was about a line in diameter, but occasionally they were not more than half a line, and sometimes a line and a half. The duration of each papule was three or four days; *fresh papulæ made their appearance every day or two.* Sometimes only one or two were present at first, ran through the whole course described, and then one or more fresh ones made their appearance and vanished in three or four days, and were followed by others to last as long. The number of papulæ seen at one time on the surface was ordinarily from six to twenty; though occasionally there was only one, and sometimes more than a hundred."

has prevailed as an epidemic, these eruptions are very rarely observable, although looked for with the utmost diligence throughout every stage of the disease. This absence of eruption in cases where every other symptom characteristic of typhoid fever was present, has been so uniform that many of our most intelligent practitioners have been disposed to doubt the diagnostic importance of eruptions in typhoid fever, and to look upon them as of accidental occurrence, not having any important connection with the true pathology of the disease.

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"They usually occupied the abdomen, thorax, and back, but were occasionally present on the extremities. * * * * A very pale delicate scarlet tint of the skin sometimes preceded the eruption of the papulæ, but never lasted more than a day or two; the skin resembling in tint that of a person shortly after leaving a hot bath. Rose-spots occurred in nineteen of the twenty-three fatal cases here analyzed."*

The typhus type of continued fever has been so uniformly characterized by cutaneous eruptions, that it is very often, even at the present day, classed among the eruptive fevers. Many of its synonymes indicate the frequency of this concomitant; viz., exanthematic typhus, *spotted fever*, *maculated fever*, *petechial fever*, &c., &c. And while, on the one hand, we find authors disagreeing in relation to the importance of an eruption in the typhoid type, they very uniformly, where the opportunities have been sufficient for extensive observation, agree as to the eruption of typhus being one of its pretty constant, indeed almost uniform, concomitants. Dr. Gerhard says: "It was present in thirty-two out of thirty-six whites. Of the four cases in which it was not visible, one died on the seventh day of the disease, and the others presented only slight symptoms of fever, which disappeared in four or five days. It was also visible, though less distinctly, in mulattoes, and we may infer that the colour of the skin alone prevented its appearance in the negroes."

In all of Dr. Jenner's fatal cases of typhus, forty-three in number, "*the mulberry-rash*," as he termed it, was present.

According to Dr. Bartlett, the eruption differs, in many respects, in a striking degree from that of typhoid fever. Its colour, especially after the second or third day of its appearance, is that of a duller or darker red. The spots are of a dun, dusky purplish hue; in some cases they become almost black. They vary in size from that of a minute point to a line, or even to an eighth of an inch. They are less regularly circular or oval than the rose-spots of typhoid fever. They are not elevated above the surrounding surface, and disappear but very partially, or not at all, on pressure. They are, almost always, much more numerous than the rose-spots of typhoid fever, *covering in many cases the entire trunk and extremities. Sometimes* they are spread almost as thickly as the eruption of measles.†

By Pringle, these spots are referred to in the following words:

* Jenner on Typhoid and Typhus Fevers. † Treatise on Fever, p. 220.

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"There are certain spots which are the frequent, but not invariable attendants upon the fever in its worst state. They are of the *petechial* kind, of an obscure-red colour, paler than the measles, not raised above the skin, of no regular shape, but confluent. The nearer the spots approach the purple colour, the more ominous they are, though not absolutely mortal." Dr. Stewart (as quoted by Dr. Bartlett) says the rash is permanent, and does not, like that of typhoid fever, come out in successive eruptions; that it, in all cases, presents the two periods of increase and decline, and that in the more severe cases it may exhibit, during the period of increase, four different states—*florid*, *dark*, *livid*, and *petechial*. When the hue of the eruption is florid, it readily disappears under pressure; when dark, it still disappears, but more slowly; when livid, semi-petechial, or pseudo-petechial, it is only partially effaced; and when petechial, it is not in the least affected by pressure. Dr. Henderson, of Edinburgh, has also noticed, that the progress and development of the eruption corresponded with the severity of the other symptoms of the disease, and that the decline of the eruption was nearly simultaneous with the first symptoms of convalescence; and, that the mortality and duration of the disease were in a very remarkable correspondence with the abundance of the eruption.

Dr. Jenner describes three stages, through which these spots pass from their commencement to their perfection—he calls them the mulberry-rash peculiar to typhus fever; says they are never papular; that each spot is slightly elevated, and of a dusky-pink colour, flattened on the surface, with no well-defined margin, but irregular in outline, and in this stage it disappears completely on pressure. Size, from a mere point to three or four lines in diameter. In two or three days they become darker and more dingy than on their first appearance, and now they only *fade* on pressure, but do not disappear. In the third stage they become dark purple, and remain unaltered by pressure, although their circumferences still fade. Sometimes they are changed into true petechiæ, or spots presenting the following characters: "A dusky crimson or purple colour, quite unaffected by pressure; a well-defined margin, and total want of elevation above the level of the cuticle."

Dr. Clymer, in his work on fever, thus describes these spots:—

"Their resemblance to flea-bites is such, that on the one hand the latter are often mistaken for petechiæ, while on the other hand some physicians will insist that real petechiæ are nothing else but flea-bites. The two appearances, however, cannot be mistaken by a care-

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ful observer, &c. Sometimes the petechiæ are few in number, and escape notice; in other instances, on the contrary, they are very much crowded. This appearance is owing to a *thin stratum of extravasation* on the surface of the true skin, and appears connected with increased force (?) of the circulating system, being most characteristic when reaction is high."

It will be observed that we have classed in our summary both the capillary congestions, and those of the surface, and the various eruptions incident to continued fever, under the common head of aberrations of the circulatory system; and although this has not been the order in which we have usually found them considered by authors, we have still retained it, inasmuch as we regard these two classes of phenomena as dependent upon causes very similar, if, indeed, not identical in their nature, viz.: Impairment of the nervous forces, from which the capillary system of the surface obtains its functional endowments. That the capillaries possess a nervous motive power, independent, and not in all cases correspondent with the heart's action, many facts both in experimental physiology and in pathology would go to corroborate. The exact nature and source of the nervous power are subjects foreign to the present part of our inquiry, and we therefore forbear here to discuss them.

On making a comparison of the cutaneous congestions and eruptions occurring in these two forms of continued fever, the following seems to be the result of our analysis of the foregoing summary of the observations and opinions of our best authors:—

Firstly. That both capillary congestions and eruptions of one or other kind are present on the cutaneous surface in the vast majority of the cases in both the typhoid and typhus type of continued fever.

Secondly. That these phenomena are more characteristic of typhus than of typhoid fever.

Thirdly. That those of typhoid fever are of a less serious character—more nearly resembling the healthy action of the capillaries than those of typhus, as evinced in the *congestions*, by the more florid and natural colour of the surface; showing that the blood, although circulating abnormally, still retains nearly its proper amount of oxygen—*i. e.* its retardation is not so great as in the typhus type, when the congested surface is "dark," "purple," "livid," and "dusky," "*resembling in appearance the cutaneous surface when its circulation is retarded from exposure to cold.** In the *eruptions*,

* Dr. Flint's First Report on Continued Fever.

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the same kind of attributes indicate that the aberration is of a less serious nature in typhoid than in typhus—the spots are very few in number, and often absent altogether, very rarely extending to the extremities; they are simply of a reddish or pink colour, which readily disappears on pressure, returning when the pressure is removed; while in typhus fever they are very numerous, generally appearing on the extremities as well as on the body; they not only arrive at the stage of simple congestion, but they go beyond that point, do not yield to pressure, and, finally, after a continuance of some days they become true *petechiæ*,* regular hemorrhagic spots, evidencing extravasation,† and, of course, a certain degree of disorganization in the vessels.‡ Again—If we look at the character of the two eruptions in another light, the same opinion of their relative gravity must obtain; the typhoid papula is an *elevated* object on the surface of the skin—the elevation is the result of an elimination *from the* blood, caused by a *kind of inflammation*, which is always a phenomenon occurring under vital influences: this pseudo-inflammatory process subsides by effusion, and one crop of papulæ is succeeded by another, which observes the same course, till the recovery or death of the patient; but in the typhous petechiæ these phenomena are absent, or, at least, only present on the first or second day, for they soon give place to others which are the evidence of more serious obstruction, and which are of such a character that their occurrence does not necessarily imply vitality in the parts wherein they occur. Thus we may say that, in *typhoid* fever, the redness of surface generally, and the eruption, may be termed true *congestion*, while the

* Dr. Stewart, as quoted by Dr. Bartlett. Also Jenner, on Typhoid and Typhus Fevers.

† Clymer on Fever.

‡ On this subject we find Vogel expressing himself, in his section on the various forms of hyperæmia, in the following satisfactory manner:—

"In typhus, petechial, and putrid fever, &c., the extravasation of true blood is very frequent, *but is always dependent* on *laceration* of the vessels; and in producing this laceration, a change in the composition of the blood can only act a very secondary part. It can only act through a series of means, by favouring congestion and stagnation of the blood." And in another place he says: "Extravasated blood always proceeds from the vessels, and results from their laceration. The view that, at least some of these effusions of blood may occur without any injury of the vessels, by a mere transudation of the blood through the attenuated vascular walls (diapedesis), is altogether untenable, although some even of our recent authors (Carswell among others) still support it. The walls of the bloodvessels—even of the smallest capillaries—are so impervious, that it is impossible for such large particles as the blood-corpuscles to pass through them in an uninjured condition."—*Path. Anatomy*, p. 93.

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livid-purple tint, as well as the maculæ of *typhus*, are as plainly the result of true *stasis*."*

Disturbances of the Digestive Organs.—Under this head could be classed many of the symptoms characterizing typhoid fever. In the earlier part of the disease there is often loss of appetite, thirst, nausea and vomiting, together with a great variety of other symptoms, indicating a disordered condition of the nutritive apparatus; but the symptom which it is our object here more particularly to dwell upon, and the one which has been admitted by all to constitute an important part of the disease, if not the disease itself, is diarrhœa.

Louis reports it as present in all but three of his fatal cases; and in relation to the time of its commencement, his statistics show that out of forty fatal cases, from which he was able to obtain information on this point, twenty-two had somewhat frequent and liquid dejections on the first day of the disease. And in all the cases which died, lesions of the small intestine were the only constant pathological appearances, and these were always the first, so far as was appreciable, to commence. In favourable cases, the diarrhœa is sometimes absent, or makes its appearance quite late. There also appears to be a marked correspondence between the severity of the disease and the prominence of diarrhœa in any given case. Thus, in the eighty-eight favourable cases given by Louis, fifty-seven were found to experience severe symptoms, and thirty-one, slight ones; out of the fifty-seven

* Vogel, again, is very plain on the above point, as well as in reference to the source from which these vessels derive their energy. His remarks express well what we would indicate in relation to these two states of the capillaries. "In many cases, capillary hyperæmia is undoubtedly dependent on the nervous system; this, from whatever cause it is induced, gives rise to dilatation of the capillaries, and relaxation of their walls; the dilated capillaries receive, on purely mechanical grounds, more blood than they previously did; and a capillary which in its normal condition could admit of the passage of only a single row of corpuscles, may now admit of two or three. At the same time an excess of plasma escapes through the attenuated walls of the capillaries. [Hence the papulæ of typhoid fever are elevated by this plasma.] The part affected contains an excess of blood-corpuscles, and hence appears reddened. This is the condition which is known in pathology as *congestion*—it frequently occurs in the living body in external parts, as in the face, the eye, or the skin. * * * As that form of capillary hyperæmia in which there is no stagnation of the corpuscles is termed *congestion*, so *the higher degree* (where they *stagnate*, and the local circulation is arrested) is termed *stasis*." In the same place he also indicates it as his opinion, that this passive or hypostatic hyperæmia is the result of nervous depression. Who could ask a more exact portraiture than the above, of the cutaneous phenomena in the two types of continued fever?

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subjects who had more or less severe symptoms, twenty-four had diarrhœa from the commencement of the disease, which will be found to be a somewhat smaller proportion than obtained in his fatal cases. Among the other patients, five had it on the second day, three on the third, four on the fourth, &c. &c.; in two cases, it appeared as late as the 18th and 30th days, but these were very mild cases, the only symptom of the disease being loss of appetite, weariness, loss of strength, &c., which continued for twenty-five days in one of the cases; indeed, we can hardly regard these as cases of typhoid fever, until after the supervention of the diarrhœa.

The length of its duration is variously reported by authors; the sum of Louis's severe cases would show an average of about four weeks, which, perhaps, would be found in any estimate made from so large a number of cases, to be about its ordinary duration. The reports of most writers favour the opinion that its duration is equal to that of the disease itself, beginning with its access, and generally continuing in a more or less degree till after convalescence is fairly and fully established.

The number of the discharges will be found to vary from one or two to twenty or thirty a day; and a cause of great discrepancy in reports of the disease in relation to this symptom, has been that many authors do not regard liquid but infrequent stools as cases of diarrhœa; while others, among whom is Louis, regard all cases, wherein the *thin* passages are found, as presenting the complication of this symptom. We do not think it necessary here to argue which mode of estimating is proper, but for our purposes, the plan of determining by the *quality*, rather than by the *number* of the discharges being most available, we have considered all, as cases with diarrhœa, whenever the discharges were of a liquid character, independent of the action of cathartics. Inasmuch, as our pathology of the disease is more plainly indicated by the aberration in the secretion, than by any other circumstance in this connection, we have considered this pathological condition more or less present, whenever this aberration has been found to exist, and in this view, we are corroborated by the observations of all our best and most philosophic observers, Bretonneau, Chomel, Louis, Bartlett, Jenner, and Flint; and, indeed, every fact in regard to this subject corroborates the opinion, that it is the character of the stools rather than their frequency, which marks the existence of intestinal lesion in typhoid fever.

The stools are generally of a liquid character, of a yellowish or dark-brown colour, more resembling, according to Dr. Bartlett, new

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cider, than anything else. Louis speaks of them as being like coffee-grounds; perhaps the best description we can give, is that they resemble muddy coffee, more nearly than any other liquid. All authors seem agreed that they in general contain but little or no mucus, and but seldom resemble in character the discharges found in ordinary diarrhœa, and that they are seldom complicated with those of a dysenteric character. Their odour is fetid, though not invariably so. There is frequently more or less blood found mixed with the passages. In two cases reported by Louis (Obs. 18 and 44), the blood was pure and fresh; in the majority, it presented the appearance of being coagulated, and gave to the evacuations the appearance and consistence of coffee-grounds, which he says is very remarkable, inasmuch as evacuations of this nature were seldom if ever observed in the course of acute diseases, other than the typhoid affection, and because, when we may be in doubt as to the nature of any disease, their existence will contribute very much to make the diagnosis more clear and satisfactory.*

Pain in the Abdomen.—In connection with the diarrhœa, abdominal pain is found to be a very common attendant on typhoid fever. It occurs most frequently in the right iliac fossa, in the hypogastric and umbilical regions, and is generally in direct relation in its severity to that of the diarrhœa. It is oftener of a dull aching character than otherwise, and sometimes there is only a slight soreness on pressure. These pains and their relations to the lesion of the intestinal canal, have been very fully considered by M. Louis, who thinks that they indicate a diseased condition of the mucous membrane of the small intestine with scarcely less certainty than does the diarrhœa itself. In a great number of his patients, these pains began before the diarrhœa, often on the first day of the access of disease; and though they were generally very shortly succeeded by diarrhœa, yet not invariably, nor should we necessarily expect them to be, inasmuch as we know full well that in true enteritis, diarrhœa does not uniformly occur on the first day, although the seat of the inflammation is in one or other of the intestines.† Dr. Bartlett describes them as continuing frequently through the whole course of the fever, and as being often the only cause of distress to the patient.

Meteorism or *tympanites*, is an intestinal symptom, to which M. Louis and most authors attach the greatest importance. The flatulent distension of the abdominal parietes was found by him to occur

* Louis. Part III. page 18.

† M. Louis. Part III. Chap. 1.

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in thirty-four out of forty-six cases of the disease. It was variable in its time of coming on; sometimes within a few days of the attack, but generally much later. By a few writers, in which number is Dr. Hale,* of Boston, it is reported as occurring most frequently quite early, but Dr. Bartlett says this is entirely opposed to his own observation and the experience of many of the best authorities.

"It varies in degree," says Dr. B., "from a slight rigidity of the muscles and straightness of the parietes to the extremest distension; in such cases occasioning, by its mechanical action on the lungs, no inconsiderable degree of dyspnœa. It generally persists after its first appearance till the fatal termination, or the approach of convalescence; although it is not usual for it to vary considerably in degree, at different periods of the fever. The flatus rarely passes off per anum, and seems to be but little disturbed by the peristaltic motion of the intestines."†

In relation to its diagnostic value M. Louis remarks: "In the same proportion that meteorism was frequent in individuals attacked with typhoid fever, so it was rarely observed in the course of other acute diseases. Out of eighty subjects, the greater part of whom had diarrhœa, six only had meteorism; four pneumonic patients out of fifty-six had some meteorism during three or four days also; but only two out of forty-six attacked with eruptive diseases, had any meteorism." He did not meet with this symptom in other acute diseases, even in any one of the eighty-six cases of enteritis, properly so called, whether more or less severe. In speaking of the cause of this complication, M. Louis says: "Although in the actual state of our science, I cannot assign any cause for meteorism, I would, nevertheless remark, that it seemed to me to follow a law which was analogous to that by which other symptoms were governed; since, as in those attacked with the typhoid affection, its frequency and force were proportionate to the degree of severity of the principal disease; and among the others, those alone had it in whom the febrile excitement was the most marked; at least such was commonly the case."‡

Pathological Lesions.—For the description of the anatomical lesions of typhoid fever, various plans and classifications have been devised; but as, in the present inquiry, it is indifferent as to the result what mode we adopt, we have chosen that which seemed most con-

* Medical Communications, Massachusetts Medical Society, 1839.

† Essay on Fevers, page 76.

‡ Louis. Part III. page 36.

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venient, and which would place those particular changes from which we deduce our views of the pathology of the disease in the most distinct relation with the other data under consideration. Many of the pathological changes observed in this disease are considered by authors as merely of an accidental or occasional nature; while others, from their frequency, indeed invariableness, have been regarded as the essential lesions of typhoid fever.

The Brain and its Membranes are seldom the seat of any very important lesion in this disease, which fact is very surprising when we consider the amount of functional derangement so constantly present. When these lesions exist, they are not of a character which shows any important relation to the phenomena of the disease, nor are they at all proportionate to the severity of the symptoms which pertain to disturbed functions in these organs. Chomel examined the brain and its membranes carefully in thirty-eight cases; there was no appreciable alteration whatever in fifteen cases. The other cases presented nothing very striking; serous effusions, more or less vascular injections in the membranes and brain itself being their chief characteristics.* Dr. Flint, of Buffalo, has also made the report of a few cases wherein he has made careful examinations of these facts, but with results not materially varying from those of other authors; in no case was there any softening of the brain itself.† Louis examined forty-six cases. There was subarachnoid effusions in twenty-eight, vascularity and more or less injection in the remainder.

In relation to this class of lesions Dr. Bartlett remarks, "there is no ascertained relation between the cerebral symptoms during life and the pathological conditions of the brain and its membranes after death. Delirium and somnolence are found to have occurred as frequently, and to have been as strongly marked, in patients whose brains presented no changes, or exceedingly slight ones after death, as in those of an opposite character. Again, it is obvious that these lesions are in no way peculiar to typhoid fever, since they are found almost as frequently in patients dead from other acute diseases, excluding those of the brain itself and its envelops, as in those dead from fever."‡

The Heart and Circulating Apparatus were found altered in about half of Louis's cases, the alteration consisting in the substance of

* Chomel, Leçons de Clin. Med. p. 294.

† First and Second Reports on Continued Fever.

‡ Treatise on Fevers, p. 90.

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the heart being of a pale colour and soft flabby consistence. In seventeen of his cases (in number, forty-six) the softening was so well marked that the tissue could be readily broken down. This softening was generally accompanied with great flaccidity of the organ, though they would occasionally exist separately.* These alterations, according to most authors, are accompanied by changes of colour, the substance of the heart becoming pale and sometimes of a livid or violet tinge. Dr. Jenner's examinations also corroborate the above, a good proportion of his cases presenting the same alterations.† The condition of the heart does not differ very materially in the two types of continued fever, it being found "flabby, soft, easily broken down in the typhus as in the typhoid variety." Here, perhaps, we can more appropriately than elsewhere notice the condition of the voluntary muscles, between which and the condition of the heart there is found a remarkable difference. We find no account elsewhere than in Louis's researches. He remarks: "I examined the muscles which govern the voluntary movements, in nearly all the subjects, and I found them nearly always natural with regard to colour and consistence; and this is the more remarkable, inasmuch, as I observed before, the *heart* was more or less seriously softened in a great number of cases. (Vol. i. p. 352.)

Louis describes the internal surface of the aorta as changed in colour in more than half of his cases. The alteration consisted in patches of morbid redness, sometimes very bright, and extending from the ascending aorta to its bifurcation, and even sometimes beyond it. The discoloration extended generally deeper than the lining membrane of the vessel, and was proportionate in its intensity of colour, and also in the amount to which the coats were involved, to the degree of flaccidity and softening of the heart.

Louis remarks in relation to this feature; that "the colour of the aorta has been particularly investigated of late by many distinguished physicians. Some have considered the red colour as simply the effect of *imbibition*, met with in violent deaths as well as in various diseases; others have regarded it as the result of inflammation on which depended a series of symptoms called inflammatory fever. But the facts we have just analyzed cannot, it seems to me, be accounted for under either of these hypotheses." He then asks: If it be simple imbibition, why is the aorta not discoloured in all

* Louis's Researches on Typhoid Fever, vol. i. p. 331.
† Typhoid and Typhus Fever, p. 80.

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cases where blood is found in its cavity? On the other hand, if it is inflammation, why did not these patients present symptoms different from those the aorta of which did not present these discolorations? He finally seems inclined to the opinion that it depends upon the imbibition of the blood, but that the special alteration which it has undergone in this disease is a necessary prerequisite to the imbibition.† The most rational explanation of the above appearance, to our mind, is that it is dependent upon the same condition of the small vessels supplying the walls of the aorta, the vasa vasorum, which operates in the minute cutaneous capillaries in the production of the spots and congestion on the cutaneous surface; for we know there are many diseases in which similar alterations of the blood occur as in typhus, and yet no such discoloration is observed in the aorta.†

The Lungs have been found very frequently presenting marked and peculiar alterations. Louis found them nearly natural in fifteen out of forty-six of his cases, which he says is the proportion in which he found them affected in other diseases, excluding of course those of the lungs themselves. Chomel reports ten in forty-two cases. Dr. Jenner says that "congestion is very frequent in those dead from typhoid fever, and rare in those from typhus fever." He calls it the lobular non-granular consolidation of the lungs, which he thus describes: "Externally, a portion of lung in this condition has a mottled aspect; here and there patches, varying in size from a single lobule, to half or more of a lobe, of a deep bluish, chocolate, violet, or purplish slate colour, bounded by a well-defined angular margin, crossed, if it includes more than one or two lobules, and mapped out into smaller patches by dull opaque whitish lines. * * * Scattered in the midst of the larger patches are frequently found one or more healthy lobules." Dr. Flint describes this condition of the lungs as a sort of pseudo-pneumonitis, which probably results from the *passive* engorgement of the lungs, and seems to think it more frequent in typhus than in typhoid fever.

Dr. Bartlett says also, "that it is quite unlike, in almost every

* Louis's Researches on Typhoid Fever, 291.

† Andral and Gavarret have made four analyses of the blood of three persons suffering from scarlatina, which indicate decidedly the existence of hypinosis. Lecanu has also made two analyses of the blood in this disease, and has obtained nearly similar results.—Simon, *Chemistry of Man*, p. 246. Hassall also places smallpox, scarlatina, and measles in the same category, and describes the character of the blood as nearly identical in all, in this respect.—*Microscopic Anatomy*, vol. i. p. 147.

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respect, the secondary stage of inflammation, although the term hepatization has sometimes been applied to it. It is not indicated by any peculiar symptom during life, and he farther expresses the opinion that neither the symptoms nor the lesions go to show that the lungs play any very important part in the numerous and complicated phenomena of typhoid fever;"* but from the comparative uniformity of their occurrence we are disposed to form a somewhat different opinion, and to think their frequency and character are such as to allow the deduction that the lungs, like many other viscera, are immediately under the reign of those morbid influences or conditions upon which the disease depends.

The state of the blood is very properly accounted as being among the anatomical changes, though examinations of this fluid are generally made previous to the death of the patient. From the numerous examinations of Andral and Gavarret, besides many others, the blood appears to exhibit the characters of hypinosis; one of the characteristics of which state is diminished fibrin; more frequently in typhoid and typhus fever than in almost any other disease. The blood in this disease, says Simon, is very deficient in fibrin, and frequently also in albumen. It coagulates imperfectly and often remains in a semifluid state, and when a clot is formed, it is soft and scarcely ever covered with a buffy coat. The character of the blood appears to be very much the same in the typhus as in the typhoid type of continued fever, as likewise are the pathological lesions referring to the heart. Blood more generally dark in the former. The state of the blood in the typhoid affection, however, is not uniform, for, as Simon remarks, it varies much at different times, which has been the cause of much discrepancy in the accounts of various authors; thus, in the stage of excitement, instead of having too little fibrin, it is often found to contain more than in the normal state, becoming then in the opposite condition of hyperinosis. From this it would appear that the state of the blood in the typhoid affection cannot be of the paramount significance that many authors would claim for it; for during the times that this fluid is presenting these very opposite chemical and physical attributes, the main characteristic phenomena of the disease do not appear to be at all altered in correspondence with them, and therefore must depend upon some other influence than the state of the blood for their continuance, otherwise they would be subject to interruptions. If they depend

* Treatise on Fevers, p. 87.

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upon hypinosis, reason would not tolerate the supposition that they remain unaffected during hypinosis.

Abdominal Viscera.—The changes which occur in these organs are by far the most important incident to the disease, and hence we have as one of its synonymes typhus abdominalis; indeed, if we except the weak and accelerated pulse and the altered condition of the heart and blood, nearly all the phenomena, both symptomatic and *post mortem*, may be said to refer to the abdominal viscera.

The spleen, with only four exceptions, was more or less seriously changed in all of Louis's cases. Its structure was ordinarily softened, sometimes thickened, and frequently four or five times larger than natural. Dr. Jenner says, "that it was enlarged in all his cases of typhoid fever; softened in one-third of the cases only." Dr. Bartlett reports it as more frequently enlarged in typhoid than in typhus fever.

The liver was softened in half of Louis's cases, in some to a remarkable degree. Dr. Jenner says that this again, together with the pancreas and kidneys, were more flabby in the cases of typhoid than of typhus fever. There was an ulceration in the lining membrane of the gall-bladder in one of fourteen cases of typhoid, and in nine of thirty-one cases of typhus; in the latter disease, the bile was much more thick and of a darker green colour than in the former disease. Dr. Bartlett adds, that this difference in the appearance of the bile in a large majority of cases is well marked in the two diseases.

The *urinary bladder* was found ulcerated in one of Louis's cases, and also in one of Dr. Jenner's cases of typhoid, but in no one of typhus fever.

Under the head of Organs of Respiration,* Louis describes various changes in the epiglottis, glottis, larynx, and trachea; such as ulceration, red patches, &c. &c., and remarks, that with the exception of that of the trachea, all the mucous membranes were more or less thickened, reddened, softened, and a certain number of times *ulcerated*, in patients who died of typhoid fever.

The *pharynx and œsophagus*, in a large proportion of Louis's cases were found in a healthy condition, but there occurred with considerable frequency ulcerations of the mucous membranes of these parts; in the case of the pharynx, in a proportion of rather more than one-sixth of the cases. These ulcerations varied from one to

* Louis, Researches on Typhoid Fever, vol. i. p. 294.

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six or eight lines in diameter and were various in their contour; generally superficial. In relation to these changes, Louis remarks: "Ulcerations of the pharynx and œsophagus having occurred only in a small number of typhoid patients, and in no other disease, may be considered among the anatomical characteristics of the former; *though they are secondary*, these are very important characteristics, nevertheless, because it would be only necessary to find ulcerations in the pharynx or œsophagus in a patient who died of an acute disease in order to be almost sure of the nature of that disease."*

Dr. Jenner found inflammation with or without ulceration of the larynx and pharynx present in about half his cases. His analysis leads him to the conclusion that the laryngeal inflammation is secondary to the pharyngeal, and that laryngitis, independent of pharyngitis is extremely unfrequent in typhoid fever.†

LESIONS OF THE ALIMENTARY CANAL.—Of all the anatomical changes pertaining to typhoid fever, there are none so peculiar and characteristic of the disease, none which are so plainly and unmistakably indicated by the symptoms during life as those which refer to the alimentary canal. So constantly do these lesions attend the peculiar phenomena which characterize typhoid fever, that at the present day, the one and the other set of facts are considered as invariably constituting essential parts of the disease, and in many instances these are the only characteristics which determine the positive existence of typhoid fever; so that these changes have become the terms by which the disease itself is indicated, as dothinenteritis, follicular enteritis, &c.

The stomach was found to present *post-mortem* changes in about two-thirds of Louis's cases. The alterations of which it was the seat consisted in changes of colour, consistence, and thickness. It was sometimes mamellonated, and four of his cases presented ulcerations; these ulcerations were small in size, superficial, and not very numerous. Chomel found various changes in the mucous membrane of the stomach, but was not able to discover ulcerations. Dr. Flint describes a remarkable and to us an important *post-mortem* fact as occurring in several of his cases, which we have not been able to observe in the reports of others. It is the presence of *ecchymoses*, of more or less extent, in the mucous membrane of the stomach. He

* Researches on Typhoid Fever, vol. i. p. 388.
† Bartlett's Treatise on Fever, p. 91.

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speaks of them as presenting "punctated redness, or ecchymoses," and expressly states that there was "no *capilliform* redness" in the cases where this appearance was observed. "The mucous membrane," he says, "was softened; several ulcerations, varying in size and form, the largest half an inch in length and three or four lines in width, superficial, apparently having penetrated only the mucous coat. These appearances were limited to the larger curvature. The organ at this part was easily torn, a rent occurring in removing it from its spinal attachments." In another case he again remarks, "the stomach, on its internal surface, presented *several patches of ecchymoses.*"*

Louis, who has paid great attention to the gastric pathological appearances, was not able to make out any very important relationship between them and the gastric symptoms previous to the death of patients in whom they existed. He observed that in many cases the lesions existed after death, where there had existed no gastric indications of their presence during life, and that distress in the epigastrium, with nausea and vomiting, often existed when the mucous membrane was found healthy. Chomel was alike unsuccessful in establishing any constant relationship between these two sets of phenomena. Flint makes a similar remark with regard to a few cases in which he made *post-mortem* examinations; thus, "in two fatal cases in which the stomach exhibited lesions, ulcerations in one of them, vomiting is not noted to have occurred."†

From this feature of non-correspondence between the symptoms of irritation in the stomach and the amount of gastric congestion and ulceration, together with the peculiar character of the congestions, as just noted from Dr. Flint, being of a hemorrhagic or ecchymosed character, we would reasonably infer that they are *passive* in their nature, and seldom dependent on nervous *irritation* of the stomach, but are the result of passive engorgement of the vessels; and, indeed, when closely considered, all the mucous congestions pertaining to typhoid fever, will be discovered to be markedly of this passive character, and but little or not at all productive of irritation. There is deficiency rather than excess of action in the vessels, and the nervous susceptibilities of the mucous surfaces appear to be unaccountably obtunded, to what would rationally be expected to act as causes of great irritation, such as ulcerations, thick-

* First Report on Continued Fever, pp. 128–129.

† First Clinical Report, p. 79.

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enings, &c.; as we have just seen that ulcerations exist in the stomach without vomiting, and in many cases of congestion elsewhere, as in those of the conjunctiva, when it did not increase the susceptibility of the eye to light.* And again, in the cases of diarrhœa, the urgency of the evacuations was not very pressing, inasmuch as it was often necessary to call the attention of patients to the subject, by asking them if they did not desire to evacuate the bowels.† We never observe in the diarrhœa of typhoid fever anything like the tenesmus, so common in other forms of enteric disorder; and yet the amount of actual disease in the intestine is far greater than in many of them.

THE SMALL INTESTINE.—The characteristic lesion of typhoid fever, and, of course, by far the most interesting of all the anatomic changes observed in this disease, is found in the small intestine; its existence is invariable, and in many instances it is the only distinguishing trait of the disease. It consists of changes, varying in character, in the mucous follicles, known as the glands of Peyer, found along the mesenteric portion of the canal. The character of these alterations has been found to vary very much, in proportion to the duration of the disease and their distance from the ileo cœcal valve, where the follicles are uniformly the subject of more serious alterations than elsewhere. Louis found the elliptical patches of the ileum more or less seriously changed in structure in all the patients the report of whose cases he subjected to analysis. The region in which they were infected, varied from the last two to eight feet of the canal. They were affected throughout the whole length of the canal in one case only. His description of these changes is more extensive and minute than we consider necessary for the purposes of our analysis, but we will give the substance of them in this place.

He found that the changes, from a healthy condition of the patches to an alteration of structure, took place sometimes, in a gradual manner; but generally it was the reverse, and with all, or nearly all, the different degrees of the lesion were found in the same patient. In passing from the patches which were least altered to those which were most so, he found that they presented the following appearances: the first were very slightly elevated and pale, or of a faint rose colour; the orifices of the crypts were obliterated, and there was slight softening of the mucous membrane; next, came patches in which the thicken-

* Flint's First Report, p. 58. † Ibid. p. 88.

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ing, softness, and redness were more marked. "The first of them had in some cases a surface which was granulated, upon which was seen a greater or smaller number of the open orifices of the crypts. In this state of alteration the most remarkable effects of the lesion was the development, and, if we may so express it, the *exaggeration of the natural structure of the parts.*" The patches were raised, and also the mucous membrane which aided in its formation. The submucous cellular tissue was more or less affected, and of a pale rose colour.* These conditions became more and more marked and exaggerated—that is, the patches became thicker, redder, and softer, till various stages of ulceration are detected; some superficial, and just commencing, with an imperfect destruction of the mucous membrane; others deeper, with its entire destruction. Sometimes they are distinct, at others they run into each other, so that the entire destruction of the mucous membrane over the whole extent of a patch is thus produced. The submucous and muscular coats also participated in these alterations, and the peritoneum became ruptured. Sometimes these alterations took place very rapidly, and perforation of the intestine would result at an early stage of the disease.

In many of the cases which died late in the disease, he found evidences of a reparative process having been set up in these ulcerated patches, and this was always more marked in that region of the intestine in which these ulcerations had begun, viz., the vicinity of the cœcum. In addition to the above, Louis also describes other changes that relate to the elliptical plates, which, he says, are peculiar to the submucous tissue. "Instead of being simply somewhat reddened and moist, without other change of structure—instead of partaking in some measure of the inflammation of the mucous membrane which covered it, as in the preceding variety, the cellular tissue was transformed, either through the whole extent, or nearly the whole extent of the patches, into a homogeneous substance, without apparent organization, of a pale rose or yellow colour. It was from two to three lines thick.

"It was easy to see that this substance was not secreted *from the surface* of the submucous tissue, *but into its substance*, on making an incision through it. These deposits were covered by mucous membrane, more or less reddened in proportion to its advance towards ulceration, which was the ultimate tendency of the parts in which they occurred." Louis says these deposits occurred in some-

* Louis's Researches on Typhoid Fever, vol. i. p. 174.

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what less than one-third of his cases. In relation to the character of the deposit, M. Vogel remarks: "This must in every case be deposited in a fluid state, and subsequently assume the solid form by coagulation; otherwise, it would not so completely fill up all the interstices between the tissues. Upon examination, however, it is always found coagulated; at least, I am unacquainted with an instance where it was observed still fluid." Again, he says: "The question of the origin and signification of this typhus matter can only be partially answered by pathological anatomy." It appears to be ascertained that this matter is secreted in a fluid state from the *capillary vessels*. *Moreover*, the cause of this separation is doubtless a local hyperæmia of the vessels; which, indeed, may be readily proved by direct observation. The secreted matter is therefore a part of the blood, which, shortly after its separation, coagulates. But we are acquainted with no principle of the human body which is capable of spontaneous coagulation, but fibrin. Of this, therefore, the typhous matter must principally consist—it is, as in similar cases, pervaded by the other elements of the blood.

"The question here suggests itself—Is this fibrin normal, or has it undergone a specific change? The possibility of such a change cannot be denied, since we know that fibrin is very transmutable; but the assumption of such a change, without a demonstration of its nature by organic chemistry, is of no advantage in a scientific point of view. By such a hypothetical transmutation of the fibrin, we may endeavour to explain—*Why the typhous deposit is not converted into normal pus, but breaks up without any distinct organization.*

"To this view may be opposed another, equally plausible. *It is* very probable that, in typhus, the *normal properties of the tissues are deprived of their ordinary energy*, and that the *formative* power is impaired. In this diminished *energy of the original tissues may likewise be sought the reason* why *the exudation does not become organized, but undergoes disintegration.* With this brief view of the subject, I wish to express myself as opposed to the opinion that there exists in the blood a specific typhous matter, with the deposition of which (in certain parts of the body) the disease localizes itself, and terminates."

From the above, it would be plainly inferred that the deposit depends upon a congestion or hyperæmia of the capillaries of the mucous and submucous tissues differing in character from inflammation—rather a condition of stasis similar to what has been observed

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in the capillaries of the cutaneous surface in the typhus type of continued fever.

Another variety of disease in these follicles is mentioned by Forget under the name of *gangrenous*. The substance of the gland loses its vitality and becomes of a yellowish or greenish colour; its edges, growing ragged and shreddy, are detached, and finally the entire gland is thrown off. Dr. Jenner says that "when the whole of the deposit has sloughed out no fresh deposit is formed, and consequently, as the whole of that deposit is seated in the submucous tissue, destruction of the muscular fibres of the intestine must be the result of simple ulceration."

In addition to the above alterations in the small intestine, which refer exclusively to the glands of Peyer and Brunner, there are other conditions of this viscus which we consider no less important in an impartial analysis of the phenomena of typhoid fever. We refer to the congestions of the *general* mucous surface independent of the elliptical patches. It has been so uniformly the habit of pathologists since the time of Louis and Chomel, to regard every other abnormal appearance in the mucous surface of the small intestine as entirely of a secondary nature, that though we find, it is true, a sufficiently accurate description of these changes, yet certainly no stress is laid upon them, nor are they generally allowed to have any important significance in the pathological relations of the disease. It is not our object to give any very extended and minute description of them, but having formed views somewhat peculiar in regard to the ultimate pathology of this disease, and regarding this condition of the mucous surface as affording a corroboration of those views, nearly as strong as that afforded by the follicular changes themselves, we would, on no account, pass them over without decided reference. Thus, we find the mucous membrane, independent of the elliptical plates and isolated follicles, to be, in a majority of cases, preternaturally red. This redness is sometimes continuous, at other times it is in zones or patches. It sometimes extends over a large portion of its tract. In some cases, where there has been hemorrhage from the bowels, blood is found in the intestine, and in addition to this, the mucous membrane is the subject of *sanguineous infiltration*, and *this condition of the membrane is observed where there has been no hemorrhage.* It may exist to the extent only of a few inches, or of several feet. It is generally continuous, not in patches or zones. The colour of the membrane ranges from a rose to a very dark red, and it has a peculiarly brilliant and trembling or quivering

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appearance, like jelly. Chomel found this lesion in seven out of forty-two cases. He is very confident that it is intimately connected with the hemorrhage from that portion of the membrane which it occupies.*

The point in reference to this condition of the mucous membrane to which we would call attention, is this: that it indicates most positively an enfeebled condition of the bloodvessels, resulting in that complete stagnation of the blood which is denominated *stasis*, and the analogue of which we described somewhat at length, as occurring in the cutaneous congestions of the typhus type of continued fever. And not only do we claim this appearance after death as corroborative of this view, but in as strong a degree do we attribute the hemorrhages to the same condition of the vessels.

There is one more point in relation to the small intestine and we have done, and that is the almost entire exemption of the *duodenum* from ulcerations and the peculiar lesions of typhoid fever. Louis's reports show that this portion of the small intestine was healthy in nearly all his cases, with the exception of being somewhat reddened and sometimes softened, but rarely ulcerated.

The Large Intestine was found the subject of pathological changes in a good number of cases. Besides the condition of meteorism, which was very common, its mucous surface was found reddened with more or less deep shades; also thickened and softened conditions were not uncommonly met with. Louis says: "The parietes of the large intestine became thicker than usual, sometimes resembling the condition in which we find the coats of the small intestine in *strangulation*. . . . Its mucous membrane was white in thirteen of his cases, and red, over a certain extent, in twenty, grayish in nine; it was of its usual consistence in one-fourth of the cases; it was softened, in various degrees, over a greater or less extent of surface, and was sometimes thickened in the others. Eight presented a certain number of lenticular crypts, few of which were ulcerated; four had hard, some rounded patches similar to those of the ileum; fourteen had ulcerations, which were generally few in number, superficial, and of small size."†

Dr. Bartlett speaks of the mucous membrane as presenting ulcerations in about one third of the cases, and that they were present in

* Bartlett. Treatise on Fevers, p. 98.

† Researches on Typhoid Fever, vol. i. p. 376.

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twenty-three of seventy-four examinations made by Louis and Barth. He says that they occupied most frequently the cœcum, though they were not confined to that portion of the intestine. In a small number of instances the submucous cellular substance of the isolated follicles were found to have undergone the same yellowish transformation that has already been spoken of as occurring in the elliptical plates of the small intestine.*

Although the large intestine has been found less frequently the subject of disease than the small intestine, still its *post-mortem* phenomena are, in our opinion, of too frequent occurrence and of too decided a character, to be regarded as altogether secondary, and in our analysis it will be our object to show that these, like many other of the pathological conditions presented by the various organs, are not more of a secondary nature than are the changes of the elliptical plates of the ileum themselves, and, like these changes, are the direct result of causes common to both. Which causes, in our opinion, are identical with the ultimate pathological condition upon which all the phenomena, both symptomatic and anatomical, depend. To the investigation of their nature and influence we will presently turn our attention.

The lymphatic ganglia of the mesentery are very generally found in a diseased condition, and the amount of disease which they evince seems to correspond with the condition of the elliptical plates of the ileum, though this is not invariable, for those found between the two layers of the mesocolon are often found in the same diseased condition. The changes consist generally in enlargement, softening, and alteration of colour—red, bluish or gray—and in the deposition of purulent matter in their interior, marked by small yellow points.

In the course of the foregoing summary we have very constantly kept in view the differences between the two types of continued fever, although not with that order and regularity which would be necessary in a systematic treatise on the two diseases. It was only important to notice these differences, as they were suggested by the description of the symptoms of the typhoid type. Farther, it will have been observed that in our description of the symptoms occurring under the head of disturbances of the digestive organs, as well as in the account of their pathological lesions, we have instituted no comparison between the two types. The reason of this omission has been, that in the typhus type, none of the symptoms during life, or

* Treatise on Fevers, p. 100.

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the *post-mortem* appearances, pertaining to these organs, seemed to have any important connection with the characteristic phenomena of the disease, there being neither *diarrhœa* nor important intestinal lesion reported with any degree of uniformity in the typhus type;* and, moreover, of late years the basis of distinction between the two types has been the existence or non-existence of disease in the intestinal canal; as will be observed in the admirable Treatise of Professor William Jenner, of London, on the Identity or Non-identity of Typhoid and Typhus Fevers.

The object of the present treatise being to arrive at what we consider the true, or, at least, the most rational theory of the typhoid affection, by an analysis of some of the most prominent phenomena in its natural history, an extended review of the many doctrines now in existence in relation to its pathology, would be inappropriate. Suggestions regarding the proximate cause of typhoid fever, will, we fear, meet no great degree of favour, for there is no disease in relation to which so many theories have been promulgated. The peculiar condition of the blood, the cerebro-spinal system, and the various abdominal viscera, have been each referred to as the pathological domain of this mysterious disease; yet neither, it appears to us, is alone adequate to the entire explanation of all its phenomena.

If we refer it to the state of the blood, and argue on the one hand that the continuously rapid and feeble pulse, the pale and softened condition of the heart's substance, the slight cutaneous eruption, the obstinate passive diarrhœa, the pain and tympanites, the congestion, stasis, and hemorrhages in the intestinal mucous membrane, as well as the changes and ulcerations in the elliptical plates, found in typhoid fever; and, on the other hand, that the still more rapid pulse, more softened condition of the heart's substance, together with the stasis and hemorrhagic spots (petechiæ), found on the cutaneous surface, are all the common result of these changes in the blood, and of these changes *alone;* and if we farther admit, with Andral and Gavarret, that these changes consist mainly in hypinosis, we must at the same time, admit that the theory is embarrassed by a great fallacy, viz. that the blood is often in the very opposite state, hyperinosis, for a considerable time during the course of the attack,

* Spontaneous diarrhœa is as *rare* a symptom in typhus as it is a common one in *typhoid* fever.—Bartlett, p. 216. Amongst Dr. Gerhard's cases: "In the entire number of autopsies there was but a single case, and that of doubtful diagnosis, in which the slightest deviation existed from the natural appearance of the glands of Peyer." —*Ibid.* p. 233.

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and yet no corresponding change in the symptoms is manifested. And farther, could we reconcile this, we would still find it difficult, without admitting the influence of other causes, to account for the production of such very dissimilar results in the two types; the principal manifestation of disease being in a *mucous* membrane in typhoid fever, and in the *skin* in typhus fever.

In relation to the cerebro-spinal system, for reasons sufficiently obvious, we would find it impossible to explain all the phenomena of the disease by here locating its proximate cause; for neither is the character of these phenomena taken as a whole, such as would be recognized as resulting from disease in the cerebro-spinal axis, nor is the correspondence between the severity of those symptoms which are plainly referable to this system, and the gravity of the more characteristic symptoms of the disease sufficiently close to authorize such a conclusion.

A very natural, and on a cursory view, almost inevitable theory, in regard to the pathology of typhoid fever, is that it must be either solely dependent upon, or very closely connected with, that invariable, and generally very serious amount of disease in the lower portion of the small intestine. This lesion, it cannot be denied, presents a greater amount of correspondence between its actual extent and the severity of the general essential symptoms of the disease, than any other set of phenomena connected with it. Louis, if he does not entertain this theory of the pathology of typhoid fever himself, has certainly, in his admirable statistical researches, afforded the strongest corroboration to others for their belief, and is often found expressing himself in a manner very favourable to this doctrine. "Notwithstanding," says he, "the small number of complications, and the comparative importance of each one of them, the symptoms were the same as in a great number of cases, in which the secondary lesions were much more grave; and this with other nearly similar facts, shows that almost all the symptoms observed in typhus fever, and *especially those which may be called characteristic, depend on the peculiar morbid changes in the ileum.*"* And again: "We must conclude *that the time*, at which the *alteration of the elliptical patches of the ileum commenced, was the same as that of the disease;* and we must not consider this lesion as one of the effects of the fever, but that it forms the anatomical characteristic of it."†

* Researches on Typhoid Fever, vol. i. p. 82.

† Ibid. vol. i. p. 92. It is with great reluctance that we attempt an interpretation of Louis's doctrine as favouring the opinion, that the above changes are the proxi-

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From the above it would appear, at the very least, that Louis regarded this lesion as one of the greatest etiological importance. But when we give all the facts in connection with the abdominal lesions a more careful consideration, we must find that it is impossible to account for all the "characteristic symptoms" of the disease, by locating its ultimate pathology in this region, for although there is a more regular correspondence between the lesions and the symptoms, here than elsewhere, still, to attribute the symptoms *entirely* to this local lesion, appears unreasonable, when we reflect that in *typhus* fever, a disease at least *similar* and even by many considered *identical*, there is, as the rule, no trace of such a lesion, no trace of the diarrhœa, or of the pain and the meteorism resulting therefrom. Under this view, one of two positions must be assumed, either that those symptoms, which the two types possess in common, and which endow them with the typhoidal aspect, are due to conditions of the organism which have no analogy in the two diseases, and that typhoid and typhus fever are two distinct diseases. For if the characteristic symptoms arise solely from the intestinal alterations in typhoid fever, as these alterations do not exist in typhus, those symptoms must have their cause elsewhere. Now it would be very surprising if two sets of symptoms, so exactly simulating each other, that the most skilful and experienced have denied the least difference between them, should arise from entirely different causes, the one depending upon intestinal follicular disease, the other upon some as yet unknown cause. Certainly, such a conclusion would not be very philosophical. This theory, then, like the others, requires to be presented in no other light than the above, to show its inadequacy to the explanation of the phenomena of the typhoid form of fever.

It may be asked, what theory then can be advanced, to which so many rational objections will not be urged? We candidly answer *none;* but in our opinion, in the adoption of a theory of this disease, we should consider that most stable and philosophic, which, while it is as competent as any of the foregoing to the explanation of all the phenomena of the *typhoid* type of fever, yet, presents nothing which would exclude altogether from the same order of diseases, its

mate cause of typhoid fever; the more especially, as Dr. Bartlett, than whom no one in this country, in our opinion, is better qualified to do full justice to this great pathologist, has expressed his unbelief that such was his opinion. We may be mistaken in our interpretation of his expressions, and, therefore, have not charged the doctrine upon him, and have only remarked that he appears to have favoured it very decidedly in the above-named places.

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unmistakable counterpart—its archetype, typhus fever. For, it is undeniable, that these two diseases are inseparably bound together, in ties of the strongest and most indissoluble, though mysterious, affinity, and the necessity which any theory may involve, of separating them, is enough, of itself, to declare its absurdity.

The view which we have taken in relation to the proximate cause of typhoid fever, is, we are fully aware, liable to many objections, but after a careful consideration of it, in all its bearings and dependencies, we can but regard it as perhaps less obnoxious to fallacy than most of its contemporaries. "Pathology," remark Todd and Bowman,* "is the physiology of disease; and it is obvious that no pathological doctrines can command confidence, which are not founded upon accurate views of the natural functions," it is equally necessary, we would add, that these views be *comprehensive* as well as accurate, for without due importance being accorded to *relative* physiology, we mean that important bearing and relation which the functions of one portion of the organism has to those of another in health, and more especially in a state of disease, we are apt to lose sight of all just reasoning and legitimate induction, and to pursue but the waifs and shadows of a most uncertain and devious philosophy.

We have seen by only a cursory view, that neither the humoral or sanguineous theory, nor the intestinal, nor yet, again, the cerebro-spinal or nervous theory of typhoid fever, will account satisfactorily for all its phenomena; we must, therefore, look elsewhere for an explanation of them. After the most careful and laborious consideration of the phenomena, relationships, and events of this truly mysterious disease, keeping in view, at the same time, as well as we were able, the *entire* physiological dependencies of such an intricate question, we are induced to regard the typhoidal state, manifested both in typhoid and typhus fevers, as a morbid affection of the whole or portions of the ganglionic system of nerves, known also as the great sympathetic nerve, but of the exact nature of which, and how produced, we know not. In the succeeding analysis, it will be our object to show that the characteristic typhoid phenomena are found to occur in a more marked degree, in those portions of the organism which are admitted to be, and in most cases can be demonstrated as being under the control of the sympathetic ganglia and their various nerves, and that the occurrence of other phenomena is

* Physiological Anatomy, p. 28, London edition. "It is also certain that improvements in pathology must follow in the wake of advancing physiology."—*Ibid.*

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the result of relations, more or less direct, with this important system of nerves. In the course of our examination, we shall endeavour to show that both anatomy and the most approved physiological doctrine of the present day, are, in a great measure, confirmatory of our position, and farther that it will be corroborated by the recorded experiments of this and the past century.

We are aware that referring the pathology of disease to the sympathetic system of nerves, though not without precedents of the most respectable and even revered character,* is still at the present day somewhat unusual, and while, in the confidence of an honest, and if not enlightened, we can at least say, a *studied*, conviction, we argue the justness of this pathology, we would ask in the first place, that indulgence which the defects of our performance must necessarily need, and in the second, a full acquittal from the arrogance which we are aware is more than apt to be imputed to him, who would deny established, and substitute new doctrines in pathology.

In view of the neglect, of which, at least in this country, the nervous system, and especially this portion of it, is the subject, we would be unwilling to enter into our analysis without referring briefly, by way of refreshing the memory, to some of the most important features in the anatomy and physiology of the ganglionic system of nerves. In so doing, we will not embarrass our description with a discussion of the many theories in regard to its functions, but will give that which we have adopted ourselves as most rational, and which, fortunately for us, we are able to say is that most universally admitted by the best physiologists of the present day, and which vivisection, that best of all tests, has most plainly, and, to our mind, incontestably demonstrated.

THE SYMPATHETIC NERVE. ITS ANATOMY.—In this description, we shall give the views of the various authors within our reach who coincide with us in relation to the function of this nerve, using very much the same order of description, and also, when its terseness and comprehensiveness will admit, their language.

Under the title of the sympathetic nerve, say Todd and Bowman,† is comprehended a great subdivision of the nervous system, which presents certain peculiarities of structure and of distribution, whereby it is strikingly contrasted with the strictly cerebro-spinal nerves.

* Lobstein, Structure, Functions, and Diseases of the Human Sympathetic Nerve.
† Physiological Anatomy, Chap. xxi.

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It consists of an uninterrupted chain of ganglia, extending on each side of the vertebral column from the first cervical vertebra down to the coccyx, and moreover extending upwards beside the cranial vertebræ, and occupying spaces between the bones of the cranium and those of the face. The continuity of this chain is preserved by cords of communication which pass from one ganglion to another; the chains of opposite sides communicate with each other, at various parts, in the plexuses of nerves which originate from them. The ganglia are sometimes divided for convenience of study into cranial, cervical, dorsal, lumbar, and sacral, each of these regions, containing a number generally corresponding to the number of the vertebræ in each. From each portion, certain sets of nerves may be traced; these are, omitting the cords of communication between the ganglia—1st. *Visceral* nerves, which generally accompany branches of neighbouring arteries to the viscera; 2d. *Arterial* nerves, apparently devoted to arteries in the vicinity of the ganglia; 3d. Nerves of communication with the cerebral or spinal nerves, which emerge from the cranium or spine, near the ganglia. The visceral and arterial branches have a remarkable tendency to form intricate plexuses, which entwine around the bloodvessels, and the visceral branches are conducted by these vessels into the tissue of the viscera. Of the branches of communication, it is sufficient to say, that they connect the ganglia with the cerebro-spinal system of nerves by means of two different sets of fibres, which are called its white and gray roots. The gray root appears to connect the ganglia of the sympathetic nerve to the ganglia on the posterior root of the spinal nerve, while the white root appears to be a branch coming from the spinal nerve, and to receive an equal amount of fibres from its anterior and posterior roots. These fibres may be seen in every instance spreading out upon the adjacent sympathetic ganglion, passing through the vesicular matter of which it is composed, and following the course of the trunk of the sympathetic for a longer or shorter way, and then proceeding from it in connection with the gelatinous fibres chiefly, to supply the viscera. These fibres are regarded as branches of the spinal nerve, which are distributed in connection with the gelatinous fibres, which are considered the true and proper sympathetic fibres, and are derived from the ganglia. Carpenter* speaks of the cerebro-spinal fibres passing into the sympathetic, and the sympathetic fibres passing into the

* Principles of Human Physiology, p. 558, ed. 1853.

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cerebro-spinal nerves at their roots. From the above, it would appear then that the sympathetic system consists in its intimate structure of two distinct kinds of fibres, viz. those which come from the spinal system of nerves and also of the true sympathetic fibres, which are called gelatinous, and are peculiar to this system, and which originate from the vesicular matter of the ganglia.

Thus constituted, the sympathetic system is found located as above described, in the various parts of the organism. In the head, four or five ganglia which are connected together by filaments; these are the ophthalmic ganglion which is situated in the orbit on the outer side of the optic nerve; from this ganglion which is in close connection with the third or motor nerve of the eye and also with the nasal branch of the fifth nerve, a sensory nerve, proceed various branches of distribution to the eyeball, under the name of ciliary nerves. This ganglion has a third root by which it is brought into connection with the superior cervical ganglion.

The other ganglia are the spheno-palatine, situated in the pterygo-maxillary fossa; like the lenticular ganglion, and indeed like every other sympathetic ganglion wherever found, it is connected with a cerebro-spinal nerve, viz. the superior maxillary or second division of the fifth pair, a sensory nerve, by two small branches, which join it from above. This ganglion distributes branches in a great variety of directions, but those which we describe as most nearly connected with our subject, are the *three palatines*, anterior, middle, and posterior — to the *mucous* membrane of the *hard and soft palate*, uvula, and to the *nasal mucous membrane;* the *spheno-palatine*—to the mucous membrane of the spongy bones and that of the septum. Also the Vidian or pterygoid branch, one branch of which enters the cranium and connects itself with a swelling on the portio dura, and another becomes a part of a plexus which surrounds the carotid artery, and finally is connected with the superior cervical ganglion.

The *Otic* ganglion is situated on the inner border of the foramen ovale near the inferior maxillary nerve. It is very plainly a sympathetic ganglion as shown by its structure, for in this we find the vesicles as well as the tubular and gelatinous fibres very distinct; indeed, it is often selected as a specimen for the microscope on this account. This ganglion is connected with the third division of the fifth nerve, which nerve is both *sensory* and motor in its function. This ganglion gives off various branches, some of which supply the cavity of the tympanum. It has also another branch which connects

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it with the glosso-pharyngeal, which is distributed to the glands, follicles, and mucous membrane of the tongue. The last one of the cranial ganglia which we will describe is the *submaxillary*, situated in the substance of the submaxillary gland; it is connected with the gustatory branch of the fifth nerve, a nerve of special sensation, and its branches are distributed to the submaxillary and sublingual glands and their ducts.

The *Cervical Ganglia* are three in number, called superior, middle, and inferior. The superior, situated at the upper part of the cervical portion of the column, is connected by large branches with the first, second, and third spinal nerves. It sends branches up into the carotid canal, which divide and subdivide in such a manner as to form a complete plexus around the artery, which is called the carotid or cavernous plexus. With this plexus, there are numerous communications; one which we particularly mention, which goes to assist in forming the *tympanic* plexus; so here again it will be perceived that the internal auditory apparatus receives a supply of sympathetic filaments. Branches also accompany the various ramifications of the internal carotid artery in its distribution within the cranium. Communications exist between the various parts of the eighth and ninth pairs and this ganglion, and also a large trunk connects it with the middle cervical ganglion. Its visceral and arterial branches are those which supply the *pharynx* by forming the *pharyngeal plexus* with the branches of the glosso-pharyngeal and vagus; also a few filaments to the larynx accompanying the laryngeal branch of the vagus. The most important branches from the second and third cervical ganglia are those large branches which, together with a smaller and more variable branch from the superior ganglion, serve to supply the *heart, principally*, with its innervation, under the name of superior, middle, and inferior cardiac nerves, which form plexuses under the name of cardiac, for the supply of this organ and of the *aorta*, some of them going to assist in forming the pulmonary plexus to supply the lungs.

The thoracic portion of the sympathetic system consists of a series of ganglia corresponding very nearly with the number of the vertebræ; these communicate freely with each other and also *with the roots of the spinal* nerves. They send branches to supply the aorta, which form the aortic plexus, and serve to constitute several large branches called the greater and lesser splanchnic nerves; they pierce the diaphragm, the former joining the great semilunar ganglion, and the latter the renal and aortic plexuses. The *lumbar and sacral*

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portions of the sympathetic are connected with the dorsal or thoracic portion, sometimes by a small intercommunicating cord between the last dorsal and first lumbar ganglion and sometimes by the lesser splanchnic nerve. They, like the above, have numerous and very distinct communications with spinal nerves, and form plexuses for the supply of some of the abdominal and of the pelvic viscera, together with the lower portion of the aorta under the name of inferior aortic plexus.

In addition to the above ganglia, which are usually denominated, from their location and close connection with the general nervous system, the spinal or vertebral sympathetic ganglia, there are others of not less, and by some authors considered of infinitely more importance; we refer to the *isolated* ganglia, situated within the thoracic and abdominal cavities, and which appear to have comparatively but little direct and intimate connection with the cerebro-spinal system or with the external parts of the organism, and to be devoted to the viscera entirely, and therefore are with their branches pre-eminently entitled to the denomination of *visceral ganglia* and *nerves*. These are found in various situations, several of them in connection with the various cardiac plexuses,* but the most important are found in the abdominal cavity forming the centres of the solar plexus, under the names of greater and lesser semilunar ganglia; from these pass off plexuses accompanying and corresponding in name with the various branches and sub-branches of the cœliac axis, and conducted by them into the organs, for which they are manifestly intended. Todd and Bowman thus describe these plexuses: "The nervous plexuses of the abdomen are extremely complicated and numerous. They are principally derived from two great ganglia, situated, one on either side of the cœliac axis, in front of the aorta. These ganglia are semilunar in shape, convex downwards and outwards; they unite below the cœliac axis; and chiefly from the convex border, a vast radiation of plexiform nerves takes place, which follow the course of and entwine around the branches of the cœliac axis, and other branches of the aorta. To this great radiator, anatomists have given the name of *solar plexus*, and the conjoined semilunar ganglia must be looked upon as the great centre—the *sun* of the abdominal sympathetic system."

* Several ganglia are found in connection with the nerves of the heart. Wrisberg describes one near the arch of the aorta, and Remak describes and figures several among the anterior and posterior cardiac plexuses, and in the substance of the heart.—*Todd and Bowman*, p. 505.

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From this solar plexus proceed subdivisions or smaller plexuses, corresponding with the arterial trunks in this region, as the diaphragmatic plexus, a small network accompanying the phrenic arteries, and the supra-renal arteries. 2d. The coronary or gastric plexus, which assist with the pneumogastric nerve in the supply of the stomach. "Of these filaments," says Cruveilhier, "some ramify upon the *cardia*, while the remainder follow the coronary artery along the lesser curvature of the stomach. *It follows, therefore, that the stomach is principally supplied by the pneumogastric nerve. The cardia and the lesser curvature of the stomach are the parts which are the most abundantly supplied with nerves.* The pylorus, to which we attribute such great sensibility, has incomparably fewer nerves."* To the liver there are sent, in great profusion, branches, from this great plexus, which constitute the *hepatic plexus*, the branches following the course of the arteries, and being conducted by them into every portion of the intimate tissue of the organ. For the spleen there is the splenic plexus, and so likewise the renal plexus for the kidneys; though this last, together with the inferior mesenteric and spermatic plexuses, are not so directly offsets from the solar plexus, being more distinct from it than those above described.

Lastly, as most intimately connected with our subject, we describe more fully the *superior mesenteric plexus*, which supplies the greater portion of the intestinal canal, accompanying the ramifications of the superior mesenteric artery to wherever these are distributed. Finding the most concise, and, at the same time, most pertinent description of this plexus in Cruveilhier's Anatomy, we prefer to use his words: "The superior mesenteric plexus, which may be regarded as the lower division of the bifurcation of the epigastric plexus, is the largest of all the abdominal plexuses; it surrounds the superior mesenteric artery, forming an extremely thick plexiform sheath for it; it passes below the pancreas, enters the substance of the mesentery with the artery, and divides, like that vessel, into a great number of secondary plexuses, which are distributed to all parts supplied by the artery, namely, the *whole* of the *small* intestines, *excepting the duodenum*,† and right portion of the great intestine.

One more point in relation to the anatomy of the nervous system in reference to this subject, and we have done; we refer to the very

* Treatise on Human Anatomy, p. 867, and marginal note.

† Dr. Todd, in the Cyclopædia of Anatomy and Physiology, says that the duodenum is supplied by the cerebro-spinal system of nerves.

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close connection that exists between the ganglionic system, and the pneumogastric or par vagum nerve; both in the substance of the lungs and in the superior cervical ganglion,* as well as in its relations to the semilunar ganglion, so close is this latter connection, that some authors have even regarded the whole nerve as originating from this ganglion,† a surmise which, we must admit, is altogether unfounded; but still, it serves to show the character of this connection, as well as in some measure the character of function attributed to this nerve.‡ Dr. John Bird, in his experiments on the nerve, found that its section produced, besides other symptoms, a peculiar congestion of the lungs, *resembling* pneumonia, which congestion was, no doubt, attributable to the cutting off from the lungs the supply from the sympathetic system. This, then, brings us to a consideration of the *functions of the sympathetic system.*

In the physiology of the nervous system, if there is one fact better established than another, it is, that the ganglionic system, whatever else may be its functions, presides over and controls the important act of circulation, secretion, and nutrition; or, at least, forms a necessary element in these functions. Anatomy, both special and transcendental, as well as vivisections and physiological experiments, all serve plainly and incontestably to demonstrate this fact in relation to its office. "Clinging to the coats of the arteries, it follows them, for the most part, in their ramifications, and attaches itself to them, as the ivy does to a tree." It seems to be nerve of the bloodvessels, and as, remark Todd and Bowman, "secretion is mainly dependent on the normal nutrition of glands, it is reasonable to suppose that that function would be, to a certain extent, controlled by these nerves." Dr. T. B. Proctor, in a very sensible treatise on the sympathetic nerve, says: "It is quite clear, from the important and interesting experiments made by Wilson Philip, Legallois, and Fleurens,

* Todd & Bowman, pp. 488 and 502, Physiological Anatomy, American edition.

† "I am perfectly aware," says Dr. James George Davey, of London, "that Dr. Stevens maintains both the vagus and spinal accessory nerves derive their origins not from the medullæ oblongata, *but from the upper part of the semilunar ganglion.* But I cannot think so."—*Vide* Ganglionic System, *London Lancet*, vol. for 1851.

‡ This nerve, from the resemblance in its functions to the sympathetic, has been called the lesser sympathetic nerve, and it is remarkable that, in that class of animals in which the sympathetic is said to be deficient, or nearly so, its place is supplied by the pneumogastric.

"In the cyclostomes among fishes," says Wagner, "the sympathetic is either wholly or in major part, replaced by the par vagum; the same thing occurs among serpents." —*Elements of Special Physiology*, p. 514.

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that neither the brain nor nerves of the spine have anything to do with the circulation of the blood; as it could be seen that animals lived for some time, and the circulation went on with vigor, after the brain and spine had been removed, separately, and also conjointly. It is self-evident, then, that it is *to the sympathetic (and to that alone) that we must look as the regulator of the arterial system.* And it will be observed that, in all parts of the animal body where large and sudden supplies of blood are required, such as the heart, stomach, bowels, and organs of generation, we have the sympathetic or ganglionic system very fully developed, and as far as I can judge, in ratio to the amount of blood supplied to these several organs. On the contrary, in some parts of the body, and in the extremities, where the flow of blood is more regular, and not subjected to those sudden calls for large supplies of blood at irregular periods, we find this nerve manifestly decreasing in size; and, indeed, as far as we can judge with the naked eye, ceasing altogether in some parts. Still, I perfectly agree with Sir Charles Bell, *that it is distributed all over the body;* but whether its influence is confined to regulating the small vessels which supply the coats of the arteries, or whether the same influence is continued by it over the whole circulating medium of the extremities and other parts that it manifestly exerts over the abdominal viscera, must, I fear, be left to a more enlarged inquiry."* Dr. Carpenter, who believes somewhat differently from the above, and, with most physiologists of the present day, namely; that the sympathetic nerve derives whatever of sensory and motor power it possesses from the anterior and posterior column of the spinal cord, expresses himself in the following apposite manner in regard to the functions of the sympathetic as a whole. "It seems fair to conclude that the motor power of the sympathetic system—which is chiefly exercised on the *muscular substance of the heart* and *walls* of the bloodvessels, on the *muscular coats of the alimentary canal*, and of the large gland-ducts which open into it, and on the muscular walls of the genito-urinary organs—is entirely derived from the cerebro-spinal system. In no instance, however, can the will† exert any in-

* Medico-Chirurgical Review, Jan. 1845, also Treatise, separate, p. 21.

† We do not wish to deny this assertion; but there is on record one familiar case, which would appear at first sight strongly contradictory of this statement, that of Col. Townsend, who is said to have had such entire control over the action of his heart that he could make it cease to beat at any time, and virtually die, and live again, so far as the circulation was concerned, at will; but, on one occasion, having

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fluence over the movements of these parts; they are strongly affected by emotional states of the mind, and they frequently seem to respond to impressions made on remote organs. If, then, the sensori-motor endowments of the sympathetic trunks be restricted to those fibres which are really cerebro-spinal in their origin or termination, it remains to inquire what are the functions of the *true* sympathetic fibres, whose vesicular centres lie in the ganglia of the sympathetic system. Upon this point, we can only surmise; but there appears strong grounds for the conclusion, *that the office of these fibres is to produce a direct influence upon the chemico-vital processes concerned* in the *organic functions* of *nutrition*, *secretion*, &c.; an influence which, although not essential to the performance of each separate act, may yet be required to harmonize them all together, and to bring them into connection with mental states."*

Sir Charles Bell's† opinion is somewhat similar. "We are left to the conjecture that the sympathetic or ganglionic system of nerves, according to Bichat, are for those thousand secret operations of a living body, which may be called constitutional: *circulation*, *secretion*, and *absorption*, are operations which simultaneously affect the entire frame."

From the above, then, it appears that the sympathetic system, as a whole, being composed of intrinsic gelatinous fibres and cerebro-spinal or tubular fibres, has two distinct and separate offices to perform in the animal economy, viz. 1st, by virtue of its cerebro-spinal or tubular fibres, to preside over the movements and sensations (what little they possess) of the *heart* and other involuntary muscles, as the muscular coats of the *intestines*, *bladder*, *and the coats of the various excretory ducts; and that it is the source of intrinsic motory power to the bloodvessels, these depending upon this system for their innervation;* and, secondly, that by its vesicular and gelatinous portions, it presides over the chemico-vital operations of the animal organism,

willed himself to death for the amusement of some friend, he carried the experiment too far, and life became extinct.

This peculiarity was probably due to an unusually large supply of filaments from the pneumogastric and spinal nerves, as is very frequently observed in dissections, or as we would now suggest, it was attributable to disease of those fibres of the sympathetic which belong essentially to it, and of the ganglia, by which disease those parts had lost their *isolating power*, so that the cerebro-spinal influence, over which the *will* presides, was transmitted unembarrassed to the heart. This case certainly should not detract from the force of Dr. Carpenter's remarks.

* Principles of Human Physiology, 1853, p. 830, *et seq.*

† On the Nerves, p. 11.

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concerned in *digestion*, *secretion*, *absorption*, &c. &c. In a few words, then, it presides over the functions of all the involuntary muscles, and controls circulation and secretion.

PATHOLOGICAL DEDUCTIONS.

Having thus carefully recounted the more important features in the anatomy and physiology of the sympathetic system, it remains but to consider them in connection with the phenomena of typhoidal fevers. Such a review will be attended with the following results. In the first place, the essential symptoms of typhoid fever are located in organs deriving their innervation principally, and in many instances, entirely from the ganglionic system: In the organic or involuntary muscles—as for instance the *heart's*—of which, during life, we find the frequency increased, the force diminished, and the regularity impaired—all of which effects must be plainly attributable to the *altered innervation* of the organ. After death, we have seen it the subject of very material alteration; its substance is flabby, pale, and much softened, so that it breaks readily under the fingers. Instance again, the *muscular coat* of the intestine; we have *meteorism*—an almost invariable symptom in typhoid fever—and which we may legitimately refer to the loss of *tonicity* in the muscular coat of the intestinal canal, from impaired innervation of that coat, by which condition, together with the altered state of the secretory surfaces, the passive accumulation of gas in the intestines is allowed, and hence the tympanites.

That this altered condition in the innervation of the organic muscular fibre does exist, is shown most remarkably in the mode of dying in some cases, viz., that mode termed *asthenia*, "occasioned by causes acting directly on the circulatory forces, affecting the *vis nervosa*, upon which the contractile property of the heart depends,"* and farther, that this depression in the involuntary muscles has no invariable correspondence in the state of the voluntary muscular system; as we shall find remarkably illustrated in the observations of Dr. Flint.† "In some of the cases attended with most danger, and some of them ending fatally, the *muscular strength was retained in a surprising degree.* In two fatal cases of the typhoid type, characterized by active persistent delirium, the muscular efforts were almost constant and quite strong up to a few hours before death. One of these cases

* Flint, p. 125.

† Reports on Continued Fever, p. 59.

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terminated on the ninth day, and the other on the third day after coming under observation. The mode of dying in each was by *asthenia*, or, perhaps, more properly, *necræmia; the system of involuntary muscles exhibiting reduction of force to a degree incompatible with life—the voluntary muscles remaining active.* This is a curious fact."

This relative condition of the voluntary and organic muscular systems, appears to impress even the observing and philosophic mind of Dr. Flint as almost inexplicable; and it is not surprising that it should, when we consider that his views of the pathology of typhoid fever have no fixed or definite reference to the organic system of nerves; but, on the admission of the ganglionic pathology of the disease, the full interpretation of these phenomena, besides many other similar facts (meteorism) before inexplicable, become easy and natural.

II. Besides the organic muscular system, which we have just shown to be under the influence of ganglionic nervous aberration, we find that the other characteristic phenomena of typhoid fever refer to the functions of nutrition and secretion, both of which important processes depend upon the vascular system, which, especially in the viscera, are admitted to be under the sole dominion of the sympathetic nerve. And, what is more remarkably illustrative of this fact, is, that there appears to be a very close relation between the amount of disease observed in any particular portion of the organism —the alimentary canal for instance—and the degree to which it is indebted to the ganglionic system for its innervation; thus we find but a small amount of disease, congestion, seldom any ulceration, in the *larynx;* ulceration is somewhat more common in the pharynx, œsophagus, and stomach, though still not abundant. It disappears in the *duodenum*, which receives but few sympathetic filaments, and again appears in the upper portion of the *ileum*, increasing, as we descend, *in direct proportion* to the amount of ganglionic fibres the part receives, till it reaches its maximum in the lower portion, where the nervous supply is very abundant; after which, we find ulceration *occasionally* in the cœcum, still less frequent in the colon, till in the *rectum*, whose innervation is principally from the cerebro-spinal system, it is never observed. So, likewise, with regard to the other organs; we find the *liver*, *lungs*, and *spleen* are all subject to congestions, which can be referred to the same abnormal innervation of these viscera.

From the relative unfrequency of disease in certain portions of

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the abdominal viscera, and elsewhere, Louis, as we have seen, though admitting their diagnostic importance, is disposed to view them as results, secondary to the lesion in the ileum; we cannot, however, agree with him, but are compelled to regard them as the *common primary results* of a *common cause* which exists in the ganglionic system, and that the frequency or the gravity of disease in any one of these organs is determined alone by the amount to which the *ganglionic ingredient* mixes with, or enters into its innervation, and that disease in these localities has no etiological reference whatever to that in the ileum; but, when it exists, is as significant of the true pathology as is the ileitis—for it invariably indicates, both by its location and character, that its origin is *abnormal innervation.*

So far then as regards the *localities* in which the manifestations of typhoid fever occur, we have found an exact correspondence with the distribution of the sympathetic nerve, as likewise between the *amount* of disease and the proportion of this kind of innervation in any given parts. Now it will be our object to examine carefully, in order to ascertain if there is any analogy between the *character* of these typhoidal phenomena, and of those results which have been obtained by experiment upon this system of nerves. In this interesting department of physiological inquiry, there have been many engaged, but a few will answer very well the purposes of our comparison. As early as the year 1732, Pomfour du Petit found that the *division* of the trunk of the sympathetic, opposite the fourth or fifth cervical vertebra in dogs, was very rapidly followed by great disturbance in the circulation of the eyeball, producing inflammation, flattening of the cornea, retraction of the eyeball, with protrusion of a fold of the conjunctiva and a *flow of tears*, and ultimately ulceration and destruction of the organ. The experiments of Dupuy upon horses, wherein he *extirpated* the superior cervical ganglion, were followed by the same results with regard to the local effect in the eye, but also, with the more opposite and corroborative consequences, that there was *an eruption over the whole cutaneous surface*, with emaciation and an œdematous state of the limbs. Dr. John Reid, has also experimented on the sympathetic nerve in the neck, and found the eye similarly affected with the above, the *conjunctiva becoming red* and *congested* in a few minutes, while in other experiments* the eye presented an *ecchymosed* or *bloodshotten appearance.* Each one of these conditions of the eye must be borne in mind, in

* Arneman's Experiments on Nerves. Gottingen, 1787.

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order to appreciate the comparison; inasmuch as, on account of the great difficulty of making such experiments on other portions of the sympathetic system, we can find none on record which will serve as reference; for it will readily appear that, from the remote position of these nerves, it is impossible to make their section without so materially deranging other important parts of the organism as to render the results valueless in deduction.

Now, a reference to some of the pathological phenomena of typhoid fever, will discover a close analogy, if not identity to the above results; in the first place, the conjunctival congestion; its character, the attendant suffusion, together with the entire freedom from pain, even on exposure to the strongest light; while, at the same time none of the symptoms of true inflammation are present; all indicate the seat of the nervous derangement upon which it depends to be the ganglionic system* and not the cerebro-spinal, the analogous derangements of which are invariably of a sthenic character, and attended with acute pain in the region in which they occur. Again, an attentive consideration of the character of these congestions will show that it does not vary in any respect, except in degree, whether occurring in the mucous membrane of the eye, that of the stomach, pharynx, small intestine, large intestine, or bladder, in the typhoid type, or on the cutaneous surface in the typhus. In all the above localities, and under all circumstances, we find the capillary congestions wearing the same aspect, assuming invariably a *passive* character, often approaching the condition of true stasis, but never attended with the florid redness, the pain or the swelling of active inflammation. Lastly, in the cutaneous petechial eruptions or maculæ of the typhous type of continued fever, we can also detect the same character of *passive congestion* from deficiency of nervous energy carried to a still greater degree; in this type, the nervous power of the cutaneous capillaries is so far diminished, that it amounts to a state of actual paralysis, allowing such distension of the capillaries that their rupture and a subcutaneous effusion is the result.

We have thus far endeavoured to show that typhoidal fevers result from alterations in the condition of the ganglionic nervous system; first, by comparing the typhoid phenomena with the normal action

* Dr. A. Billing remarks, in relation to this subject: "Without, therefore, at present seeking for farther proofs, I deduce from blushing, and from the effects of electricity, fire, and cantharides, that the capillaries are dependent upon the nervous system for that tone or energy which preserves them from over-distension."—*The First Principles of Medicine*, p. 44.

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of the sympathetic system, and we have found that the analogy is complete, and that typhoid phenomena are but the result of aberrations in the normal action of these nerves. Their action may be either exaggerated or diminished; for instance, a portion of this system controls the action of the heart, and in health endows this organ with a frequency of action amounting to 60 beats per minute in the adult; and without disturbing causes, this number will continue unvaryingly in its regularity till the close of life. But we know that this regularity is liable to many interruptions and disturbances; some of them but momentary, as from the emotions; others disturb it for many hours by increasing its frequency, as in paroxysmal fever; and lastly, in typhoid fever, we find this increase of frequency kept up for many weeks, but still retaining the remarkable feature of continuousness, which distinguishes the normal action of the sympathetic system from that of the cerebro-spinal system, which is intermittent in all its phenomena, whether normal or abnormal. Were we now requested to explain the difference which marks the increased frequency of pulse in these three instances; to answer why is it evanescent in one case; of but a few hours' duration in the second; and yet continue many weeks in the case of typhoid fever?—we think, we should do it thus: The heart, being under the dominion of the ganglionic system, performs its normal action through its influence; but from the intimate connection between the sympathetic and cerebro-spinal systems, especially in this organ, it is very liable to be affected by emotional causes acting in the brain; these, of course, will be evanescent; or it may be affected by causes acting in the spinal marrow, which may be more durable, as would be the case in a paroxysm of intermittent fever;* but it will be observed that, in these cases, the organic system is only secondarily implicated, and so soon as the mental emotion subsides, or the spinal irritation is removed or has exhausted itself in a paroxysm, the excitor being withdrawn, the sympathetic becomes again normal, and the action of the heart consequently natural. But in continued fever, the case is quite different; the irritation is now located in the ganglionic centre itself, which supplies the heart, and consequently the increased frequency

* We have adopted the pathology of intermittent fever advocated by Maillot, viz. that it consists in cerebro-spinal intermittent irritations. Prof. J. F. Lobstein remarks: "The paroxysms of intermittent fever are tied down to a regular rhythmus, in consequence of being radicated in the nervous system, upon which nature has impressed a law according to which they must perform their functions periodically.—*Sympathetic Nerve*, p. 121.

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continues as long as this irritation remains, which is coeval with the duration of the disease. There is one feature of continued fever, to which, heretofore, we have seen no satisfactory explanation that we think can be accounted for rationally according to the pathology here suggested; we refer to the paroxysms and excerbations which frequently complicate the course of continued fever. We would attribute these paroxysms to the extension of the irritation from the ganglionic to the cerebro-spinal centres, and we conceive that it is only under such circumstances that we find these intercurrent paroxysms masking the course of the typhoid affection. By this means there is effected a true *blending of types* from a blending of proximate causes, and the two sets of phenomena exist in combination; a continued fever characterized by paroxysms of exacerbation. In the same manner, on the other hand, can we conceive of paroxysmal fever becoming continued under favourable circumstances, by the transmission of irritation from the cerebro-spinal to the ganglionic system.

As the abnormal innervation in the sympathetic system can produce a continued accelerated action in the heart, in typhus and typhoid fever, so, likewise, can the diarrhœa of the typhoid type be shown to be a result probably of a similar condition. The normal action of the ganglionic system endows the intestinal canal and other viscera with the function of healthy secretion; but during the existence of the typhoid state this proper action is destroyed; in place of the organs being the seat of a normal and proper circulation, which is necessary for the due exercise of their function—secretion, the vessels become the seat of obstructions and congestions, the secretions become more abundant, but abnormal and vitiated; we have then *diarrhœa*. As these congestions become more marked, we find the paralyzed and over-distended vessels giving way, allowing submembranous effusions of blood, and occasionally considerable hemorrhages—all being the result of the altered condition of the innervation of these organs. Thus, what in its beginning was a purely dynamical affection, soon becomes organic; for without the proper supply of ganglionic nervous influence, we have seen, from the above-mentioned experiments, that the circulation ceases, the capillaries become turgescent, especially in highly vascular secretory organs (as the mucous surfaces, or the glandular plates of Peyer, for instance), effusion of lymph takes place, and, as above stated, finally, the capillaries are ruptured, and the tissues in certain places rapidly disintegrate by the process of ulceration—which is the actual condition of things in the intestinal disease of typhoid fever.

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From what has already been said in relation to the distinctive features of the two types of continued fever—typhoid and typhus, their interpretation, according to the pathology herein argued, will have been, doubtless ere this, naturally suggested. We have seen that *typhoid* fever is marked by an accelerated pulse, more or less nervous derangement, an altered condition of the blood, frequently a *mild* cutaneous eruption, diarrhœa, and meteorism. On *post-mortem* examination, we find lesions; viz. congestions and ulcerations of a peculiar character, that is, simulating those produced by experimental sections of the nerves in all those parts supplied by the *visceral* portions of the ganglionic system of nerves. On the other hand, we have seen that *typhus* fever is characterized by a somewhat more accelerated pulse, much more marked nervous derangement, the same altered condition of the blood, well-marked and *always serious* alterations in the capillary circulation of the *skin*, amounting often to actual ecchymosis, while the *post-mortem* examination shows almost entire *exemption* from lesion in the abdominal viscera. Thus, we find that while both diseases have all their general symptoms so exactly similar that we are forced to acknowledge their identity, and see in them what is essentially but one disease, yet typhoid fever has its principal and most prominent manifestations in the abdominal viscera —internally; and typhus fever manifests itself in aberrations of the circulation, very analogous to those of typhoid, but occurring in the capillaries of the *cutaneous* surface.

Now the ganglionic pathology is the only theory by which such marked incongruities in the two forms of what, to the observation and scrutiny of every one, must ever appear as one disease, can be perfectly and satisfactorily reconciled. We cannot deny that the two are but types of the same disease; yet how incongruous and strange it is that, in certain cases (*typhoid*), diarrhœa and intestinal lesion should appear the main—the most important features; while in the other cases (*typhus*), undeniably of the *same disease*, even more grave and threatening, there should be not a trace of diarrhœa, and on *post-mortem* examination no intestinal lesion whatever, but, instead, serious disease and congestion of the *skin*, with subcutaneous sanguineous effusions, similar to the submembranous sanguineous effusions of the typhoid type. Indeed, from the very smallest degree of attention must result the conclusion that, in the two cases, *the disease is one and the same*, but seated in different portions of the organism; and this conclusion will accord exactly with what we have considered the distinctive pathology of the two forms, viz.

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that in each type the disease is located in a different portion of the ganglionic system. There are certain parts of the system which are affected in both forms of the disease—as the ganglia supplying the heart; but after this, the two types have entirely distinct and separate sets of ganglia, the morbid action in which gives rise to their respective manifestations. In *typhoid fever*, the *internal* or *visceral* isolated ganglia, such as the semilunar, &c., are the seat of the morbid action; and these supplying mainly, we may say entirely, the abdominal viscera, and having but little connection with the other or external portions of the organism, these viscera become the seat of nearly all the morbid phenomena; while that little implication of the cutaneous surface and general nervous system, which we often observe, is entirely due to the remote and very obscure connection which their isolated ganglia may have with the nerves supplying these parts.

Now, in the *typhus* type, the same disease, or morbific agent (its exact nature we do not pretend to define), affects an entirely different set of nervous centres—a set of ganglia which, by their anatomical position, their internal and universal relations with the anterior and posterior roots of the spinal nerves, are plainly destined to preside over the capillary circulation of parts more superficial—the cutaneous surface. We have reference to the *vertebral* sympathetic ganglia; and, in attributing the location of *typhus* fever to these ganglia, we have a ready and satisfactory interpretation of all its distinctive characteristics. The skin becomes congested and ecchymosed (petechial), because its circulation is dependent upon and controlled by innervation derived from these vertebral ganglia; which ganglia being the seat of abnormal action (perhaps paralysis), innervation is deficient, the cutaneous circulation is retarded; in certain places there is obstruction, with actual rupture and effusion, giving rise to petechiæ. The general (cerebro-spinal) system is more seriously involved in typhus than in typhoid fever, because the connection is more direct between the vertebral ganglia—which are the seat of typhus—and the cerebro-spinal system. In a word, then, we would locate *typhoid fever* in the *visceral* portion of the ganglionic system, and *typhus* in the *vertebral* portion.

We are fully aware that our views of the pathology of typhoidal fevers would be greatly corroborated, could there be discovered any appreciable *lesion* in the ganglionic nervous centres, in subjects who have died during their progress; but, like the pathological anatomy of all the nervous system, this would be an investigation attended with

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many difficulties; for histological changes in the nervous centres are of such a character that though they may be competent to subvert the intellect, entirely paralyze or destroy the functions of a large portion of the organism, and ultimately, upon the most positive rational evidence, *seem* to be the cause of death, yet on examination the alterations observable in those centres are of the most insignificant and irrelevant character, pertaining only to the involucre, while the centres themselves, which, from the previous symptoms, had been plainly the true seat of the disease, have been found apparently normal and intact. These changes, then, are probably molecular and inappreciable with our present means of investigation, and will require years, and far more perfect appliances, to incorporate them among the positive and demonstrable things of our science. Still, there are occasionally isolated facts, even in the *pathology* of the ganglionic nervous centres, which we may refer to in corroboration of our opinion that these centres are affected in typhoidal fevers, and that such affection gives rise to its characteristic symptoms, or to phenomena quite analogous in their nature. The case reported by Professor Lobstein is of this nature. It was that of a young girl who had suffered from paralysis of the lower extremities for some time, but for three months previous to her death laboured under the most incurable *diarrhœa* with tormina, &c. On making a *post-mortem* examination, there was found a large abscess on the left side, extending from the sixth to the tenth dorsal vertebra. On opening this abscess, it was found that the *trunk* of the left *sympathetic nerve* was entirely *destroyed* from the sixth to the twelfth vertebra, *and in the lumbar* portion the *same nerve was in a state of inflammation.**

There are also two cases reported as occurring in the practice of Dr. Aronsshon, of the Strasburg Hospital: The first, that of a man forty-seven years old, affected with *diarrhœa.* He died of a tumour in the abdomen, and it was found that "the *semilunar ganglia* were affected with *distinct inflammation.*" The second case was a woman aged about thirty years, who, in her second pregnancy, was subjected to vomiting throughout the whole of her gestation. She was also affected with a *furfuraceous eruption* upon the breast and arms, with *swelling* of the *limbs*, and with *diarrhœa.* On a *post-mortem* examination, the villous coat of the stomach was found inflamed and thicker than usual, especially towards the pylorus, "and the semilunar ganglia were found in a *state of genuine inflammation.*"

* Structure, Functions, and Diseases of the Sympathetic Nerve, p. 147.

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"In the body of a boy ten years of age," says Lobstein, "who had died from the retrocession of a *miliary eruption*, attended with symptoms of great anxiety, oppression of the chest, and distension of the epigastrium, I found a place in the left trunk of the *intercostal* (which is the old name for the sympathetic) *nerve, highly inflamed* between the eighth and tenth ribs, *with a phlogosis of the ninth and tenth thoracic ganglia*, and their two anastomotic branches with the costal nerves."

The following observations are quite pertinent to the state of congestion in which the lungs are almost invariably found, to a more or less degree, in typhoid and typhus fever. "On examining into the condition of the nerves in diseases of the lungs, I discovered another alteration which is peculiar to these organs; to wit, in that species of *peripneumony*, in which the lungs became red and *slightly indurated*,* and which, I think, should be called spleenification; the *nervous filaments* attending the ramifications of the bronchia *were* found equally *red*, a little more tumid, but much more tender than usual; so as to be *broken by the slightest degree of force*."† And, lastly, the same author‡ quotes a case still more in point, from the writings of Professor Autenrieth, of Tubingen, wherein it is asserted, though he does not appear to connect the circumstance with the pathology of the disease at all, that he has *seen* the *abdominal nerves of the ganglionic system somewhat changed in subjects who have died of typhoid fever.*

The above cases, though not conclusive, are at least strongly corroborative of our view of the pathology of typhoid fever; for while we must admit that the diarrhœa, the eruption, the pulmonary congestion, œdema, &c., might have been produced by other causes than disease of the ganglia found inflamed or divided, still, when these coincidental circumstances are viewed in connection with the known result of artificial section of accessible portions of the sympathetic nerve, as those about the neck and eye,§ and also in view of the entire dearth of experiments and facts bearing upon this portion of the ganglionic system, we must regard them as significant and valuable, even though they afford what we may term only a legitimate suspicion of the correctness of our pathological views.

* Exactly what is described by Dr. Flint as the pseudo-pneumotis of continued fever.

† Lobstein, p. 189. ‡ *Op. cit.* p. 137.

§ See experiments of Panfour du Petit, Dupuy, John Reid, and others, already referred to.

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We have now completed our investigations in relation to this intricate, but, at the same time, most interesting topic of pathological inquiry; we have reviewed its history, and collected from every source within our reach as complete a delineation of its prominent and characteristic phenomena as has been necessary for their full development. In so doing, we have been struck with the vast number of reliable and significant facts our science is in possession of, in regard to this disease. No disease in the catalogue is more invariable in its characteristic manifestations; no disease has been more diligently studied, or has enlisted in its investigation such faithful observers. Our knowledge in regard to its observed *phenomena* and *facts* is clear, well defined, almost certain; to complete the science in regard to it, it has but remained that these cognate, well-ascertained facts and phenomena be rationally and correctly *interpreted*, that its true pathology might be *deduced*. To this arduous, though not unpleasing task, not without many misgivings, we have earnestly and diligently devoted ourselves, more with the hope that our labours would prove suggestive to others of the true mode of arriving at the real pathology, than that we should be able to supply the want or remedy the deficiency.

Starting with what we considered the rational assumption that the pathology of typhoidal fevers is in the ganglionic system of nerves, we have compared their characteristic traits and phenomena with, first, the *normal* action of this portion of the nervous system, then with the known and well-established results of *experimental irritation and action* of various portions of these nerves, and we have found that the analogy is sufficiently close to admit the legitimate inference that the symptoms and pathological lesions of typhoid and typhus fever are produced by a normal action in certain portions of this system of nerves. First, because no typhoid or typhus phenomenon ever occurs, except in regions supplied by this system; secondly, because the peculiar phenomena of these diseases occur in a more marked degree, in those parts most abundantly supplied from this source; and thirdly, because the nature of these symptoms are always found more purely and characteristically typhoid in those portions of the organism supplied exclusively by this kind of innervation. And farther, on the other hand, we are forced to admit the truth of these impressions, because we have hitherto had no theory or legitimate and consonant combination of theories, to our own mind, as competent to the full and rational explanation of *all* the phenomena of the disease as the one now offered.

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PRACTICAL DEDUCTIONS.—Pathology is only valuable when it has a tendency to the prevention, amelioration, or cure of disease, and the results of our most successful labours in this department are but nugatory unless in them can be found a clue to a more rational and perfect management of the affections to which they refer.

If the views embodied in the foregoing treatise are correct, the following practical inferences must present themselves as legitimate, if not inevitable: First, that in the management of this class of fevers the strictest attention should be paid to the improvement of the tone of the nervous system; all depressing measures, or such as are calculated to exhaust the nervous energies, as depletion by purgation or otherwise, should be scrupulously avoided; and secondly, that in their place means of an opposite character should be invoked. Indeed, that treatment now most in favour, though but empirically* applied, will be found on the admission of the above pathology the most rational, and to offer the best hope of success. We have reference to that treatment which is addressed almost exclusively to the nervous system, and has for its object the sustentation and elevation of its energies. Some of the means employed have been attended in their application with the most marked beneficial results. Among these, we would mention the plan of Dr. Percival, wherein the disease was treated by frequent profuse cold affusions, especially in the case of children; which treatment we should rationally expect, from the known effect of cold water thus applied, to improve the condition of the depressed nervous centres. The administration of stimulants, as camphor, quinia, brandy, opium, turpentine, and the ethers, have all been favourably known to the profession as remarkably efficient in these fevers.

In relation to the beneficial results recently obtained from large doses of quinia, by Dr. Dundas and others, we can readily appreciate what vast benefit may accrue from them, especially in cases where the cerebro-spinal system of nerves are extensively implicated, and where the disease is marked by regular obstinate paroxysms, for quinia, though it possesses, in our opinion, but little influence over the ganglionic system itself, still, would relieve these periodic exacerbations by its effects upon this system, through the cerebro-spinal nerves (which in these cases we regard as the instigator of the paroxysm), upon which most of its power is expended. That the doses required should be large, we can easily understand, as any

* Of course, this term is not applied in its offensive sense.

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effect produced upon the ganglionic system through the cerebro-spinal is only accomplished by powerful and long-continued impressions, on account of the comparative isolation of the two systems from each other.

To those who are in the proper field for such experiments, and possessed of the proper facilities, we would recommend the trial, in typhoid and typhus fevers, of such agents as are known to possess a direct power to stimulate the nervous system, even when in a state of partial paralysis—such an agent is *strychnia*. This we would suggest as applicable in minute but efficient doses, with the view of waking up and restoring the diminished energy of innervation, upon which the impaired function depends, in the same manner that we would advise it in other similar cases where the cerebro-spinal system was implicated.

The above is all we offer in regard to treatment; there are many measures of a like nature, which, were we writing a complete treatise on the *management* of these diseases, would require a full and extended consideration, as also the measures and applications which the emergency of each case will naturally suggest and demand.

H. F. CAMPBELL,
C. T. QUINTARD,
ROBERT CAMPBELL,
Committee.

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ON

COXALGIA, OR HIP DISEASE.

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ON

COXALGIA, OR HIP DISEASE.

BY ALDEN MARCH, M. D.,

OF ALBANY, NEW YORK.

THE views which I am about to present concerning the pathological condition of the hip-joint, when occupied by the affection commonly called "hip disease," and its treatment, I am fully aware will differ widely from the commonly expressed opinion of almost all surgical writers and teachers.

Nearly every surgical author *assumes* that *spontaneous dislocation* of the hip is not unfrequently the result of *absorption*, *ulceration*, or *destruction* of the ligaments of the joint and of the acetabulum, and of contraction of the muscles surrounding the joint.

I shall take the position that *spontaneous dislocation* of the *hip* (as *purely* the *result of morbid action unaided by superadded violence*) *seldom* or *never takes place*. I also propose to point out a mode of treatment by which *progressive absorption* of the acetabulum and head of the bone may be arrested, before the life of the patient is endangered by the progress of the disease.

About the year 1845 or 1846, I was led to entertain views upon this subject, as I believe, nearly if not quite peculiar to myself, from the examination of the bony specimens of the hip-joint, of two or three persons, who had evidently died while labouring under a severe form of hip disease.

After making this examination, I came to the conclusion that all the usual symptoms of a dislocation, such as are commonly relied on, during life, as diagnostic of an idiopathic dislocation, could be accounted for, and satisfactorily explained, at least to my mind, in the strongly marked organic changes in the form and relations of the

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head of the femur and acetabulum, without the existence of such an accident.

If it should be asked, upon what ground I found the proof and argument to sustain my doctrine; and why I dare oppose the weight of the highest surgical authority on this subject: I will answer, that my convictions are based upon actual observation and personal examination of about forty pathological museums in this country and in Europe.

Of the pathological museums in the United States, I have examined *three* in the city of New York—*three* in Philadelphia—*one* in Baltimore—*one* in Washington—*two* in Boston—*one* in Richmond, Va.—*one* in Hanover, N. H.—*one* in Cincinnati—and *two* in Albany, N. Y.

In the year 1848, I visited and examined the following in Europe, viz.: Pathological Museum of Paris—London University Hospital Museum—Guy's Hospital Museum—London Hospital Museum—St. Thomas's Hospital Museum—St. Bartholomew's Hospital Museum—Middlesex Hospital Museum—St. George's Hospital Museum—College of Surgeons' Museum—King's College Museum—Edinburgh Royal Infirmary Museum—Royal College of Surgeons' Museum—Edinburgh University College Museum—Glasgow Hunterian Museum—Belfast College of Surgeons' Museum—Stevens's Hospital Museum, Dublin—Richmond Hospital Museum, Dublin—Dublin College of Surgeons' Museum—Park Street School of Medicine Museum, Dublin—University of Leyden (Holland) Pathological Museum—Amsterdam Pathological Museum—Heidelberg Pathological Museum—Friburg (Germany) Pathological Museum—Berne (Switzerland) Museum of Natural History—Zurich Pathological Museum—Brussels (Belgium) Anatomical Museum.

It will appear by the above catalogue, that I have visited and made personal examination of over forty different anatomical and pathological museums; nor can it be said that I visited these places out of mere curiosity, and without having any specific object in view.

I made it my business to examine critically every morbid specimen of the hip-joint, and with paper and pencil in hand, made a record on the spot of the number of specimens, whether dried or wet; and classified them according to the nature of their respective organic changes.

From the examination of the collections of the above institutions, I have had an opportunity of personally examining about two hundred specimens of what I took to be "hip disease," and as I learned to be

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such, from observation and the evidence of the curators and of the catalogues of the museums.

To insure a ready comprehension and proper appreciation of the pathological views I propose to explain and illustrate by specimens, I shall be under the necessity of presenting a brief description of the most important anatomical structure of the hip-joint; dry and uninteresting as such a matter-of-fact statement must be to the veteran surgeon and general practitioner.

The osseous structure of the hip-joint is made up of the *os innominatum* and head of the *os femoris*, which is received into a deep cavity of the former, called the acetabulum, or cotyloid cavity, by that species of articulation denominated *enarthrosis*, or ball-and-socket joint.

The transverse and vertical diameter at the brim of the acetabulum, in the dried skeleton, varies, in the adult male, from an inch and seven-eighths to two inches; and in depth from eleven-twelfths of an inch to an inch. The diameter of the female acetabulum varies from an inch and five-eighths to an inch and seven-eighths. The depth being but little less than that of the male. These measurements must vary, of course, according to the different periods of life at which they are taken.

The texture of the head of the bone, and of the innominatum, is a loose, cancellous material; quite vascular, and subject to inflammation.

The acetabulum is lined with cartilage at all parts, except at a circular pit (fundus acetabuli), which is situated at the lower part of the general cavity near the notch. The cartilaginous articulating surface of the head of the bone forms something more than half a segment of a sphere; and upon its face that looks towards the cavity of the pelvis, there is a deep pit into which is inserted the ligamentum teres.

The appropriate ligaments of the hip-joint, are the *capsular*, the *ilio-femoral*, the *teres*, the *cotyloid*, and the *transverse*.

The *cotyloid* ligament is a prismoidal cord of fibro-cartilage, which surmounts and is attached to the brim of the acetabulum, by which the latter is deepened.

The *capsular* ligament is attached to the outer border of the acetabulum and outer face of the cotyloid ligament above; below, it is fixed into the inter-trochantral line and neck of the bone. It is much thicker, and more extensively attached at its upper part, where strength and security seem to be needed.

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The *ilio-femoral* ligament is accessory to the capsular, and extends from the anterior inferior spinous process of the ilium to the anterior inter-trochantral line, by which the capsular ligament is greatly strengthened in that direction.

The *ligamentum teres* is attached by a round apex to the depression just below the middle of the head of the femur, and by its base into the borders of the notch of the acetabulum. It is covered by the synovial membrane.

The *transverse* ligament is continuous with the cotyloid, extending from one point of the notch to the other, completing the circle of the cotyloid ligament, and converting the notch into a foramen, through which the bloodvessels enter to supply the interior of the joint.

The synovial membrane is quite extensive, lining the capsular ligament, the free surface of the cotyloid, the ligamentum teres, and the cartilages of the head of the bone.

The muscles about the hip-joint, the active agents in producing distortion, are the flexors, adductors, and rotators.

I believe all authors describe two principal forms of morbus coxarius, or hip-disease; one, in which the inflammation commences in the cartilage; and the other, when the cancellous structure of the bone is the primary seat of the affection. In either case, the result may involve all parts of the joint, and prove equally destructive. That form of it in which the cartilage is the first part attacked, is not unfrequently met with in adults; whereas the scrofulous form, the other variety, is mostly confined to children, and persons under the age of puberty.

The hip-joint may be subject to other forms or varieties of disease: rheumatic inflammation, interstitial absorption, porcelaneous deposit, etc. But it is believed that the changes of structure consequent upon these affections do not often simulate or counterfeit the symptoms of dislocation. I shall, therefore, confine my remarks mostly to the subject of *ulcerative absorption*, by which such changes have resulted in the articulation as have too often been called *dislocation*.

I presume it will hardly be denied that surgical authors, and even the most experienced practical men, support the doctrine of spontaneous luxation of the head of the os femoris as a result of "hip disease." And yet, to prove this point, it is deemed important to make brief extracts and quotations from such surgical works as are contained in my own library, which are all of modern date, as well as to refer to those of a more remote period.

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I shall not confine myself to any chronological order in my quotations. It will be sufficient if it be shown that the same opinions are entertained, whether they have been borrowed from one or another, whether from a predecessor or contemporary; or whether the erroneous notion of dislocation (as I have deemed proper to call it) has been derived from the mere observation of the symptoms in any given case during life.

In the work entitled *The Principles and Practice of Modern Surgery*, by Robert Druitt, will be found the following, under the head of Diseases of the Hip-Joint: "But if the disease proceed, it is succeeded by another kind of shortening, caused either by the destruction of the neck of the femur by caries, or (as is more commonly the case) by the destruction of the acetabulum and capsular ligament, and *dislocation* of the *bone upwards* by the muscles." To illustrate the deformity and dislocation, the author gives a sketch, or wood-cut, taken from a patient under the care of Mr. Fergusson, in the King's College Hospital. In the position which I take in order to disprove the doctrine of spontaneous dislocation of the hip-joint, without accident or superadded violence, I shall rely upon nothing but *post-mortem ocular demonstration;* so I shall require the same proof to show the existence of a spontaneous luxation.

At the time the sketch was taken of Mr. Fergusson's patient, upon which Mr. Druitt relies for proof of dislocation, the case was under treatment, the patient was not dead, nor was the figure drawn from *post-mortem* appearances of the dissected parts.

This is a pretty fair example of proof, upon which the profession at large and authors rely to sustain their position on this subject.

James Miller, in his *Practice of Surgery*, remarks on this subject, under the head of Morbus Coxarius: "As disorganization advances within, the joint becomes more and more loose; and *dislocation* may occur by *muscular action alone*, without the intervention of a fall, or other injury. The dislocation is usually upwards on the dorsum of the ilium." Here the principle of *dislocation* is boldly recognized; but does the author give any evidence of its existence by a *post-mortem* dissection?

Sir Charles Bell, in his *Institutes of Surgery*, in speaking of morbus coxarius, remarks: "Another peculiarity in the position of the patient with diseased hip, is that of throwing the thigh of the affected side over the other, that the head of the thigh-bone may be raised, so as to relieve the inflamed socket. The thigh-bone becomes as a lever, loaded at the lower end, by which the upper end is raised, and

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the pressure taken off the inflamed glenoid cavity. It is a position of great relief; but the consequence is *actual dislocation in extreme cases.*"

It has been my privilege to examine the specimens of this affection in the London University Hospital Museum, where many of Mr. Bell's morbid specimens are deposited, and yet I could discover no preparation of "hip disease," where it appeared in the least degree as though the head of the femur was luxated during the life of the patient.

Baron Dupuytren, on the *Injuries and Diseases of the Bones*, under the head of Congenital Dislocation, observes: "Whatever importance may be attached to this dislocation in the abstract, it is deserving of still more attention on account of its presenting all the signs of luxation consequent on disease of the hip-joint, with which it has *always been confounded.*" After giving the diagnostic symptoms of the two diseases, and the different distinctive marks of each, the author remarks: "For it (congenital dislocation) does not include that painful and cruel disease of the hip-joint which usually issues in *spontaneous dislocation* of the *femur.*"

Here the doctrine of spontaneous dislocation of the femur is recognized, and yet it is not supported by any proof from *post-mortem* examinations.

J. M. Chelius, a professor of surgery in Heidelberg, and author of a *System of Surgery*, speaks of dislocation of the head of the thigh-bone, in coxalgia, as *accidental and rare*, rather than as a *common occurrence.* He says: "Dislocation of the head of the thigh-bone is not a necessary phenomenon in the third stage of coxalgia; it is rather accidental, and depends on the position of the diseased extremity, on the motions of the patient, and other influences during the relaxation of the muscles, and on the relaxation and destruction of the ligaments, especially of the round ligament."

In Pirrie's *System of Surgery*, under the head of Morbus Coxarius, we find the principle of spontaneous dislocation recognized. He says: "When dislocation takes place, which, however, is not always the case, the head of the femur is usually drawn upwards and outwards on the dorsum of the ilium." He adds: "It may be found in other directions, but very rarely indeed." A figure of a patient of the author's is given, with the following note of explanation: "A drawing, exhibiting the great shortening of the limb, and the alteration in the form of the hip in the advanced stage of morbus

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coxarius, subsequent to spontaneous dislocation, from a patient in my surgical ward at the hospital." It will be observed that the author does not make his drawing from the dissected bones of the diseased hip-joint. Because the limb is two or three inches shorter than the other, and because the leg is flexed on the thigh, the thigh on the pelvis, with inversion of the foot and great adduction of the limb, and projection of the trochanter-major, the author seems to take it for granted that there is a dislocation. I propose to demonstrate from the actual morbid specimens, hereafter, that all these symptoms can and do exist in a great majority of the fatal cases of hip disease, without dislocation.

The late Robert Liston, in his *Elements of Surgery*, after giving a description of the changes in the osseous parts of the joint, caused by ulcerative absorption and destruction, says: "From such changes in the osseous parts of the articulation, the limb is shortened, sometimes to a great degree, though no dislocation has occurred." He adds, "indeed, dislocation is by no means so frequent a cause of the shortening as is *generally believed*." All this, thus far, goes to support the point I am aiming to establish; and coincides with my views as to the cause of the *shortening* and *distortion* of the *limb*. But the author puts forth another doctrine, in relation to this subject, to which I cannot subscribe. It shows, however, that he regards luxation as a legitimate result of what he calls morbus coxarius. He says: "A portion of the *dorsum* of the *ilium*, upward and backward, which is the most frequent distortion in this disease, is thus gradually absorbed so as to form a cavity for the reception of the femur, the extremity of which becomes *more solid* in *texture* and *more smooth* in its articular surface. Whilst a depression is thus formed, new bone is deposited round its margins, whereby the cavity is increased in depth, and resembles the original acetabulum; the new deposits having become smooth and of a regular form. In many cases the new acetabulum is almost entirely formed by the deposition of new bony matter, which gradually adapts itself to the extremity of the femur."

This is undoubtedly true in cases of idiopathic dislocation; and something of a similar result may take place in *interstitial absorption* at one part, and in *interstitial accumulation* in another—in the porcelaneous deposits and changes that take place in old age; and sometimes from a contusion upon the trochanter major.

But I deny that any such result ever has, or can take place in genuine ulcerative absorption with more or less destruction of the

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head of the femur and acetabulum. When the cartilage is destroyed, and a portion of the head of the bone has been carried away, it is either in a naked carious condition, or it is vascular and covered with coagulable lymph. In the latter case, if it were applied to a similar surface, and the parts kept in easy apposition, and perfectly at rest, the anastomosing bloodvessels might shoot from surface to surface, by which a circulation between the two parts would be maintained, of which, under otherwise favourable circumstances, anchylosis would be the result. If the head of the bone, thus changed in its organization, could rest upon a perfectly healthy cartilaginous surface, it is possible, indeed it is very likely, that we might in a few instances, with the head of the bone healed over, hardened and polished, have a considerable degree of freedom of motion. But when the *ulcerated spongy head*, or what remains of it, is thrown (if it ever is) upon the periosteum of the dorsum of the ilium, I do not believe that any such result as that described by Mr. Liston can take place.

In a recently published work on *Congenital Dislocations of the Head of the Femur*, by John Murray Carnochan, M. D., under the head Diagnosis, in comparing the symptoms of *spontaneous* with those of *congenital* dislocation, he remarks: "The occurrence of spontaneous dislocation—which as a result of ulceration of the border of the acetabulum, sometimes takes place, although rarely, in morbus coxarius; it is exceedingly rare for the malady to occur symmetrically and simultaneously on both sides—a circumstance which is of common occurrence in the congenital dislocation of the femur."

Samuel Cooper, in his work entitled *The First Lines of the Theory and Practice of Surgery*, says: "In the second stage of this disease, the acetabulum is either widened, or parts of its brim destroyed, while the head of the femur is more or less diminished, and both the ligamentum teres and the synovial membrane are sometimes nearly annihilated. Hence, the most frequent cause of the shortening of the limb in the second stage of the disease, and shortening not dependent upon actual dislocation, which, however, sometimes occurs, as as the case in a patient under my care last spring (1839) in U iversity College Hospital, and the specimen taken from which is now preserved in the museum of the same college." In 1848, nine years after the deposit of the specimen, I examined the University College Hospital Museum, most thoroughly and minutely, and am sure that it did not then contain a single specimen of what I regarded a spontaneous dislocation of the head of the femur as the

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result of ulcerative absorption. In some of the specimens, the head of the bone was almost or entirely absorbed, while the acetabulum in some was elongated upwards and backwards; in others, enlarged in three-fourths of its circumference; and in others deepened, and even largely perforated. In one specimen, I observed that the head of the bone was entirely destroyed, while the neck occupied the hole, caused by ulceration at the bottom of the acetabulum, and presented towards the cavity of the pelvis.

From Prof. Wm. Gibson's work, entitled *Institutes and Practice of Surgery*, I select the following quotation, for the purpose of showing that he regarded spontaneous luxation of the hip consequent upon ulceration as of frequent occurrence. The author remarks: "Sometimes the foot and limb, instead of being turned inwards, are directed outwards. This will depend, I imagine (he continues), in some measure, upon the position the patient has been accustomed to lie in, and in others upon the direction the head of the bone or its remains may happen to take after *abandoning* the *acetabulum;* for *among* the *numerous morbid specimens contained in my cabinet*, I find the upper extremity of the *shaft resting upon the obturator opening in some*, in others upon the *tuber ischii*, in others *upon the pubis*, but generally on the *dorsum ilii*. Occasionally, it happens that the head of the thigh slips from the acetabulum, before it has undergone any important change; the capsule, round ligament, and cartilages of the cup have been previously destroyed by ulceration. At other times the bony texture of the acetabulum is destroyed by caries, and the head of the thigh-bone, or its shaft, enters the pelvis."

So far as the change and destruction of the bony texture of the joint are concerned, all this is true; but let us examine the facts in relation to the *frequency* and *variety* of *spontaneous dislocation*, or "abandoning the acetabulum."

In February, 1848, I visited the Pennsylvania University College Museum, after having listened to a lecture from the Professor of Anatomy, by whom, at my request, I was shown the museum, and sought, by his aid, to examine all the specimens of hip disease, or ulcerative absorption of more or less of the head of the thigh-bone and of the acetabulum. It was agreed, both by the professor and myself, that we did not find a single specimen of a well-marked case of the disease under consideration. When I called to mind the language of Professor Gibson, as above quoted, I must confess I was

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not a little surprised that I could not find a few "among the numerous morbid specimens contained in his cabinet."

At the time of my visit, Professor Gibson was too ill to lecture, or to be at his room in the college, which of course deprived me of the privilege of conversing with him on the chief object of my visit, and of his valuable aid in making an examination of the morbid specimens of the museum. But when I reflected that the "numerous specimens," to which he refers in his surgical work, might be in his private room among other morbid specimens which he was in the regular habit of using in the course of his lectures for demonstration and illustration, I was disposed to believe that they were "numerous," and that it would be a rich treat to get access to them. To the anatomist, who politely showed me the "Wisterian" or general museum, I expressed my regret at not being able to see Professor Gibson, and explain to him my views on the subject of spontaneous dislocation; yet he was sure that it was of common occurrence, for he had seen it in patients treated for hip disease by himself. To this remark I replied: "Doctor, was your diagnosis verified by a *post-mortem* examination?" To which he replied: "No; the patients did not die." I thought too much of the age and standing of the professor, to put the question which would naturally follow: "How, then, did you ascertain that luxation existed during life?"

In January, 1853, I revisited the medical colleges and anatomical and pathological museums of Philadelphia, when I was fortunate in meeting with Professor Gibson, and to whom I broached my favourite subject of inquiry in something of the following language: "Doctor, I desire to examine your specimens of hip disease (ulcerative absorption), to see whether the oft-repeated assertion of authors, that spontaneous dislocation results from this affection, be verified in the condition of the parts after death." To which the professor replied: "You may look in vain—you never see them." The professor's private collection of fractures, and other morbid specimens in osteology, was extensive and interesting; among them we found four or five genuine cases of caries, or ulcerative absorption of the bony structure of the hip-joint; and yet, not in a single instance, did we meet with a case that appeared in the least like a dislocation, as though the altered and half-destroyed head of the bone had ever played or rotated on the "*dorsum ilii*, upon *the obturator opening*, *upon the tuber ischii*, or *upon the pubis*."

In the Jefferson Medical College Museum, Philadelphia, there are four or five good specimens of genuine hip disease, if we include one

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in which there is an anchylosis of the hip-joint—one of the two, out of nearly two hundred specimens of hip disease I have examined, that would favour the doctrine of spontaneous dislocation. In this instance, the head of the bone was not much altered in shape or diminished in size, while it rested on the margin of the upper and back part of the acetabulum, slightly projecting over a pretty deep and rough acetabulum, where it had become anchylosed by bony deposit.

Sir Astley Cooper, in his *Treatise on Dislocations and Fractures of the Joints*, under the head of Dislocation from Ulceration, says:—

"Dislocations may arise from ulceration, by which the ligaments are detached, and the bones become altered in their form. We frequently find this state of the parts in the hip-joint; the ligaments ulcerated, the edge of the acetabulum absorbed, the head of the thigh-bone changed both in its magnitude and figure, *escaping from the acetabulum* upon the ilium, and there forming for itself a new socket." It will be observed, that here the doctrine of spontaneous dislocation of the hip-joint is recognized and taught by the highest and most weighty surgical authority of modern times.

For many years, I have looked upon the writings of Sir Astley Cooper as the most reliable of any in the English language. He was my oracle; and I almost venerated the man for the clear, concise, and honest manner in which he was accustomed to present his views on the various subjects of surgical science.

He never appeared to be at a loss for an appropriate case for the illustration of his subject; nor did he appear to be satisfied with a mere superficial examination of the subject under investigation. His deductions and conclusions were commonly founded on anatomical and pathological demonstrations. Nevertheless, with all this rare combination of a variety of talents, it is possible that even a great and a good man may be mistaken.

To illustrate the subject of spontaneous dislocation, Mr. Cooper gives the following case. He says: "Mr. White, of Parliament Street, had under his care a patient in the Westminster Hospital, who was the subject of dislocation of the os femoris on the dorsum of the ilium, from ulceration. He sawed off the head of the femur, and the patient was restored to a very useful motion of the limb." With regard to the formation of a "new socket," as asserted by Mr. Cooper, and as repeated by Mr. Liston, I have already stated such pathological objections as seem to me conclusive against the doctrine of the formation of a new joint. And, besides, in all the specimens of the numerous pathological museums I have examined, no evidence

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in the preparation itself has been discovered to show any such result. In *Braithwaite's Retrospect*, No. 22, January 7, 1851, at page 196, there is a synopsis of a case of removal of the head of the femur, at St. Bartholomew's Hospital, by Mr. Skey, in a case of "hip disease." After giving the history of the disease, the age of the patient, being thirteen years old, her condition on admission to the hospital, &c., the account says: "The left femur was *dislocated* on the *dorsum ilii*, the *limb shortened*, and the *leg* and *thigh flexed*." After consultation, "it was considered that removing the *head of the bone* would give the patient the best chance of recovery." Next, follows a description of the operation; and lastly, the morbid appearance and organic changes of the joint. The report states, that "the *acetabulum* was found to have been *enlarged* by *absorption*, and was *extended* in a direction *upwards* and *backwards*, as if an attempt had been made by nature to form a *new joint in this direction*. The *head* of *the femur had been entirely absorbed;* a portion of the neck remained, which, with the great trochanter, was the part removed." Now, if "the head of the femur had been entirely absorbed," how does it happen that "the left femur was dislocated on the dorsum ilii?" Or, if "only a portion of the neck remained," under what conceivable circumstances could there be a dislocation of what did not exist? Now, at the risk of being charged with a degree of temerity unbecoming my rank and position in surgical science, I venture to suggest, in the case of the patient of Mr. White, reported by Sir A. Cooper, "who was the subject of *dislocation* of the *os femur* on the *dorsum of the ilium from ulceration*," that there might have been "entire absorption of the head of the bone, and a portion of the neck." If so, where is the evidence that the patient "was the subject of dislocation of the os femur on the dorsum of the ilium from ulceration?"

Mr. Fergusson reports a case of excision of the upper end of the femur in morbus coxarius, an abstract of which is given in *Braithwaite*, 1845, where it is said, "the head of the femur was *displaced on the dorsum ilii*, and could be felt by the finger to have passed into a large sinus." I apprehend the "large sinus," into which the head of the bone had "passed, and that could be felt by the finger," was nothing more nor less than an *enlarged acetabulum upwards and backwards*, a *recession* of the *attachment* of the *capsular ligament*, its *thickened state*, and *deposit of lymph and serum between it and the cellular tissue* and *muscles covering it*. In the abstract above referred to, no account is given of the pathological condition of the diseased bones, on examination at the time, or after the operation.

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It is proposed next briefly to examine some of the cases bearing upon the subject under investigation, presented by Sir Benjamin Brodie, in his work entitled *Pathological and Surgical Observations on the Diseases of the Joints*, published in 1834; and also his *Lectures* on the same subject published at a subsequent period. When we consider that Sir Benjamin Brodie stands pre-eminently at the head of the surgical profession on this subject, if not in general surgical science, it is with feelings of great diffidence that I attempt to examine the testimony, and collect the reported cases for analysis, of this truly great and practical surgeon. The first case selected is under the head of Inflammation of the Synovial Membranes of the Joints. Master L——, about eight years of age, in September, 1824, was attacked with symptoms of hip disease, and for such affection was treated for some time by his attending surgeon. "In March, 1825, it was discovered that the limb was shortened." In November, 1825, Mr. Brodie was consulted respecting the patient. "At this time," Mr. Brodie says, "there were all the marks of a dislocation of the hip upwards and outwards. The limb was shortened, the toes turned inwards, and the head of the femur was distinctly to be felt on the posterior part of the ilium above the margin of the acetabulum." In this case it will be observed that there was no *post-mortem* examination; and I shall attempt to show hereafter that all the above symptoms of dislocation can be explained in the changed and destructive condition of the parts of the joint. In this instance it would appear, according to the views of Mr. Brodie, that the disease commenced in the synovial membrane, and that it resulted in ulceration of the cartilages and ulcerative absorption of more or less of the head of the bone without suppuration; as it is distinctly stated that "a swelling, which at one time appeared to be an abscess, disappeared." In support of this view of the case, Mr. Brodie remarks: "In whatever way the ulceration of the articular cartilage is produced, there is this remarkable difference between it and the ulceration of the soft parts; suppuration seldom takes place while the ulcer of the cartilage is small; and often the disease proceeds so far as to cause caries of the bones to a great extent, without *matter being found in the joint.* Under the head of Ulceration of the Articular Cartilages, Mr. Brodie gives the following case for the illustration of his subject; and it is presented not only to show that Mr. Brodie believes that spontaneous dislocation does take place; but also, to show the precise pathological condition of all the parts of the joint as I have viewed them for a long time past; such as will

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account for all the symptoms of a dislocation, when in fact none exists; at least, in the common acceptation of the term, as we are in the habit of using it in relation to an idiopathic dislocation. "A middle-aged man was admitted into St. George's Hospital in the autumn of 1805, on account of a disease of his left hip. He laboured also under other complaints; and died in the February following. On inspecting the body, the soft parts in the neighbourhood of the joint were found slightly inflamed, and coagulated lymph had been effused into the cellular membrane round the capsular ligament. There were no remains of the round ligament. The cartilages had been destroyed by ulceration except in a few spots. The bones, on their exposed surfaces, were carious; but they retained their natural form and size. The acetabulum was almost completely filled with pus and coagulated lymph; the latter adhering to the carious bone, and having become highly vascular. The *head of the femur was lodged on the dorsum of the ilium.* The capsular ligament and synovial membrane were much dilated, and at the superior part their *attachment* to the bone was *thrust upward*, so that, *although* the *head of the femur was no longer in the acetabulum, it was still within the cavity of the joint.*" Mr. Brodie next explains how dislocation was effected in this case. He says: "It was produced in consequence of the destruction of the round ligament, and of the head of the femur having been first pushed outwards by the coagulated lymph and pus which occupied the cavity of the joint, and then drawn upwards by the action of the muscles inserted into the great trochanter." Hereafter, I shall make some practical deductions in relation to my mode of treatment, from the pathological condition above detailed. In a note at the bottom of the page, Mr. Brodie says: "This case affords one example of the *dislocation* of the *hip from an internal cause*, which some surgical writers have described." Mr. Brodie says: "The *head* of the *femur was still within the cavity of the joint.*" If this be true, how could the head of the femur be "lodged on the dorsum of the ilium"? Mr. B. explains: he says the head of the bone had left the acetabulum, but had not escaped from the cavity of the joint. If this be a satisfactory definition or explanation of a *spontaneous dislocation* resulting from hip disease, then all I can say is, that it differs altogether from the relation of the head of the bone and the capsular ligament in idiopathic dislocation, where there must be, either extensive detachment of the capsular ligament from the margin of the acetabulum, or a wide slit through it, at the point forcibly pressed upon by the head of the bone in the act of displacement.

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I shall next make a few extracts from Mr. Brodie's lectures, which will go still farther to confirm his views of *spontaneous dislocation*, and of the pathological conditions of the joint upon which the dislocation depends.

In speaking of inflammation of the synovial membrane, he says:—

"A girl was seized with an attack of what was supposed to be rheumatic fever. In the course of two or three days the fever was excessive. She lay in a state of delirium for several days, which seemed, as it were, to supersede the local symptoms which existed in the first instance. She nearly died; but, by and bye, things took a favourable turn, and she recovered. After her recovery, her friends looked to the limb, and, to their surprise, found it very much distorted. Mr. Earl and myself (Brodie) saw the girl under these circumstances, and we found that *the head of the bone was lodged on the dorsum of the ilium.*" In this case there is no demonstrative evidence, from an examination of the diseased joint instituted after death, "that the head of the bone was lodged on the dorsum of the ilium."

Under the head of Scrofulous Disease of the Hip, I select the following for the purpose of showing the views of the author concerning the manner in which dislocation takes place. He says: "When an abscess presents itself externally from this disease, there is invariably another change in the condition of the limb. It becomes shortened; and this occurs in two different ways. If the *head* and *neck* of the bone be *destroyed*, and especially if the margin of the acetabulum be *destroyed* also, it is no matter of wonder that the muscles should pull up the thigh bone, and that the limb should be shortened. In these cases, the limb is shortened just in the same way in which it would be shortened in a case of *fracture* of the *neck* of the *femur*. The toes, if left to themselves, turn outward, and the thigh may remain in a line with the trunk. But, at other times, the shortening of the limb takes place in another way. The cavity of the acetabulum is filled up with lymph or pus, or with lymph and pus altogether; the capsular ligament of the joint in consequence is dilated, the patient having undergone a great deal of suffering from the tense state of the capsule previously. Then the round ligament is destroyed by the ulceration which has already consumed the cartilage. The head of the femur is pushed from the joint until it passes from beyond the margin of the acetabulum, and then the muscles pull it upwards, and lodge it on the dorsum of the ilium. Sometimes when the head of the femur is dislocated, it will be nearly entire; in other cases, it is partly absorbed. The dislocation is generally in the same direction; and here, as in those other cases in which dislo-

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cation arises as a consequence of inflammation of the synovial membrane, the dislocation being upwards and outwards, the limb is much shortened, and the toes are turned inward. There is one circumstance which favours the escape of the head of the femur from the socket of the acetabulum, and also favours the escape of the remains of the neck wherever the head has been destroyed; and that is the position in which the child generally lies if left to himself, with his knee bent inwards so as to cross the sound limb."

I think it has been fully shown, from the extended references and quotations of authors on this subject, that the notion or doctrine of spontaneous luxation in hip disease is the prevailing opinion, and that, in the majority of the works referred to, no proof from dissection has been adduced. It is proposed next to examine the morbid changes of the joint in an advanced stage of the disease, and demonstrate from the specimens of *post-mortem* examinations or dissections. I have already quoted, from the writings of Sir Benjamin Brodie, a minute and graphic description of the diseased appearances of the joint—to which nothing can be added except my views in relation to the condition of the capsular ligament as it is attached to the os innominatum. "In the advanced and ulcerative stage of the disease, the round ligament is destroyed, the cotyloid detached and carried away from the margin of the acetabulum, and the capsule, greatly thickened and softened, recedes from the margin of the acetabulum and seeks new attachments, as ulcerative absorption of the acetabulum encroaches on its natural boundaries. This new attachment of the capsular ligament to the dorsum of the ilium, and the thickened state of the periosteum, is shown in the rough surface; and, in some instances, in spiculæ of bone, occupying a space of considerable distance all around the enlarged acetabulum.

Wherever a chronic periostitis has existed for some time, it is well known that the thickened membrane is much more firmly adherent to the rough surface of bone to which it is attached, than in the normal condition. If this be true, as we meet with periostitis on the tibia, for instance, why may it not be equally true in the case of the innominatum around the acetabulum in hip disease? If this point be conceded, it will be obvious that it presents a strong barrier against the doctrine of spontaneous luxation.

Mr. Brodie says: "If the *head* and *neck* of the bone be destroyed, and especially if the margin of the acetabulum be destroyed also, it is no matter of wonder that the muscles should pull up the thigh-bone, and that the limb should be shortened." If there should be nversion of the foot, and strong adduction and flexion of the limb,

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as most likely there would be, all the well-marked symptoms of an upward and backward dislocation would be present, and yet the head and even the neck of the bone cannot be dislocated because they do not exist. The pathological condition of the textures, both *osseous*, *cartilaginous*, and *ligamentous*, which enter into the formation of the hip-joint, in morbus coxarius, have been minutely and truthfully described by many writers; with the single exception of the relation of the head of the bone to the attachment of the capsular ligament to the outer face of the innominatum surrounding the *elongated* and *enlarged acetabulum*.

In as few words as possible, and by the aid of the pathological specimens of the hip-joint, I now propose to explain, and even *demonstrate*, how all the symptoms of a dislocation can be present during life, when, in reality, on making a dissection after death, the head of the bone is not found lodged on the dorsum of the ilium.

The cartilage is absorbed, the round ligament is destroyed, and three-fourths of an inch of the upper part of the head of the bone has been carried away by ulcerative absorption. Simultaneously, the cartilage of the acetabulum is disposed of in the same way; the cotyloid ligament, at the upper and back part, is destroyed or removed from the margin of the acetabulum, and the socket is elongated or enlarged three-fourths of an inch in the upward and backward direction. By the action of the forepart of the *gluteus-medius*, which lies in juxtaposition with the upper and back part of the capsular ligament, and by the *tensor vagina femoris*, when aided by the action of the *psoas magnus* and *iliacus internus* muscles, the *foot is inverted*, and the *thigh is flexed* on *the pelvis* and *strongly adducted*, by which the *trochanter major* is made to project, and to occupy a position nearer the *anterior superior spinous* process of the ilium than in the normal condition; and by the thickening of the capsular ligament, and by effusion of serum and lymph in the cellular tissues, and in the *glutei muscles*, a prominence is observed which has been regarded as the head of the bone *lodged* on the *dorsum of the ilium*.

By the destruction of three-fourths of an inch of the head of the bone, and by absorption and elongation of the acetabulum upward and backward to the same amount, if these two parts remain in contact, as it is contended they do, by the action of the glutei muscles, then we have an inch and a half of shortening of the limb. The other symptoms of a dislocation have already been explained.

I have quoted from more than a dozen authors who have written on this subject, and, without a single exception, all admit and sustain the doctrine of spontaneous dislocation in hip disease. It is

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true, two or three of them regard it as occurring very rarely. All affirm the doctrine, but very few give any proof of it, by an actual examination of the dissected parts. I have selected the strongest marked cases, and have shown in most of them that the diagnosis was founded on *anti-mortem examinations*—the *appearances during life*, as in the case presented by Mr. Fergusson, and one by Sir Benjamin Brodie. Mr. S. Cooper gives a case of dislocation, which was preserved in the London University Hospital Museum, in the year 1839; but in 1848, I could find no specimen in that museum which appeared at all like a spontaneous luxation.

Mr. White's case, reported by Sir A. Cooper, in which the head of the dislocated bone was sawed off, might have been of the character of Mr. Skey's, in which he performed the operation for the removal of a *dislocation* of the head of the bone upward and backward, when, in the course of the operation, he found that all of the head and a portion of the neck had been removed by absorption. It will also be borne in mind that Mr. Brodie gives a case in which "the head of the femur was lodged on the dorsum of the ilium, and yet it was still in the cavity of the joint."

This is a summing up of all the *demonstrative testimony* on the subject of spontaneous dislocation of the hip-joint, caused by ulcerative absorption.

It is possible, and perhaps probable, that a different construction may be given to the term *dislocation*. Indeed, this is to be inferred from the remark of Mr. Brodie, in relation to one of his reported cases; in which he says, "the head of the femur was lodged on the dorsum of the ilium, and yet it was still in the cavity of the joint."

Luxation, which signifies "to put out of place," is defined to be "a displacement of a part from its proper situation." In this sense, the change of *form and position* of the acetabulum, as in cases of ulcerative absorption, might be called a luxation. And yet, surgeons never speak of a luxated or dislocated acetabulum. The most common idea attached to the term "luxation," is, that it is a putting out of joint—a breaking up and destruction of the natural relations of two or more bones at an articulation, which can be accomplished only by the laceration of ligaments. Now, it has not been shown, in a single instance, of what has been regarded a case of spontaneous luxation, so far, at least, as my investigations go, that the head of the bone had been thrust through a *rent* in the *capsular ligament*, or that it had *been detached from the bone and carried sufficiently far back on the dorsum of the ilium to permit the head of the bone to occupy the space between its attachment and the margin of the aceta-*

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bulum. To this statement I must, however, make an exception, if it can be so considered. I find, on referring to my notes taken in the summer of 1848, at St. Bartholomew's Hospital Museum, the following memorandum: "Wet specimens of hip disease, 13; one of which, No. 17, appeared to be more like a spontaneous luxation from hip disease than any I had seen, or afterwards saw, in all of my researches."

Dr. Scoon, a graduate of the Albany Medical College, some four years since reported the following case to me: A lad, fourteen years of age, was attacked with the ordinary symptoms of hip disease, and while under the care of one or two physicians of the country town in which he resided, at the end of six or eight months, there began to be shortening of the limb, inversion of the foot, flexion and adduction of the thigh, and preternatural prominence of the trochanter major. Next, an abscess formed, which was opened; at length, the patient became troubled with a cough, and other symptoms of disease of the lungs, and in about twelve or fifteen months died. Three out of the four physicians who saw the patient during life, were firm in the belief that there was a dislocation of the hip upwards and backwards, and, without doubt, would have reported it as such, had not a *post-mortem* examination proved the contrary. Dr. Scoon, who was thoroughly imbued with my notions of the pathological conditions of hip disease, contended that there was no dislocation. He made the *post-mortem* examination in the presence of the three gentlemen, who called it a case of spontaneous luxation. The soft parts were carefully thrown aside, and on exposing the interior of the joint, the round ligament was found shrunken, though not detached either from the acetabulum or head of the bone—the articulating surface of the acetabulum, that which is naturally covered with cartilage, *destroyed, and elongated upwards and backwards nearly three-fourths of an inch*—the cartilage was removed from the upper part of the head of the bone, where it was a little flattened by ulcerative absorption. The capsular ligament was thickened, but still attached to the border of the *enlarged acetabulum* and face of the ilium; and not the slightest evidence appeared, to show that the head of the bone had ever occupied the dorsum of the ilium. It is not my design to expose the errors in diagnosis of physicians and surgeons, in relation to the form of disease of the hip, now under consideration; but I have known several instances of hip disease, pronounced idiopathic luxation, the pulleys applied, and a vigorous attempt made to reduce the supposed dislocation. I am sustained in my views, as respects spontaneous dislocation of the hip, by one of the most *minute, experienced, and accurate* pathologists in the United States. I refer to Dr. J. B. S.

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Jackson, of Boston, who has in his pathological cabinet four or five strongly marked specimens of hip disease, and in almost every respect corresponding with my own, and those of the College of Physicians and Surgeons of New York. The specimens of Dr. Jackson and my own were exhibited to a portion of the members of the American Medical Association, at the rooms of Dr. Jackson, while the Association was in session in Boston, in May, 1849.

Having occupied so much time in investigating the subject of spontaneous luxation, and the pathology of the joint in hip disease, I must hasten to a brief consideration of what I stated at the commencement of this article, a mode of treatment that was new, and nearly or quite peculiar to myself. But I find, in looking over the *Medical Examiner*, published in Philadelphia, January 19, 1839, that William Harris, M. D., has anticipated, and put into practice my theory in the treatment of four cases of morbus coxarius, without giving any explanation of the theory or principles upon which the practice is founded. In the third or fourth case treated by Dr. Harris, he speaks of *dislocation* in familiar terms, and as though it followed, as a matter of course, that there was a *dislocation*, because the *symptoms were present during life.* In no case, however, does he speak of a dislocated hip, demonstrated by a *post-mortem examination.* Dr. Harris's new, and, as I deem, important practice in the treatment of hip disease after *distortion*, and, as he believed, *dislocation*, had taken place, consisted in the use of *extension* and *counter-extension*, by means of Physick's long splint, modified by Professor Gibson. There seems to be nothing new in the *medical* treatment of the four cases reported as being successfully treated on the principle of extension and counter-extension.

The first, and as Mr. Brodie says, *a* most, but I say *the* most important part of the treatment of morbus coxarius, consists in the keeping of the joint in a *perfect state of repose.* For a long time before the republication of Mr. Brodie's lectures in this country, and of course, before I saw his work, I was in the habit of explaining and illustrating this important principle in my lectures, in nearly the same language as that adopted by Mr. Brodie. My illustration has been this: "Suppose you had a severe burn, or an ulcer on the back of one hand, and with the other were to rub it a dozen or twenty times a day, how long do you think it would take to heal such a sore, under such treatment?" Mr. Brodie says: "If your leg were inflamed, and you were to rub it a dozen times every day, would you not make the inflammation worse? If your leg were ulcerated, and you were to rub it in the same manner, would you not prevent the

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ulcer from healing? Nay, rather, would you not make the ulcer spread? If the hip-joint, then, be inflamed, and you move it several times a day, will not the inflammation be kept up? If the cartilages or bones or both are ulcerated, and the joint be moved, and the ulcerated surfaces rubbed against each other, is it not to be expected that the disease will be aggravated?" Mr. Brodie adds: "It matters not how trifling the disease may be, your *first duty* to your patient is to make him lie down at once, and keep the hip-joint in a perfect state of immobility."

It is proposed next to show how *extension* and *counter-extension*, the mode of treatment which I believed was peculiar to myself, as it is not to be found in any of our systems of surgery, can be explained on pathological principles. In the advanced stage of the disease, the cartilages of the acetabulum and of the head of the bone are destroyed; the carious and loose spongy bony tissue is covered with coagulated lymph, and this adhering to the surfaces of the bones, becomes highly vascular. Now, if these two surfaces thus covered with *organized* or *organizable* matter, could be kept in easy apposition and at rest—without too much pressure on the one hand, and on the other without exerting too much separating or sundering force, in many instances a bony anchylosis would be the result. By the *pressure* of the muscles that act on the head of the bone and acetabulum, in addition to *ulcerative absorption*, we get *progressive absorption*, and hence the great destruction of the osseous parts of the joint. Experienced surgeons know very well that, if a dossil of lint be too bulky, or too much compression be applied to a healthy granulating ulcer, absorption of the granules will be the result. Extension and counter-extension are used with a view to prevent *undue pressure* on the delicate and tender surfaces of the diseased bones. This treatment of hip disease can be used to advantage in an advanced stage of the affection, as is shown in the case treated by Dr. Harris. In the early stage of the disease, before shortening or distortion of the limb takes place, it will be important to resort to the use of the long splint, to maintain the joint in a state of rest, that the inflammation may subside.

The late Sir Charles Bell, who was a man of genius, and really a medical philosopher, in speaking of the position of the limb of the patient with diseased hip, says: "The thigh of the affected side is thrown over the other, that the head of the thigh-bone may be raised, so as to relieve the inflamed socket." From this hint or remark, we infer the propriety of making use of *extension* and *counter-exten-*

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sion to afford the same relief to the inflamed socket, without endangering an "actual dislocation in extreme cases," of which there would seem to be danger, in the estimation of Mr. Bell. I have used the long splint in several cases in the treatment of hip disease, where there did not appear to be much or any shortening. In only two cases have I used the long splint with efficient *extension* and *counter-extension*, to overcome what appeared to be a permanent and unfortunate distortion. In one case the leg was flexed on the thigh, the thigh on the pelvis, and the femur rigidly fixed in such a state of adduction, as would carry it nearly across the opposite limb above the knee. The inflammation was so acute, the affected part so tender, and the patient so sensitive and irritable, that it was impossible to place even a small pillow between his knees and lower part of his thighs. I was sure that if the limb should become anchylosed in that position, as there was good reason to apprehend that it would, it would be almost a useless appendage as an organ of locomotion.

To obviate, or prevent such a result, I conceived to be my duty, if practicable. Accordingly, I put my patient under the full influence of ether, and proceeded to *extend* and *abduct* the limb, by bringing it along the side of the other, and to which I applied the long splint, and made use of extension from the ankle, and counter-extension from the perineum. Before the influence of the ether had entirely subsided, I gave the patient a full dose of morphia, which was repeated three or four times a day, for two or three days; till some of the soreness, caused by the operation, had passed off, and he became accustomed to the mechanical power applied to the limb. In a few days, I began to give the patient, at night, a powder, composed of calomel, jalap, and Dover's powder, which insured a good night's rest, and generally purged in the morning. The splint was used eleven or twelve weeks, and under this mode of treatment the boy seemed to recover, if not with great freedom of motion in the joint, at least with a limb nearly as long as the other, and in a vertical position, or that which corresponded with the other limb when standing upon his feet. The patient continued to improve for several months, when at last, by over exercise and imprudence, suppuration was brought on, and an abcess was opened at the inner and upper part of the thigh. This was not followed by any more shortening or malposition of the limb. It is now a little shorter, and nearly anchylosed.

In another case, in which extension and counter-extension were employed, the patient was about fifteen years of age; and previous to the attack was considered healthy. In the latter part of April,

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1852, he waded into the river, where he remained for a short time, which was supposed to be the origin of his disease. In a week after the exposure he was attacked with a hard chill, which was followed by a high fever, when very soon severe inflammation fixed in the left hip, which continued without any abatement for eight or ten weeks. I was informed by the mother of the boy that the physician who first attended him regarded the affection as inflammatory rheumatism. In ten or twelve days from the commencement of the disease the limb began to contract, being strongly everted, the leg flexed on the thigh, and the thigh very much flexed on the pelvis, and greatly *abducted* or *carried off* from the other knee. When the patient stood erect on the well limb, the other could not be brought within a foot of touching the ground, nor could the two knees be made to touch by about the same distance. All motion at the hip-joint appeared to be lost. The health of the boy seemed in every respect good. Such was the condition of the patient on the 23d of October, 1852, when I was consulted upon the *possibility* and *propriety* of fetching the diseased limb into a position, by which it could be used as an organ of locomotion. From the little experience I had had in the treatment of the case already briefly noticed, which was in some respects analogous, although it had existed a less number of weeks than the case now under consideration had months, and from the strong anxiety of the boy to be relieved, and with the full consent of his parents, even though I had no *book authority* for my proposed plan of procedure, I resolved to make an effort to place the limb in a position favourable for walking.

It will be observed that from about the last of April, the time of the attack of the disease, to the 23d of October, the day of the operation, nearly six months had elapsed—long enough for pretty strong adhesion to take place between the head of the bone and acetabulum, capsular ligament, etc. consequent on acute inflammation of all parts of the joint. The patient was taken into the theatre of the Albany Medical College, before the medical pupils of the institution, and, by the counsel and aid of a few physicians, placed under the influence of ether, when my assistants fixed by their hands the pelvis, while I embraced the limb above the ankle with one hand, and the thigh just above the knee with the other, by which, I was enabled to produce a rotating motion of the limb, breaking up the adhesion of the head of the bone, and carrying the knee inwards towards the other, so that the head of the bone was observed to move outwards and backwards. As the adhesions yielded

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to the force applied, a distinct *crackling* or *crumping crepitus* was very perceptible. In the distorted position of the limb, the head of the bone appeared to rest on the anterior or pubic side of the acetabulum, and pressing firmly on the anterior or pubic wall of the capsular ligament. The limb was *extended*, *adducted*, and brought so as to lie parallel to the other in the extended position, and within about an inch of being as long as the other, where it was fixed by a perineal belt, long splint, compress and roller, after the manner of treating fracture of the thigh on the principle of permanent extension. The patient was conveyed in the horizontal position to his lodgings in the city, where he remained with the splint applied for two weeks without much suffering or medication. At the end of the two weeks he was removed to the country, six miles; and on the second day after reaching home the splint was removed and not again replaced. The patient soon after began to move about on crutches; and now (Feb. 26) I find him able to walk very well without the use of a crutch or even a cane. The limb appears to be an inch or an inch and a half shorter than the other—but by the aid of a high-heeled boot, which he is having constructed, he anticipates skipping about as lively as other boys.

It will be observed that in this case there was no suppuration or abscess, which is another instance, among quite a number that have fallen under my observation, of absorption of the head of the bone, "showing (as Mr. Brodie says) that ulceration may take place without the formation of pus."

I believe it is a conceded point, by all pathologists and surgeons, that the muscles are the inherent, internal disturbing agents of displacement in fractures—that the lesion in the broken bone, and the irritation of the muscles in juxtaposition result in *inflammation* and *spasmodic contraction*. I believe, also, that another point in relation to hip disease is equally well settled; it is, that the muscles about the joint are the active agents in producing distortion. The inflammation of the several parts of the joint extends to the muscles more or less, by which they are stimulated to act either continuously or spasmodically, fixing the limb in its malposition. I have seen a case of white swelling of the knee-joint (ulceration of the cartilages), where the spasm of the hamstring muscles would raise the joint from six to eight inches from its resting-place; and in a case, too, where binding the limb to a fixed Amesbury's splint, did not prevent the motion of the joint caused by involuntary contraction of the muscles.

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I shall quote once more from Mr. Brodie, to show the comparison which he institutes between the condition of the hip-joint, in the advanced stage of morbus coxarius, and fracture of the neck of the thigh-bone.

Mr. Brodie says: "At the conclusion of this investigation you will ask, 'What becomes of a joint of which the cartilage has been destroyed by ulceration?'" He replies: "If it be extensively destroyed without suppuration, the case may be *compared to one of simple fracture;* and, if there be suppuration, it may be compared to one of *compound fracture;* and in either instance the ulcerated surfaces of the bone come together, and ultimately become united. Bony anchylosis, however, takes place very slowly in the scrofulous disease, which I described in the last lecture; the bond of union being for a very long time nothing but a soft ligamentous substance. But in other cases, where the cartilage is ulcerated without scrofulous disease of the bone, bony anchylosis takes place, I believe, readily enough."

Now I would most respectfully ask Sir Benjamin Brodie, or any other experienced and prudent surgeon, if he should be called upon to treat a case of "simple fracture" of the neck of the thigh-bone, whether he would not employ suitable means, if bony union was desired, to elongate the shortened limb, and to maintain the extremities of the fractured bone in contact?

If there is some analogy between the condition of the hip-joint in morbus coxarius, and fracture of the neck of the bone, why should there not be some similarity in the mode of treatment?

If the operation of excision of the carious head and neck of the thigh-bone in morbus coxarius be an established practice, resting on high authority; and if the result of the operation be favourable in re-establishing the health of the patient, and restoring the limb to more or less utility, may we not make a practical application of these facts in sustaining the principle of extension and counter-extension?

I do not believe there ever was a case in which the head of the bone was excised in a carious condition without there was a similar state of disease in the acetabulum. Removing the head, therefore, does not remove all of the disease. In all of the specimens of ulcerative absorption of the head of the thigh-bone that I have ever examined, I have uniformly noticed quite as much disease and organic change in the acetabulum. The disease commences quite as early in the acetabulum as it does in the head of the bone.

In my notes, taken from the examination of the specimens of hip-disease in Guy's Hospital Museum, I find the following observations:

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"I saw one wet preparation where the *acetabulum* was *perforated*, and the *abscess communicated* with the rectum. Also, a specimen of a young child, in which the acetabulum was *ulcerated*, and *even perforated*, through which a *probe was passed*, while the *head* of the *bone was in its normal state.*"

If the carious head of the bone, when disturbed by the movements of the limb, or by the *contraction* of the muscles about the joint, be not the source of *irritation*, and the *cause* of the extension of the disease, how does it happen that excision ever succeeds when so much disease is left in the acetabulum?

Besides, if a compound fracture be much more dangerous and troublesome to treat than a simple fracture; and if we regard the hip-joint, in morbus coxarius, as it has been by Mr. Brodie, in the light of simple and compound fracture, would it not be far better to remove the *cause* of the *continuance* and *extension* of the disease without converting a simple into a compound fracture? Would not *extension* and *counter-extension* be attended with far less pain, and suffering, and danger to the patient, than excision of the head of the bone?

In comparing my mode of treatment of the advanced stage of hip disease with that employed by Dr. Harris, I find that the *principle* of extension and counter-extension is the same; and the *practice* differs only in this respect, viz., I accomplish at once, by the aid of ether or chloroform, what took Dr. Harris some days or weeks to effect. It will be remembered, however, that Dr. Harris's cases, reported in 1839, were treated before the discovery and introduction into practice of either ether or chloroform.

I find I must bring my lengthy paper to a close; and will only add that my aim has been to establish two points—the pathological condition of the head of the bone and acetabulum, and their relation to each other in the condition that has been so universally regarded as luxation; and the principle of treatment by extension and counter-extension, explained and founded on the laws of ulcerative and progressive absorption. If my views are sustained by ascertained facts, and by demonstration from the morbid specimens of the hip-joint—if the theory of practice, upon the plan of *permanent extension*, should become an established *principle* in the treatment of hip disease, in an advanced stage, or even before any great organic change has taken place in the joint, I shall have accomplished my object; and I trust have contributed a trifle to the advancement of surgical science. If I am in error in the views I entertain on this subject, I

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will sincerely say that I am open to conviction. I solicit the attention of the profession to the investigation of the subject; and desire it to be either proved or disproved by dissection and demonstration after death.

At a meeting of a large number of the delegates of the Association, and citizen physicians of New York, held at the Anatomical Theatre of the College of Physicians and Surgeons, a synopsis of the foregoing Essay was read; and the author's views of the pathological condition of the disease, in an advanced and destructive stage, demonstrated from a pretty extensive collection of specimens of hip disease, belonging to the New York College of Physicians and Surgeons, to the Albany Medical College, and to the author. At the same time the model of a splint was exhibited, which was designed to treat the disease, so far as mechanical means are concerned, upon the pathological principles more fully explained in the Essay.

The treatment of hip-disease by a long splint, I believe, was first suggested and employed by our late distinguished countryman, Dr. Physick, of Philadelphia. It seems that the only object he had in view, in using the long splint, was to secure rest and perfect immobility of the joint.

In the early stage of the disease, before there is any organic change either in the hard or soft parts of the joint, *rest alone*, even without the aid of a splint, would be sufficient to answer one of the first and most important indications; and perhaps the resolution and moral discipline of an adult might be relied on without the use of a splint. But in restless children, Dr. Physick's mechanical restraint was absolutely necessary in order to insure the most favourable and successful result. If, however, my views are correct in supposing that pressure produces absorption of the organized and organizable matter on the head of the bone, and the cup of the acetabulum, after destruction of their cartilages, then the principle of extension and counter-extension, as first employed by Dr. Harris, is highly important to prevent *progressive absorption*.

Dr. Physick's long splint, as used by Dr. Harris, was calculated to fulfil a part of the indication—relief from pressure of the upper part of the head of the bone, and the upper and marginal part of the acetabulum. But in fixing it to the whole length of the limb, and to the trunk nearly as high up as the axilla, there must necessarily be a considerable amount of pressure on the outer face of the trochanter-major, the tendency of which would be to produce absorption

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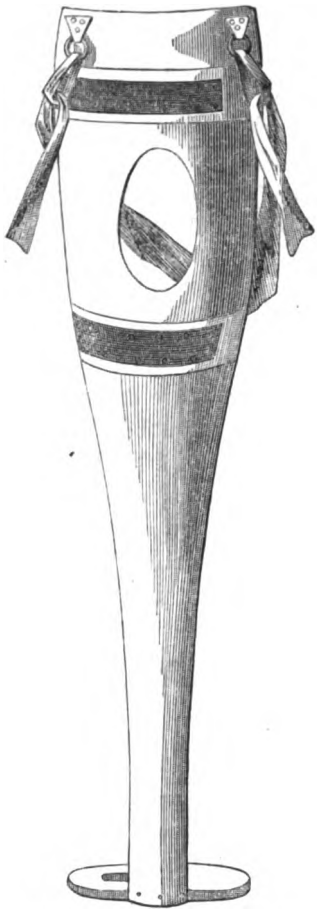
of the head of the bone occupied by the attachment of the ligamentum teres, and of the deep part of the acetabulum, so that the diminished head and neck of the bone might be thrust through into the cavity of the pelvis, which I have actually seen.

Fig. 1.



The structure of the splint exhibited to the members of the Association is designed to obviate all pressure on the trochanter-major.

The accompanying drawing, made by Mr. Daniels, of New York, will convey a better idea of its design than can be explained in words only. Its length, breadth, &c., must be adapted to the age and size of the patient.

The apparatus is simple, and consists, as will be observed, of a long leg and foot-piece, and a perineal belt or strap. In having the upper portion of the splint broad, and moulded to the trunk, and the extremities of the perineal strap attached to the anterior and posterior angles of the same, the extending and counter-extending force can be made to act very much in a line with the axis of the limb; a circumstance of considerable importance in the treatment of fracture of the neck of the femur.

The long splint may be made of thin wood, and moulded to the shape of the patient's limb. The foot-piece projects to an equal distance on each side, so that the splint may answer for either side. The extension may be made by straps attached to the ankle and lower portion of leg, and fixed to the foot-piece.

It will be observed that I have hardly alluded to the *early diagnostic* symptoms of morbus coxarius, nor to the *general plan* of treatment. These points I propose to take up hereafter.

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ON THE

SURGICAL TREATMENT

OF

MORBID GROWTHS WITHIN THE LARYNX.

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ON THE

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OF

MORBID GROWTHS WITHIN THE LARYNX,

ILLUSTRATED BY AN

ORIGINAL CASE AND STATISTICAL OBSERVATIONS, ELUCIDATING THEIR NATURE AND FORMS.

BY GURDON BUCK, M.D.,

SURGEON TO THE NEW YORK HOSPITAL.

Though cases of morbid growths, occupying the cavity of the larynx or its upper orifice, are found scattered through the records of medical science during the preceding century, very little was known of this disease by the profession at large, until Prof. C. H. Ehrmann, of Strasburg, published his able monograph in 1850, entitled, *Histoire des Polypes du Larynx*, in which, besides collecting these scattered cases, he has recorded two others that came under his own observation, in one of which he performed a successful operation for the removal of the disease. Of this operation, the illustrious Stromeyer, who presided on the occasion of its being reported before the Scientific Congress at Aix la Chapelle, in 1847, justly said that it should be ranked among the most brilliant achievements of modern surgery.

In the case about to be related, Professor Ehrmann's operation, with slight modifications, was repeated twice, and though not followed by the same happy result, yet the patient was rescued from impending suffocation, and her life prolonged for more than a year. The extraordinary interest and importance possessed by this case will, it is believed, be a sufficient apology for the length to which the narrative has been extended, and the experience afforded by it in its whole progress may also furnish useful suggestions for the guidance of the practical surgeon in analogous cases.

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Case.—On the 21st of April, 1851, I visited, at the request of Dr. C. L. Mitchell, Madam L——, a widow lady, aged 51 years, and the mother of several children, who resided in Hoyt Street, Brooklyn. She was suffering from obstruction of the larynx, attended with great dyspnœa and complete loss of voice. Her aspect, as she was seated in her chair, was that of a person in perfect health, with a florid countenance and rather corpulent condition of body. Her breathing was sonorous and laboured; inspiration evidently requiring greater effort than expiration; her voice could not be raised above a whisper. She was unable to perform the act of snuffling or to expel air through the nostrils, which was a source of very great annoyance to her. Deglutition was easy, except when she attempted to swallow liquids rapidly, and then the dyspnœa was increased. The larynx was the seat to which all her trouble was referred. Sometimes she described her sensations as if a lump obstructed her throat; sometimes as if a ruffle rose and fell alternately in the larynx; at other times, the account she gave of her sensations was vague. The dyspnœa was aggravated by whatever tended to produce excitement, and the approach of evening exerted a marked influence in causing an exacerbation of the symptoms. In the recumbent position she always required her head to be raised by extra pillows. Hoarseness, which was the earliest symptom observed in the onset of this ailment, had gradually resulted in complete extinction of the voice. For more than a year the aphonia had persisted, unaccompanied by dyspnœa. On the 14th of August preceding, Madam L. had called to see me, at Dr. Mitchell's request, and upon careful examination, redness and congestion of the fauces were the only symptoms her case presented besides aphonia; neither cough nor dyspnœa had as yet manifested itself. This latter symptom (dyspnœa) supervened about the beginning of the following January, and was soon accompanied with cough. Both steadily increased from this time, the cough being attended with an expectoration of yellowish viscid mucus, that had of late been tinged in the morning with blood. On inspection, the epiglottis was ascertained to be free from swelling, and in a healthy condition.

Exploration with the finger could detect no swelling or other morbid change at the orifice of the larynx. The fauces and pharynx were red and congested, but not swollen. Compression of the larynx externally augmented the dyspnœa, and produced a very disagreeable feeling. The patient had always enjoyed good health previously, and continued still to do so, with the exception of her local ailment.

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A strong solution of nitrate of silver had been applied to the larynx, and irritants externally to the front of the neck, in conjunction with other treatment, but without any benefit.

Diagnosis.—The symptoms above described, all seemed to point to the existence of obstruction to the passage of air through the larynx as their cause. What, then, was the condition upon which this obstruction depended? There being no ground of suspicion that a foreign body had lodged in the larynx, or that a nervous spasmodic condition of the parts produced the symptoms under consideration, the question arose, Was it acute inflammation producing serous effusion into the submucous cellular tissue at the orifice of the larynx, and constituting œdema glottidis; or, was it a deposition of false membrane upon the free mucous surface of the laryngeal cavity, and at the same time of coagulable lymph in the submucous cellular tissue, constituting membranous laryngitis?

Both these diseases are acute, rapid in their progress, and accompanied with febrile excitement; conditions wanting in the present case; moreover, the epiglottis and laryngeal orifice had been ascertained by inspection, and by the touch, to be in a normal state. Was it chronic inflammation with ulceration and thickening of the laryngeal mucous membrane, or abscess surrounding the cricoid cartilage, conditions depending on tubercular or syphilitic cachexia? Nothing in the previous history of the case warranted such a supposition; on the contrary, the patient presented every indication of the most perfect health. Was it a post-pharyngeal abscess which, by its encroachment upon the larynx, narrowed its cavity and thus obstructed respiration? Though this disease is sometimes chronic in its progress, and embarrasses more or less the functions of the larynx, its earliest and most marked effects are produced upon deglutition, first embarrassing it, and ultimately rendering it almost impracticable. Inspection and the touch, which readily detect the existence of such an abscess, had decided this question. By this process of exclusion, the limits were narrowed down to one other condition, and only one capable of producing obstruction, namely, the existence of a morbid growth within the larynx. Still, however, the question might be raised, whether an aneurismal tumour, pressing on the trachea, might not in this case produce symptoms simulating obstruction of the larynx? It is well known that such cases have occurred, and that skilful surgeons have been misled so far as to resort to the operation of tracheotomy for their relief.

Though a careful exploration of the chest could not detect the

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existence of such a tumour, it could not be thence inferred beyond a doubt that no such tumour did exist; for it is also known that aneurism has occurred in this connection without any physical signs by which its existence could be recognized. The question of diagnosis, therefore, lay between the existence of a morbid growth within the larynx and an aneurismal tumour pressing on the trachea.

The peculiar sensations referred by the patient to the larynx, and the constancy and long-continued existence of the aphonia, which in aneurism pressing on the trachea is occasional and exists only during the paroxysms of dyspnœa, were features which, taken in connection with the absence of all physical signs of aneurism, rendered it almost certain that a morbid growth within the larynx, and not aneurism, was the disease for which our treatment must be applied.

Upon this conclusion, the treatment proposed, which consisted in an operation upon the larynx itself, was based; and it was urged with the greater confidence from the consideration, that while it afforded the only means of rescuing our patient from impending suffocation in the event of the existence of a morbid growth, it would not shorten her life if no such condition were detected.

The conclusion thus arrived at, and the operation proposed for her relief, were fully explained to Madam L., who very naturally shrunk from the idea of submitting to so formidable a remedy. She was urged, however, to take it into consideration, and the belief was expressed that she would herself be forced by the urgency of her symptoms to solicit it.

Paroxysms of dyspnœa had of late recurred with increasing frequency, especially at night, and it was anticipated that the apprehension of instant suffocation, produced by these paroxysms, would render the patient desperate, and eager to submit to any measures that promised a possibility of relief. On Wednesday, the 30th day of April, Madam L.'s son called, to say that his mother had decided to submit to the operation, and wished that an early day might be appointed for its performance. Early on Saturday morning, the 3d of May, an urgent message came from Dr. Mitchell, requesting that the operation might be delayed no longer, our patient having been attacked the evening previous with a very alarming paroxysm, threatening immediate suffocation. Twelve o'clock at noon was therefore fixed upon for the operation—which was performed in the presence of Dr. Mitchell, of Brooklyn, Drs. J. Kearny Rodgers, Swett, Post, and Metcalfe, of this city, and Dr. Maulsby, of the United States Navy, and my pupil, Mr. W. Morrow Knox. Before the operation,

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Dr. Swett carefully auscultated the patient, and found the respiration everywhere clear, though feeble. The sound of obstruction in the larynx was heard alike in expiration and inspiration—no sign of the existence of an aneurism could be detected about the trachea at the summit of the lungs.

Operation.—The patient was seated before a window in a low arm-chair, with the head thrown back, and the front legs of the chair raised about three inches upon blocks. The neck being short and fleshy, the notch of the thyroid cartilage could only be obscurely distinguished by the touch. From this point an incision four inches in length was made, along the median line downwards, dividing the skin and subjacent tissues, till the cartilages of the larynx and the three upper rings of the trachea were laid bare—the latter being effected partly by lacerating, and partly by pushing downwards the isthmus of the thyroid body with the handle of the scalpel. After the hemorrhage had entirely ceased, the crico-thyroid membrane was incised, and the incision continued upwards in the mesian line with the utmost precision through the whole extent of the thyroid cartilage: at the moment of penetrating the larynx, air rushed in with a whizzing sound, and the voice became extinct. The thyroid cartilage being ossified, the division was made with strong scissors, curved edgewise. The section was then continued downwards through the cricoid cartilage, and the exposed rings of the trachea. The sides of the larynx were stretched apart with retractors, and brought into view its cavity, lined by growths attached to its lateral walls.

On the left side, two or three granules, half the size of grains of rice, hung pendulous from thread-like stalks. The remainder of the tumour was attached by a broad base, partly concealing the ventricle, and extending higher up upon the wall of the laryngeal cavity. The entire extent of the growth could not be traced, owing to the deep situation of the larynx, and the limited extent to which the sides could be separated from each other. Several portions of the tumour were snipped away, and, in cutting them, their substance appeared to be of a firm consistence, not unlike condylomata.

The hemorrhage from these incisions was of short duration, and was mostly prevented from flowing into the trachea by stuffing pieces of sponge, held by the forceps, into the open larynx, and keeping them there for a short time. A good deal of time was necessarily consumed in accomplishing this partial removal of the tumour, and the patient was much fatigued by the coughing excited by the flow of blood into the trachea. The flattened form of the tumours, and

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the breadth of their attachment, together with the depth of their situation, and the narrowness of the space within which the manipulations had to be performed, proved to be insurmountable difficulties, rendering the entire removal of the disease impracticable. It was therefore decided to suspend farther attempts for the present. A portion of the two upper rings of the trachea was removed on either side, in order to lodge the trachea-tube, which was introduced and secured in place by a tape passed round the neck. By this arrangement, respiration was rendered perfectly easy and comfortable. The patient displayed the most remarkable courage and firmness throughout the whole of this very difficult and protracted operation. When visited the same evening, considerable oozing of blood was found to have taken place from the lower angle of the wound, and the viscid secretion, expectorated through the tube, was also stained with blood. Her respiration continued easy.

Sunday, May 4.—Patient is quite comfortable—respiration is easy; the tracheal secretion continues abundant, viscid, and opaque, but no longer bloody. At twelve o'clock, Drs. Rodgers, Isaac Wood, A. C. Post, Swett, and Mitchell being present, a second attempt was made to remove the tumour. The edges of the wound being stretched apart, and the interior of the larynx exposed, several portions of the tumour were seized with dissecting-forceps, and snipped off with curved scissors. It was impossible, however, to expose sufficiently the upper part of the cavity, to allow the removal of the remotest portions of the tumour. I was therefore obliged, though reluctantly, to abandon again farther attempts at excision, and to content myself with the application of the acid nitrate of mercury to what remained of the tumour—which was effected by means of a small sponge, fastened to the end of an aneurism-needle. On reflecting farther on the course I had decided to pursue—of relying upon caustic, there appeared serious objections to repeating the application of it, inasmuch as it would be necessary to disturb parts already highly inflamed, and thus risk the most serious consequences. The single application of caustic already made could scarcely be expected to produce more than a partial effect upon the disease. I therefore came to the conclusion that it would be better to make one more attempt to get access, if possible, to the remotest portion of the tumour, situated high up in the cavity of the larynx. I had, in the mean time, ascertained, by having recourse to the cadaver, that, by extending the incision upwards to the os hyoides, and also splitting the base of the epiglottis to the extent of half an inch, very considerable ad-

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vantage could be obtained in exposing the upper part of the laryngeal cavity. On Monday, 5th, at half-past one P. M., I visited Madam L., with the intention of carrying into effect the procedure just described. I found her tolerably comfortable, although her cough had disturbed her a good deal, and prevented her sleeping. The tracheal secretion continued very abundant and viscid; the throat had become sore, and deglutition very painful. On again stretching apart the wound, a good deal of swelling was found to have taken place, rendering the cavity of the larynx still more remote and inaccessible.

The lining membrane of this cavity and of the trachea was very red and swollen, and the several parts of the tumour were rendered obscure and confused by a coating of lymph which had taken place upon them as well as by the swelling. A patch of false membrane, of the size of the thumb-nail, lined the posterior surface of the trachea. In this condition of things I consulted with Dr. Rodgers and my other colleagues present, as to the safety of proceeding, as I had intended, to a farther attempt to remove the tumour. It was thought best not to undertake it, but rather allow the external wound to heal up, and resort, if necessary, at a subsequent period, to a fresh operation. For the present the tube was directed to be left out till evening, and in the mean time everything was done to encourage our patient to sleep. At 7 P. M. I found Madam L. had slept four or five hours, and was much refreshed. The tube was again replaced in the trachea. Swallowing had now become quite impracticable, owing to extreme soreness of the throat, and when attempted everything was rejected through the wound and tube. Farther attempts to swallow were therefore interdicted, and enemata of beef tea ordered—a half pint every four or five hours. A sponge squeezed out in hot water was directed to be applied over the tube.

May 7. Patient is again able to swallow, if careful to attempt it only when she is free from cough: her pulse is 98 and good—the wound is diminishing in size—the enemata are to be discontinued.

Our patient's progress from this date continued favourable. The wound healed kindly around the tube, which was changed once in twenty-four hours. Deglutition again became easy, and her cough diminished in frequency and violence. To her great delight she discovered that, by closing the outer orifice of the tube, she could draw in air through the nostrils, and also blow her nose. The trachea tube being perforated with a large opening on the convex surface of its curve, allowed the current of air to pass to and from the lungs,

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through the larynx, when the outer orifice of the tube was stopped by a cork. This arrangement, as it also permitted the use of the voice, was a great satisfaction to Madam L., who was thus able to converse with her friends in a more distinct whisper, and with greater facility than before the operation. These beneficial results of the operations, alas! were not destined to be permanent. About the middle of August, Madam L. began to be sensible of returning obstruction in the larynx, which gradually increased till the slightest exertion or any emotion would disturb her breathing very much, and render it sonorous, especially during inspiration. Though the removal of the cork from the tube afforded immediate and complete relief of the dyspnœa, she was exceedingly averse to dispensing with it, inasmuch as it deprived her of the use of her voice, and when the cork was once removed it became difficult for her again to get accustomed to wearing it. Her general health still continued good.

As the obstruction of the larynx increased, Madam L., fully aware of the desperate nature of her disease, became anxious that another effort should be made to remove, if possible, the tumour entirely. On the 20th September I acceded to her request, in the hope that I might accomplish the desired object by modifying the operation in the way already proposed; that is, by extending the incision to the os hyoides.

The Second Operation was performed in the presence of Drs. Mitchell and De Wolf, of Brooklyn; Drs. Rodgers, Post, Swett, and Halsted, of the New York Hospital; Dr. B. F. White, of Greenwich, Conn., and Mr. W. Morrow Knox, as follows:—

The patient being placed as in the first operation, an incision was made from the upper margin of the opening occupied by the tube, along the median line, to within one inch and a half of the chin, and the dissection continued down so as to expose the whole extent of the larynx and the os hyoides. The edges of the wound, which consisted of cicatrix tissue and were very tense, had to be dissected up from the cartilages on either side and from around the opening in the trachea, to which they adhered very closely. The incision was also extended downwards to the distance of one inch below the opening for the tube. The tube was now removed, and the larynx split up throughout its whole extent, and the section prolonged to the os hyoides. On stretching apart the sides of the larynx with retractors the growths in the cavity were brought to view. Two or three pedunculated portions of the size of grains of rice floated loose in the cavity, and were readily torn away with forceps. The principal

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mass, however, was found of a flattened form and closely attached on either side to the ventricles, which were entirely concealed from view. They also spread forwards over the base of the epiglottis. Their extent could be better appreciated by the touch than by the sight. The passage of the rima glottidis was so much contracted by them that the end of the little finger could only be made to enter it from below upwards by considerable pressure. On passing the fore-finger through the mouth over the epiglottis, the tumours could now be distinctly felt for the first time at the orifice of the larynx. The extent of the disease thus ascertained was so formidable that we were forced to the painful conclusion that it would be impossible to accomplish its entire removal with the knife, and were therefore obliged to content ourselves with removing what was within reach. This was done by seizing with a clawed forceps all that could be got at, and paring it, piece-meal, with a small curved blunt bistoury. The passage of the rima glottidis was thus enlarged sufficiently to allow the finger to pass without resistance. The wound was now closed with three sutures above the tube. The tube was then replaced and the vapour of hot water was directed to be inhaled from a sponge. The patient bore this operation with the same fortitude as the first: she soon became tranquil, and passed the remainder of the day and the succeeding night comfortably.

September 26. Patient's progress is favourable. The wound above the tube has entirely healed by the first intention, and below it is contracting daily.

After the wound had entirely healed around the tube, a short time only elapsed before unequivocal signs of the recurrence of the disease manifested themselves. Dyspnœa became easily excited by exercise or any emotion, and the tumour was more readily felt by the finger passed into the mouth. Early in the month of January, increasing difficulty was experienced in replacing the tube after its removal, and patient was sensible of its being crowded more and more towards the left side, where it had excited a good deal of soreness in the trachea. Considerable hemorrhage was often caused by changing the tube. Madam L. could still speak in a distinct whisper with the outer orifice of the tube closed, though she was no longer able to wear the cork constantly in the tube. Her general health still continued unimpaired, though her spirits were more depressed. The tumour not only encroached by its growth upon the tube, but the tube itself gradually worked its way upwards in consequence of the chin being drawn downwards by the contraction of the extensive

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cicatrix. As this mutual encroachment was steadily increasing, a its irritating effects constantly augmenting, I proposed to Mada. L. the only resource that remained for the temporary relief of her truly deplorable condition, which was to make a new opening in the trachea, as low down as possible, where the tube might be inserted beyond the reach of the tumour.

To this proposal she readily assented, and at two o'clock P. M., on the 7th January, 1852, the operation was performed as follows:—

Third Operation.—The patient being in the same position as in the previous operations, and with the tube remaining *in situ*, an incision of four to five inches in length was made in the median line, commencing at the existing tracheal opening and extending downwards upon the sternum. The division of the integument and subcutaneous adipose and cellular tissue exposed the notch of the sternum below and the trachea above, with the thyroid body and sterno-hyoid and thyroid muscles investing it. The chin being drawn down towards the sternum by the longitudinal contraction of the cicatrix, rendered the trachea very deep and difficult of access. In order to stretch apart the edges of the wound, it was necessary to dissect up the closely adherent skin and condensed cellular tissue from around the tracheal opening. The next step was to lay bare the trachea, and make a new opening, as low down as possible. This was done partly by dividing, partly by stretching and tearing the isthmus of the thyroid body, after which the trachea was punctured transversely between two rings; the lower edge of the slit was seized with clawed forceps, and a semicircular piece was circumscribed and excised with a narrow scalpel. The space separating the new from the old opening, was more than half an inch. The tube was removed from the old opening, and a fresh tube introduced into the new one, and secured in the usual way. The patient at once experienced a great improvement in her feelings about these parts. The lower half of the wound was closed by four sutures and adhesive straps.

The wound again healed kindly around the tube, as it had done before. The same disposition of the tube to work upwards towards the larynx showed itself as the new cicatrix contracted, and drew down the chin. The difficulty in changing the tube was again renewed as before. Two distinct ridges were perceptible, over which the end of the tube rubbed whenever it was introduced. Difficulty in swallowing also steadily increased, till Madam L. was obliged to forego all solid food and restrict herself to liquids. She also suffered severely from earache.

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In the month of March, she discharged from her throat a hard substance, of a dark colour, and of the form of an eye-tooth, which she at first suspected might be a tooth, though she was not aware of having any decayed teeth at the time. From her description, I was led to regard this substance as one of the arytenoid cartilages in a state of necrosis. It had probably come from the right side of her throat, where she had felt for some time previous a pricking sensation. In April, the obstruction in the larynx had so far increased that the voice became extinct, and patient was obliged to make use of a slate and pencil to express her wishes. The difficulty of swallowing also continued unabated. In May, the difficulty of swallowing had increased to such a degree, that the patient actually suffered from hunger, notwithstanding which she still retained in a good degree her florid complexion and plump appearance. For several weeks preceding, there had been almost daily a considerable discharge of bloody fluid from the throat, as well as from the tube, and more recently this discharge had become fetid, and of a decidedly gangrenous odour; portions of a tough fleshy substance were also brought up from the throat.

In June, the patient's condition had become truly deplorable; she still, however, retained to a remarkable degree the appearance of health, and kept about her house. The increasing impediment to deglutition allowed only a very scanty supply of liquid nourishment to be introduced into her stomach. She was harassed, especially at night, by her cough, which had never left her since it first made its appearance. The tube was more and more encroached upon by the growth within the larynx, and its outer orifice, instead of looking directly forwards, was deflected to the left side. The fetid bloody discharge already noticed continued abundant. Patient's condition steadily grew worse, but no new feature presented itself till the 4th of August, at 3 o'clock in the morning, when she came into her daughter's room, earlier than usual, to have the tube changed. She had herself already removed the tube which she had worn through the night, and was very urgent to have a fresh one introduced with all possible dispatch, as suffocation seemed impending. Her daughter became exceedingly alarmed and agitated at her situation, and failed in her attempts to replace the tube. In a few seconds her mother sunk down into a chair and expired.

Dissection.—The larynx and trachea, in connection with the tongue and œsophagus, were removed. On laying open the œsophagus posteriorly, the morbid growth was found to have spread

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over the orifice of the larynx down upon the anterior wall of the pharynx as far as the lower edge of the cricoid cartilage, and presented a moderately elevated, flattened tumour, of a grayish colour and firm consistence, with well-defined edges. It appeared to be deposited in the submucous cellular tissue in small agglomerated lobules. The œsophagus at this point was very much contracted, to which was doubtless to be attributed the extreme difficulty of deglutition that had existed so long before death. Everywhere else the œsophagus had a healthy appearance. On laying open the larynx and trachea posteriorly, the entire cavity of the larynx was found to have been invaded by gangrene, which had destroyed the morbid growths occupying it, and exposed the cartilaginous walls in a state of necrosis. The arytenoid cartilages had entirely disappeared, and all that remained of the epiglottis was about one-third of its length at its summit, which was only held *in situ* by the investing mucous membrane passing from its lingual surface to the base of the tongue. No traces of the ventricles remained, and the aryteno-epiglottic folds were destroyed, as well as the mucous membrane lining the pharyngeal gutters which lie to the outside of these folds. On the right side, the destructive process had extended forwards to the base of the tongue, a little in advance of the epiglottis. The entire surface involved in the gangrene was uneven and shaggy, presenting a dark grayish-brown colour, with loose shreds of cellular tissue and black spiculæ of necrosed cartilage projecting into the cavity of the larynx. The aperture through which the tube had passed was of the same sloughy appearance. The circumference of the morbid growth which had escaped gangrene presented a thickened, callous, and everted margin, closely resembling the edges of a scirrhous ulcer. The lower limit of the gangrenous ulceration did not extend beyond the first ring of the trachea, from which point downwards the mucous membrane was smooth and healthy. The rings of the trachea, from the first downwards, were entire, and retained no trace whatever of the openings that had been made for the tube; the tube having worked its way gradually upwards from the trachea back to the larynx. The examination was necessarily restricted to the parts just described.

Microscopic Examination.—H. Van Arsdale, M. D., microscopist to the New York Hospital Museum, has kindly furnished the following note of his examination of the morbid growth after death, which corresponds exactly in its results with his and Dr. Swett's ex-

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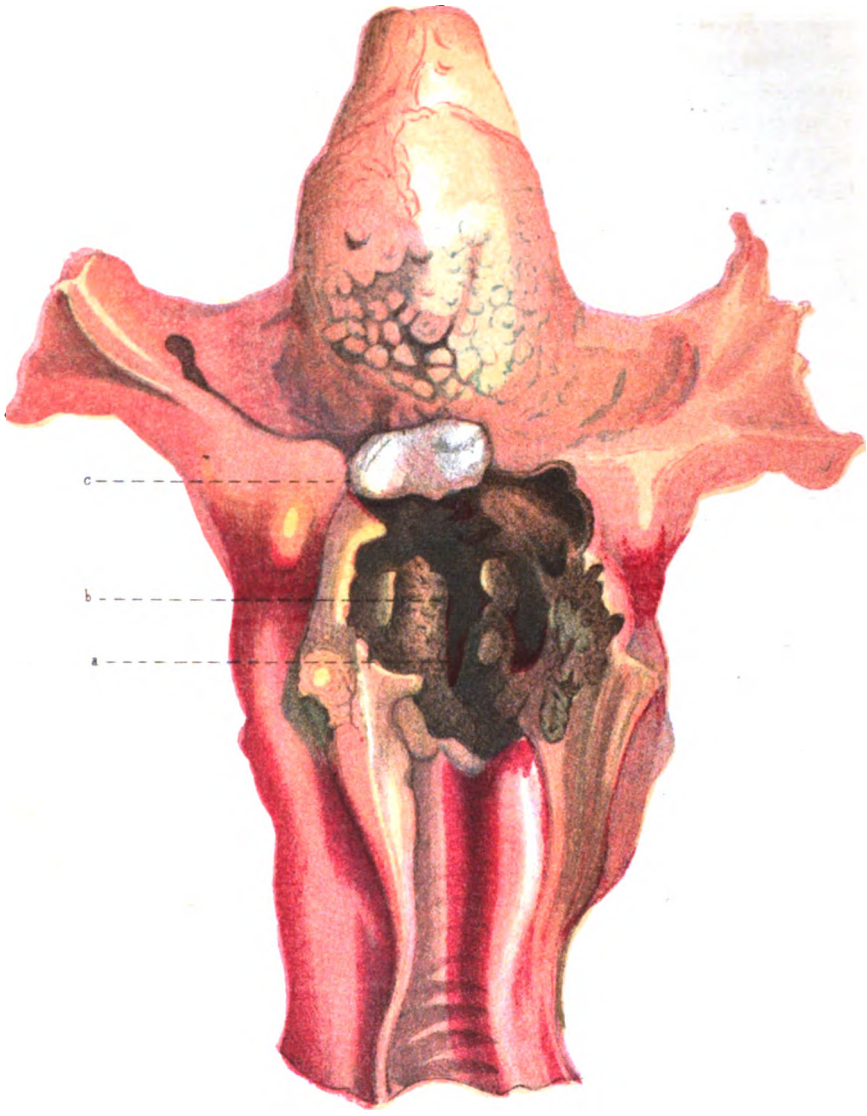

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amination of a specimen of the tumour removed by the second operation.

"On examining with the microscope the nature of the morbid growth in the larynx, I find it attached to the inner surface of that organ by a loose fibrous tissue. The meshes of the tissue are filled up with epithelial cells and small globules of free fat. Near the surface of the tumour the fibres gradually disappear, and the structure then consists entirely of epithelial cells and free fat. The structure may, therefore, be regarded as non-malignant *per se*, and to belong to that class of morbid growths known as epithelial."

The recurrence of gangrene is a feature perhaps unique in respect of the larynx, and one which alone could have taken place in the peculiar circumstances which this case presented.

Without an opening in the trachea, through which respiration could be performed, these growths would have produced suffocation long before they could have attained such a size as to be compressed by the unyielding cartilaginous walls of the larynx, to which compression the occurrence of the gangrene is doubtless to be attributed.

The opposite lithographic drawing illustrates the case of Madam L. just narrated.

The larynx and trachea are laid open posteriorly.

The walls of the laryngeal cavity are in a state of gangrene, with spiculæ of necrosed cartilage standing out from their surface.

a. A small portion of the thyroid body exposed, but not invaded by the gangrene.

b. The aperture through which the trachea tube was inserted.

c. The remains of the epiglottis still held *in situ* by the glosso-epiglottic frænum.

Since the publication of Professor Ehrmann's work, the attention of the profession in this country has been farther directed to this subject by a treatise on the surgical treatment of "Polypi of the Larynx, by Horace Green, M. D., New York, 1852," in which Professor Ehrmann's monograph has been for the most part reproduced, and three additional cases recorded that came under the author's own observation. In one of these cases the polypous growth was expelled from the cavity of the larynx in the act of coughing, and brought within view at the side of the epiglottis, where it was seized by a double hook and excised by Dr. Green. This operation, so creditable to the operator, was followed by complete success. It is worthy of note that the first case of this disease published in this

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country, was by Dr. John C. Cheesman of this city, in the *Transactions of the Physico-Medical Society of New York*, 1817, vol. i. p. 413.

These cases, together with seven others, including the one which is the subject of this paper, form an addition of eleven to the number reported by Ehrmann; the whole constituting an aggregate of forty-two cases, from which the accompanying tabular view has been constructed. In many of these cases the reports are extremely deficient in details; and in some no report whatever accompanies the specimen. Notwithstanding these defects, useful deductions may be drawn from these materials, illustrating the nature and treatment of this very important, though happily rare disease. Case 4, reported by Dr. Green, has been excluded from this category, inasmuch as the disease originated in the pharynx, and only affected the larynx secondarily.

Sex.—The greater frequency of the disease in the male than the female sex, appears from the fact, that of 38 cases in which the sex is stated, 27 were males and 11 were females.

Age.—The ages of 35, in which it is stated, range from 2 years to 65 years; 4 were below 5 years; 5 between 5 and 12, 1 was 18 years, 1 was 30 years, 1 was 33 years; 8 between 35 and 45; 9 between 50 and 65; 6 were reported as young, aged, or adult.

Causes.—In three of the cases of children of 2, 6, and 10 years of age, the disease originated in croup, which was not fairly recovered from, but became chronic. In one of 11 years, the earliest symptoms of the disease were preceded by hooping-cough. Tubercles existed in three cases, and in one syphilis is assigned as the cause of the disease. In all the other cases no cause is assigned, and we are left to the inference that there existed no appreciable cause.

Symptoms.—The essential symptoms are those which affect the functions of the larynx as the organ of voice and the channel for the transmission of air to and from the lungs. Deglutition, dependent as it is upon the healthy action of the larynx for its due performance, is necessarily disturbed more or less by its deranged actions.

1. *Voice.*—A change in the voice is generally the earliest symptom that manfests itself in this disease. It is reported to have existed in 24 out of the 42 cases, and in 19 of these 24 was the first symptom that occurred. Though no mention is made on the subject in the remaining 18 cases, there is nothing in their history to authorize the inference that it did not exist. The hoarseness gradually

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progresses till it results in complete aphonia, and often persists for months before any disturbance in the respiration supervenes. In cases 19 and 40, eighteen months elapsed before dyspnœa was superadded to aphonia.

2. *Respiration.*—Symptoms of obstructed respiration sooner or later supervene upon the changes in the voice. Dyspnœa is stated to have occurred in 38 out of the 42 cases collected. Generally the dyspnœa is constant, and as the disease advances is accompanied with suffocative paroxysms. Case 33 is, however, an exception in which, though dyspnœa is constant, there were no suffocative paroxysms; also in cases 14 and 23 there were suffocative paroxysms, but no dyspnœa in the intervals. This latter peculiarity was found on dissection to be explained in case 14 by the position of the tumour, which had its long, pedunculated attachment at the superior orifice of the larynx, and was floating, so that when it lay external to the cavity of the larynx, respiration was free, and only became obstructed when the tumour was drawn within the larynx. Sometimes the impediment is greatest during inspiration, sometimes during expiration, and at other times it occurs alike in both. The suffocative attacks are accompanied with a hoarse croupy laryngeal sound which is sometimes whistling. These attacks are also subjected to the nocturnal influence.

3. *Cough.*—Cough is reported to have occurred in only 16 cases; no doubt it occurred more frequently than this small proportion would imply. The deficiency of details in many of the cases reported explains the omission. The cough presents no uniformity in its character; in some instances it is slight and infrequent, in others very violent, and attended with a hoarse, croupy, or laryngeal sound, which is also sometimes whistling.

4. *Sensations in the Larynx.*—Patients refer to the larynx itself as the seat of obstruction, and sometimes have a sensation as of a body moving up and down in its cavity. Sometimes, also, a peculiar cluck or valvular sound can be heard in the larynx. In 14 cases, the sensation of obstruction was enumerated among the symptoms, and in two of this number a motion as of a valve, with an accompanying sound, was perceived.

5. *Deglutition* is generally more or less embarrassed, and when attempted it not unfrequently provokes a suffocative paroxysm. In 8 out of the 7 cases in which dysphagia is noticed, the patient could only swallow liquids.

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Form of Tumour—its Nature, &c.—These growths are variously designated as mucous polypi; solid, fleshy tumours; hard, white, fibrous tumours; cauliflower excrescences with rounded granular surface, resembling syphilitic vegetations at the verge of the anus, and around the corona glandis; warty excrescences and verrucal.

Examined by the microscope they are found to be composed of fibrous tissue, epithelial cells, and globules of free fat, and hence are not malignant in their nature. In one instance, case 27, the growth is described as a "pulpy medullary tumour," which would imply that it was of a cancerous nature. From their form these growths may be arranged into two distinct classes.

1. The polypoid, comprising the smooth globular or elongated tumours attached by a slender stem, which renders them floating and of which cases 6, 7, 9, 14, 18, 28, 31, 32, 34, 36, and 41 are examples. 2. Vegetations embracing cauliflower excrescences, and all others with granular surface resembling syphilitic vegetations. A large proportion of this second class have more or less pedunculated attachments, which would facilitate their excision; the remainder are sessile, and spread out over a broad surface, as in case 40.

Seat of Attachment.—According to their various seats of attachment, these growths may also be arranged in three classes.

Class I.—Those attached to the aryteno-epiglottic fold, case 14, are an example of this class, in which the floating polypoid tumour while occupying the pharynx, left the breathing undisturbed, but on being drawn into the larynx, at once excited suffocative paroxysms. In case 42, which is represented in figure 2, the bulky growth which belongs to the class of vegetation, completely fills up the right pharyngeal gutter, and is attached to the outer aspect of the corresponding aryteno-epiglottic fold.

Class II. is much the most numerous class, and embraces those tumours which originate from the walls of the laryngeal cavity. The vocal cords and ventricles are their favourite seats of attachment, and such was found to be their origin in no less than seventeen cases. From these points, the surface of attachment may spread in every direction; forwards over the base of the epiglottis, as well as upwards and downwards. In cases 10, 16, 23, the surface of attachment spreads upwards from the ventricle to the aryteno-epiglottic fold. In cases 24, 26, 28, the growth spread downwards to the third tracheal ring. In cases 32 and 34, the tumours originated so near the upper orifice of the laryngeal cavity, that they were projected from the cavity by coughing, and brought into view in the fauces.

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In case 18, the tumour occupied the left ventricle and projected into the cavity of the larynx, and at the same time developed itself externally between the thyroid and cricoid cartilages at the expense of these cartilages, forming a second tumour which occupied the pharynx.

Class III.—Examples of this class comprising tumours originating in the trachea are very rare. Case 2, in which the tumour was attached at the upper part of the trachea, and case 28, in which it was attached to its anterior wall, half an inch below the cricoid cartilage, are the only instances.

Other Lesions of the Larynx.—In three cases, 8, 36, and 40, there was ossification of the cartilages. In case 8, besides ossification of the cartilages, the epiglottis was loaded with fat. In case 13, there was circumscribed caries of the thyroid cartilage produced by the pressure of the tumour. In case 40, besides ossification, there was subsequent necrosis and gangrene of the cartilages. In case 26, there was thickening of the laryngeal parietes and external swelling in the region of the larynx. In case 18, there was œdema of one aryteno-epiglottic fold, and in case 11 of both.

Duration of Disease.—Of the twenty cases in which the duration is mentioned, one occupied twenty weeks, and another twelve years. The intermediate periods were as follows: 1, several weeks; 1, several months; 2, six months; 1, nine months; 2, eighteen months; 4, several years; 2, two years; 1, three years; 2, three and a half years; 2, six years.

Mode of Termination.—In twenty-six of the fatal cases, in which the mode of termination is stated—one died of pneumonia after tracheotomy, six of gradual suffocation, and nineteen of sudden suffocation. In case 7, the suffocative attack that proved fatal was the first and only attack that occurred.

Diagnosis.—Hoarseness, or some other change in the tone of the voice is the earliest symptom indicative of the growth of a foreign body within the larynx. So long as this is the only symptom, it is scarcely possible to diagnosticate the disease. Sooner or later, however, dyspnœa and suffocative paroxysms supervene, and are aggravated by the noctural influence. A sensation of obstruction in the larynx itself is almost always experienced at this period by the patient; and to this obstruction is referred all his trouble. Neither of these symptoms, however, can be regarded as pathognomonic; they are all common to other diseases of the larynx. The only condition strictly pathognomonic is either the actual expulsion by cough.

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ing of a portion of the tumour itself, as in cases 8 and 41, or the discovery of the tumour by inspection, as in cases 32 and 34, or by the touch, as in cases 16 and 40. In the absence of these unequivocal tests, the diagnosis is to be deduced from a careful consideration of the history of the case in question, and a critical comparison of its symptoms with those produced by all the known causes of obstruction. In fact, the first question to be decided is, whether obstruction of the larynx be the cause of the symptoms, or some other conditions of the system simulating it. In deciding this preliminary question, we shall be materially aided by the application of Dr. Marshall Hall's test, as to the patient's ability to inhale or exhale air through the nostrils. Inspection of the throat, and exploration of the orifice of the larynx with the end of the forefinger, should never be omitted. Case 14 strikingly illustrates its importance, inasmuch as the nature of the disease would thus have been discovered, and a sure mode of relief suggested.

Supposing it to be decided that obstruction does exist, the mode of distinguishing the particular nature of the obstruction will be best illustrated by the course pursued in the interesting case related in this paper, and to which the reader is referred:—

Prognosis.—The prognosis in this disease, if left to itself, is almost necessarily fatal. In one solitary instance (case 41), the efforts of nature were competent to detach and expel the foreign growth, the expulsion being followed by recovery. In another case (8), several portions of the tumour were expelled by coughing, and temporary relief was thus afforded to the respiration and voice. In a third case (28), the tumour was found detached and free in the trachea; but suffocation took place before it could be expelled. In the progress of the disease, when once suffocative paroxysms have become established, the danger of suffocation is imminent, and the catastrophe may occur at any moment.

Treatment.—Surgical aid is our only resource for the relief or cure of this formidable disease, and the particular mode of applying it must be determined by a careful consideration of each individual case. If the tumour be inserted into one of the aryteno-epiglottic folds at the orifice of the larynx, by a narrow attachment, as in cases 14 and 42, or within the cavity of the larynx, so near to its orifice as to admit of its being expelled from the cavity, and brought within reach, as in cases 32 and 34, it should be excised through the mouth, as was successfully done by Dr. Green in case 32. Only a

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small proportion of cases, however, has hitherto presented the conditions to which this mode of operating would be applicable.

By far the largest number admit of relief only by Professor Ehrmann's operation, which consists in laying open the cavity of the larynx throughout its whole extent, and excising the growths, the trachea being also opened at its upper part, and a tube inserted to maintain respiration.

Although the same complete success did not follow this operation, in the case herein related, as in that of Professor Ehrmann, still, the experience afforded by it has served to establish the value of the operation, and it will doubtless be conceded that the prolonging of the patient's life for fifteen months was no unimportant achievement. It should also be remembered that the extensive attachments of the morbid growths to the walls of the larynx, which proved an insurmountable obstacle in this case, is not the condition most frequently met with in this disease. In the majority of cases, the growths are attached by a narrow base admitting of entire removal.

Where suffocative paroxysms have become established, no time should be lost in opening the trachea and inserting a tube, whereby the distressing dyspnœa is at once relieved, and the danger of suffocation averted.

The laying open of the cavity of the larynx, with the view of extirpating the tumour, may be resorted to at a subsequent period, more or less remote from the operation of tracheotomy, and the question of its performance may be left to the option of the patient; yet I conceive it to be incumbent on the surgeon to urge it, notwithstanding the uncertainty of its complete success. Should it be found impracticable, after exposing the laryngeal cavity, to remove the entire disease, I would advise a thorough cauterization of the morbid growths with the acid nitrate of mercury, before closing the wound. After this, the application of the dilute acid nitrate, or a strong solution of nitrate of silver, may be repeated by the mouth, for the purpose of completely destroying the disease, or at least of repressing its growth.

In case 33, Dr. Green made use of the solution of nitrate of silver, of the strength of one drachm to the ounce, and repeated the application daily for about four weeks, by means of a sponge saturated with the solution, and conveyed through the rima glottidis. His patient was very much benefited, so that his respiration became easy and unobstructed. His voice was also improved, though not restored to its natural tone. At the expiration of four years, he still con-

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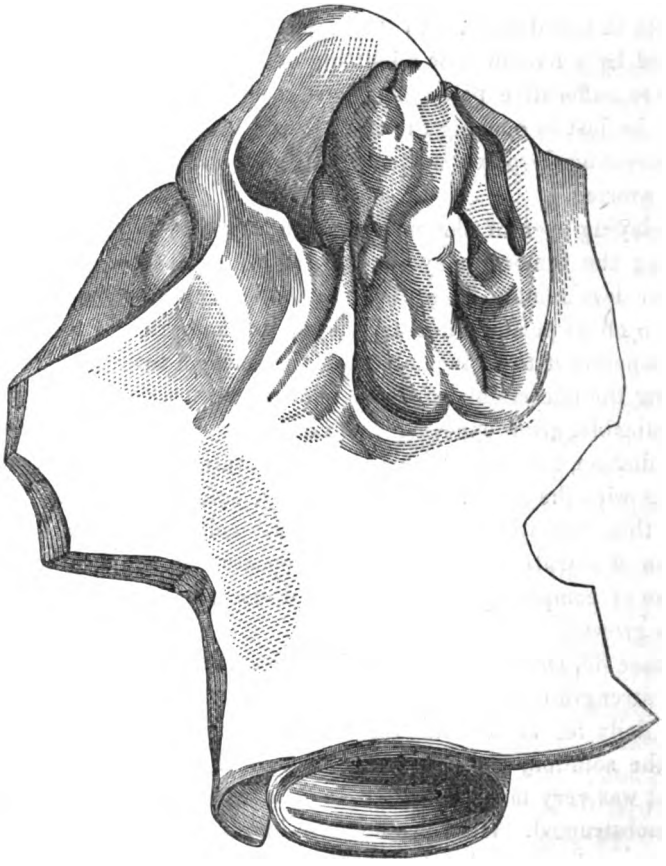
tinued to enjoy the full benefit derived from the above treatment. In regard to this treatment by caustic *per vias naturales*, it should be borne in mind that, in the case reported by Dr. G., which is the only example of this treatment, though there existed dyspnœa and aphonia, there were no suffocative paroxysms. Where this latter symptom is present (and it is very seldom absent), danger of suffocation would be greatly enhanced by the application of caustic. For the greater safety of the patient, tracheotomy should be performed as a preliminary to the use of caustic. In every case in which a trachea tube is inserted, the tube should be perforated with a large opening on the convex side of its curve, in order that, by closing its outer orifice, the patient may at pleasure use his voice and expel sloughs and other discharges from the larynx when caustic applications are being made.



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The preceding cut illustrates case 42 of the table. The specimen belongs to the Pathological Museum of the New York Hospital. Nothing is known of its history. The tumour, which is of the size of the last phalanx of the thumb, is attached to the outer layer of the right aryteno-epiglottic fold, fills up the corresponding pharyngeal gutter, and almost closes the entrance to the larynx.

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A Tabular View of Forty-two Cases of

Case.	Where reported, and by whom.	Sex.	Age.	Cause.	Voice.	Respiration.	Cough.	Sensations in the larynx.	Deglutition.
1	Lieutaud, Hist. Anatom. Med., lib. iv. obs. 63, 64.	M.	30	Asthmatic.			Frequent.	Obstruction.	
2	Idem.	M.	12	Phthisical.					
3	Desault, Œuvres Chir., tom. ii. p. 254.	M.				Alternately free and obstructed with suffocative paroxysms.		Obstruction.	
4	Idem.					Do. Do.		Obstruction.	
5	Idem.					Do. Do.		Obstruction.	
6	Pelletan, Clin. Chir., tom. i. p. 15, obs. 8.	M.	35			Suffocative paroxysms.			
7	Dr. Schultz, of Deux Ponts, private correspondence.	M.	6	Croup, gradually becoming chronic.	Hoarse from outset.	First and only suffocative paroxysm proved fatal.			
8	Otto Seltene, Beobach. zur Anatomie, &c., Berlin, 1824.	M.	65		Hoarseness several years, gradually progressing to complete aphonia.	Difficult.	Slight and infrequent	Obstruction.	Embarrassed.
9	Andral, Clinique Méd., Paris, 1834.	M.		Phthisis.	Nearly extinct.			Obstruction and constriction.	
10	Idem.			Phthisis.					
11	Senn de Genéve, Jour. des Prog., &c., tom. v. p. 230.	M.	Adult.		Hoarseness for six years and aphonia.				Difficult and restricted to liquids.
12	Dr. Schultz, de Deux Ponts, private correspondence.	F.	44		Hoarseness for several weeks.	Very laboured, & with suffocative paroxysms.	Croupy.	Obstruction.	
13	Urner, Dissert. de Tumorib. in Cavo laryngis, Bonn. 1833.	M.	54	Phthisis.	Hoarseness at outset; afterwards aphonia.	Difficult alike in inspiration and expiration; suffocative paroxysms.	Violent.	As if a cork blocked up the trachea.	Difficul
14	Dupuytren, Leçons, Orales. tom. iii. p. 602.	M.	Old.			Tranquil, except during suffocative paroxysms.			
15	Dr. Brauers, de Lauvain, Journ. de Græfe et Walther, 1834, vol. xxi. p. 534.	M.	40			Laboured.		Obstruction.	
16	Rayer, Maladies de la Peau, tom. ii. p. 422.	F.	50			Suffocative dyspnœa with laryngeal whistle.			
17	S. Dawosky, Journ. de Med. Prat. de Hufeland, tom. lxxx. 2d part, p. 78.	M.	10	Repeated attacks of croup.	Hoarseness for six months preceding.	Dyspnœa.	Croupy.		
18	Trousseau et Belloc, Traité de la Phthisie Laryngie, p. 41.	M.	42		Changed for six months preceding the appearance of noise in inspiration like the roaring of a lion.	Dyspnœa and suffocative paroxysms alike in inspiration and expiration.			
19	Gérardin, Lecture faite à l'Acad. de Medicine, Sept. 27, 1836.	M.	63		Sudden loss of voice 18 months before dyspnœa supervened.	Suffocative paroxysms; whistling sound in inspiration.	Hoarse.		
20	Ch. Mayo, in Ryland, Treatise on Diseases of Larynx, &c.	F.	Adult.			Suffocative paroxysms.			
21	Ryland, Treatise, &c.	F.	Adult.	Phthisis.					
22	C. H. Ehrmann.	M.	9		Changed, and for some months unaccompanied by any other symptom.	Oppressed.	None.		

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Morbid Growths within the Larynx.

Duration of disease.	Mode of termination.	Form and nature of tumour.	Seat of attachment.	Other lesions of larynx.	General remarks.
	Sudden suffocation.	Polypus from several roots.	Near the glottis.		
	Sudden suffocation.	Floating polypus with single pedicle.	Upper part of trachea.		
	Sudden suffocation.	Pyriform and pedunculated.	One of the ventricles.		
		Do. Do. Do. Do.	Do. Do. Do. Do.		
	Sudden suffocation.	Solid tumour, size of small nut, floating with slender pedicle.	Near the glottis.		
20 w'ks	Sudden suffocation.	Floating fleshy polypus with narrow pedicle.	Base of epiglottis.		
Several years	Gradual suffocation.	Fleshy, notted tumour, of large size, with slender pedicles composed of three excrescences united, one above the glottis, two below.	To the inferior vocal cords of both sides.	Larynx almost entirely ossified; epiglottis loaded with fat.	Three times expectorated globular fleshy masses with temporary relief of respiration and voice.
		Reddish, soft, pedunculated; like uterine polypus.	Bottom of one of the ventricles.		
		Cauliflower tumour, of size of large pea, like syphilitic vegetations of the rectum.	Surface of mucous membrane from superior orifice to vocal cord.		
6 years	Sudden suffocation.	Whitish, hard, fibrous, fissured, pedunculated tumour, of size of large nut.	Right ventricle, and nearly filling up the supra-glottic portion of the laryngeal cavity.	Slight œdema of the aryteno-epiglottic folds.	
Several weeks	Sudden suffocation.	Fleshy, movable excrescence.	From base of epiglottis to top of trachea, completely obstructing the larynx.		
18 mo's	Sudden suffocation.	Round, fleshy tumour, of size of small nut.	Both inferior vocal cords; right ventricle nearly obliterated, the left narrowed; thyroid cartilage excavated for partial lodgement of tumour.	Thyroid cartilage carious at the point of excavation, but not in its entire thickness.	
	Sudden suffocation.	True polypus, 18 lines in length, bifurcated at free extremity, floating in pharynx.	Aryteno-epiglottic fold of one side.		Doubtless could have been seen and felt.
	Expected to be fatal.	Verrucous tumour.	Filling the larynx.		Laryngo-tracheotomy was performed, and caustic employed.
		Vegetations.	To the left superior vocal cord, and from thence upwards to above the orifice of the larynx.		Perceived by the finger per oram.
	Gradual suffocation.	Small and large pedunculated excrescences of almost cartilaginous hardness.	Anterior wall of laryngeal cavity, and completely filling it up.		
18 mo's	Pneumonia 5 d'ys after operation of laryngotomy.	Mucous polypus.	Filling left ventricle, and projecting into cavity of larynx; prolonged also externally into the pharynx between the thyroid and cricoid cartilages.	Œdema of left aryteno-epiglottic fold.	Tracheotomy relieved, temporarily, all oppression.
2 years	Gradual suffocation.	Large-sized tumour, white, covered with asperities attached by short pedicles.	Right ventricle; the tumour nearly filling the laryngeal cavity.		
	Sudden suffocation.	Whitish, firm, elastic polypus.	Base of epiglottis.		
		Small polypus, with narrow pedicle.	Left inferior vocal cord.		
6 mo's	Gradual suffocation.	Rounded, smooth, lobulated, fleshy tumour, of size of filbert.	Entire extent of left inferior vocal cord, and filling up the rima.		

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A Tabular View of Forty-two Cases of

Case.	Where reported, and by whom.	Sex.	Age.	Cause.	Voice.	Respiration.	Cough.	Sensations in the larynx.	Deglutition.
23	Rendtorff, Dissertation Inaug., Heidelberg, 1840.	M.	18	An abundant secretion of mucosity.		Frequent efforts to expectorate, during which *alone* respiration was difficult.	None.	No rale in inspiration.	Sore throat of recent date.
24	Gottl. Gluge, Abhandlungen zur Physiologie und Pathologie, &c. &c., Jena, 1841.	M.	50		Aphonia.	Dyspnœa.			
25	Idem.	M.	Adult.						
26	Idem.	F.	40		Very much changed.	Dyspnœa.			
27	Nasse, in Alber's Anatom. Patholog. Atlas, Pl. viii. Fig. 8.	M.	Adult		Hoarseness for several years.				
28	Stallard, Lond. Med. Gaz., 1844.	F.	40			Intense dyspnœa.	Cough.		
29	C. H. Ehrmann.	F.	38		Rough & hoarse; finally aphonia.	Suffocative paroxysms.	Cough and expectoration of portions of tumour.	Obstruction, and sound of a valve opening and shutting.	Fits of coughing excited by deglutition.
30	Ruff, Médecin à Strasburg, loc. cit.	F.	4⅛		Hoarseness.	Laboured and whistling.	None.		
31	Bertherand, Prof. à Strasburg, loc. cit.	M.	60		Occasional hoarseness for 2 years, progressing to aphonia.	Dyspnœa, at first occasional, then constant, and accompanied with suffocative paroxysms and whistling sound.	Laryngeal cough.		
32	H. Green, on the Surgical Treatment of Polypi of the Larynx, New York, 1852.	F.	31½		Huskiness going on to aphonia in 5½ years.	Difficult for a long time, and at length accompanied with suffocative paroxysms.	Laryngeal cough.		
33	Idem.	M.	42		Slight change, going on to complete aphonia.	Considerably embarrassed, and with hissing noise, but no suffocative paroxysms.	Slight cough.	Constriction and obstruction.	Difficult.
34	Idem.	F.	Adult.		Hoarseness.	Difficult, with paroxysms of suffocative dyspnœa.	Cough.		
35	Dr. J. C. Cheesman, in Transactions of Physico-Med. Society of N. York, vol. i. p. 413, 1817.	M.	4			Asthma.	Cough.		
36	W. Parker, M. D., N. Y. Journ. of Med., &c., new series, v. viii. No. 1.	M.	60		Hoarseness gradually increasing.	Dyspnœa; suffocative paroxysms.			Restricted to liquids.
37	Idem.	M.	2	An attack of croup 8 months previously, from which he never recovered.	Hoarse.	Difficult and suffocative.	Croupy cough.		
38	Boston Soc. for Med. Improvement, in Amer. Journal Med. Science, July, 1851, p. 37.	M.	50		Hoarseness, wheezing, and at length aphonia.	Dyspnœa.			
39	Watson's Lectures, Philad. 1845, p. 514.	M.	11	Hooping-cough.	Aphonia.	Difficult, & with suffocative paroxysms.	Cough accompanied by whistling sound.		

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Morbid Growths within the Larynx—Continued.

Duration of disease.	Mode of termination.	Form and nature of tumour.	Seat of attachment.	Other lesions of larynx.	General remarks.
	Sudden suffocation.	Homogeneous, solid, fleshy tumour.	To whole length of left aryteno-epiglottic fold, and spreading down to superior vocal cord.		Could have been felt, and perhaps seen.
	Sudden suffocation.	Hard, white, fibrous tumour, size of pigeon's egg.	Posterior wall of larynx from left arytenoid region to third ring of trachea, filling its cavity.		
	Sudden suffocation.	Gray-whitish excrescence of mucous membrane.	Right vocal cords, and filling laryngeal cavity, pushing to one side arytenoid cartilage and epiglottis.		
Several years		Tumour of mucous membrane.	From orifice to third tracheal ring; left vocal cords and ventricle effaced; arytenoid cartilages and muscles confounded with tumour.	Thickening of laryngeal partition; external swelling of larynx.	Laryngeo-tracheotomy was performed; patient survived only short time, and died suddenly.
Several years		Pulpy medullary tumour.	Left inferior vocal cord.		
	Sudden suffocation.	A detached polypus of size of an almond, with pedicle of three-fourths of an inch	Anterior wall of trachea half an inch below cricoid cartilage.		
3½ y'rs	Typhus fever.	Cauliflower-shaped polypous excrescence, the surface presenting rounded fleshy granulations.	Left inferior vocal cord.		Laryngeo-tracheotomy performed, and tumour excised successfully.
Several months	Gradual suffocation.	Nine separate, rounded, cauliflower polypi, of size of peas, like syphylitic vegetations on the corona glandis.	From upper third of epiglottis to below vocal cords; ventricles free.		
2 years	Suffocation.	Fusiform, pedunculated polypus.	Below inferior vocal cord.		Tracheotomy was performed, but without relief.
6 years	Recovery.	Oblong, rounded, fibrous tumour, of size of a cherry, with string-like pedicle.	Left ventricle, or vocal cord.		Tumour rose within view, and was excised per os orem
3 or 4 years	Recovery after use of nitrate of silver.	Numerous minute polypiform vegetations.			Twenty or thirty small polypiform vegetations brought up on the sponge.
Several years	Patient did not return.	Pedunculated tumour.	Rising into view above the entrance of the larynx.		
	Slow suffocation.	Verruces (Verrucæ).	At the base of the epiglottis.		
	Sudden suffocation.	Pedunculated mucous polypus of size of a chestnut.	Right superior vocal cord.	Cartilages of larynx ossified.	
9 mo's	Sudden suffocation.	Three watery excrescences.	Surrounding glottis.		
12 y'rs	Sudden suffocation.	Of rounded form, with rough surface like syphilitic warts; when cut, of fibro-cellular appearance.	Below the ventricles posteriorly, and by broad base.		
6 or 8 months	Sudden suffocation.	Warty growth.	Base of epiglottis and vocal cords.		

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A Tabular View of Forty-two Cases of

Case.	Where reported, and by whom.	Sex.	Age.	Cause.	Voice.	Respiration.	Cough.	Sensations in the larynx.	Deglutition.
40	G. Buck, reported herewith.	F.	53		Hoarseness gradually progressing to aphonia.	Dyspnœa and suffocative paroxysms.	Severe cough.	Obstruction, and of a body moving up and down.	Difficult, and at length restricted to liquids.
41	Pauli, Gazette Medicale de Paris, 1851.	M.	36				Dry cough for 2 years, and emaciation.	Of a foreign body in throat.	
42	Specimen in N. York Hospital Museum, without history.		Adult.						

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Morbid Growths within the Larynx—Continued.

Duration of disease.	Mode of termination.	Form and nature of tumour.	Seat of attachment.	Other lesions of larynx.	General remarks.
About 3 years	Sudden suffocation.	Warty growths, pedunculated and with broad base.	Both ventricles and vocal cords; base of epiglottis and aryteno-epiglottic folds.	Ossification of cartilages, and subsequent necrosis; sloughing on interior surface of laryngeal cavity.	Laryngeo-tracheotomy performed twice, and partial extirpation of tumour effected afterwards; tracheotomy low down.
2 years	Recovery.				This patient recovered after expelling a vascular polypus half a finger in length during a violent fit of coughing.
		Solid tumour, with fissured, uneven surface, of size of last phalanx of thumb.	To outer surface of right aryteno-epiglottic fold, and filling up the pharyngeal gutter of same side, blocking up the entrance to the larynx.		

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ON

THE SYMPATHETIC NERVE

IN

REFLEX PHENOMENA.

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ON

THE SYMPATHETIC NERVE

IN

REFLEX PHENOMENA.

BY HENRY F. CAMPBELL,

OF GEORGIA.

In a recent number of the *Gazette Medicale* appear some remarks,* by M. Cle. Bernard, on the Reflex Actions of the Nervous System. In these, he refers one order of such reflex phenomena to the sympathetic system of nerves, and illustrates, by experiments upon the frog, as well as by reference to many of the acts of nutrition and secretion, that such a relation exists between the cerebro-spinal and ganglionic system of nerves, as well as between the excitor and motory portions of the cerebro-spinal system. Or, in his own words, "two kinds of nerves are requisite for the production of these reflex phenomena of organic life: the first, transmits the impression to the nervous centres; the second, to the viscera. With one order of these nervous filaments is always connected a ganglion of the great sympathetic. Example: the lingual nerve transmits the impression of the taste to the nervous centres; a special nerve then conveys a corresponding excitation to the submaxillary gland; on one of these nerves is situated a ganglion of the sympathetic, the submaxillary ganglion, &c. He gives several illustrations of this fact, and, farther, appears to be of the opinion that this set of phenomena are *identical* with those occurring in the cerebro-spinal system of nerves denominated *excito-motory*, by Dr. Marshall Hall; but with this latter part of his paper we have nothing to do. It is only with that portion in which he appears to claim as his own, the suggestion of the theory that there does exist such a reflex relation between the sympathetic and

* Translated by the New Orleans Medical Register.

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the cerebro-spinal systems, and his presentation of it as an observation entirely new.

Now, we are not aware of the exact length of time that these views have been held by physiologists; they may be old, or, on the other hand, they may be of recent suggestion; but certainly, we cannot award to M. Bernard the merit of being the first to express such views in relation to the function of the sympathetic system of nerves; and while we are exceedingly reluctant to refer to our own humble labours in connection with the brilliant discoveries of this most philosophic and able observer, a sense of duty to ourself, as a member of this National Association, prompts us not to allow this assertion to pass unchallenged.

A reference to a paper presented by us to the Medical Society of Augusta, Georgia, and published in the *Southern Medical and Surgical Journal*, on the Influence of Dentition in producing Disease, will show that this subject was fully discussed by us as early as June, 1850, and that the whole argument upon which our theory of the mode in which dentition does produce certain morbid results (diarrhœa, for instance) is based upon the supposed existence of such a reflex relation between the cerebro-spinal and ganglionic systems of nerves, as will be seen by the following: After referring the occurrence of *convulsions* during dentition to the reflex relations existing between certain nerves of the cerebro-spinal system, viz., the fifth pair as excitor, and the muscular branches of the spinal system as motory, we then endeavour to account for the occurrence of *diarrhœa* by establishing the existence of a similar relation between the cerebro-spinal and branches of the ganglionic system supplying the intestinal canal; which suggestions will be found embodied in the following extract from our essay in the June number of the *Southern Medical and Surgical Journal*, volume for 1850, p. 329: "We have now glanced sufficiently, we think, at the anatomy and physiology of the sympathetic system of nerves, to make the application of such points as are pertinent in the solution of our pathological problem. In its anatomy, we have seen its connections with all three of the divisions of the fifth nerve by ganglia, the connection of these various ganglia with each other, as well as with the cerebro-spinal axis, and lastly, the distribution of branches from these ganglia, which are conducted by the arteries into every part of every one of the splanchnic viscera. In its physiology, we find it in entire charge of the important functions of nutrition and secretion, and that wherever these processes are effected, it is by the agency of this nerve

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alone upon the bloodvessels. And farther, we have seen that pathology, and experiments upon the lower animals, establish its indispensableness to the due performance of these functions, and that whenever the supply of its innervation has been cut off from any particular part of the organism, that part immediately manifests symptoms of impaired nutrition and altered secretion.

"Now, we are all aware that nearly the whole of the intestinal canal, or rather that portion between the stomach and lower part of the colon, receives no direct innervation from the cerebro-spinal axis, but is entirely dependent upon the sympathetic nerve for its supply of nervous influence, of whatever kind it may enjoy, whether motory, sensory, or secretory, and consequently an impairment in the function of this nerve must necessarily correspondently alter its condition, so far as regards all those functions with which this nerve endows it. The alteration in these functions would, of course, depend in a great degree upon the amount of impairment in the source of irritation; thus, as we have seen, if the supply be entirely cut off, the functions of the arteries seem in a great measure to cease, passive congestions occur, and the parts inflame and ulcerate. Now, we can also very naturally conceive of a condition of these nerves somewhat analogous to the above, yet intermediate between the entire interruption caused by section and perfect health; a condition of embarrassed, or perhaps of exalted innervation. Now, this intermediate condition is exactly the state which, from the developments of the foregoing investigation, we feel that we are authorized to affirm is that which occurs as the result of severe dentition, and that upon it is dependent the whole train of intestinal morbid phenomena observable during this process.

"That this so far is legitimately inferable, we do not think any one will deny. Now let us inquire how far these phenomena are dependent upon dentition; *and analogy with the excito-motory* system will much assist us in our argument. We have seen that local irritations can, through this system, produce convulsions by the reflex function of the nerves, the sensitive branches of the fifth pair becoming *excitor* to the *motory* spinal nerves; *and so may we justly infer do these same branches (of the fifth pair), under certain circumstances, become excitor to the secretory filaments of the sympathetic, distributed so abundantly to the intestinal canal,* by a transmission of this irritation through the various ganglia with which it is connected. *Thus, the irritation at first produces simply an exaltation of the innervation of these secretory surfaces, and consequently* secretion is more active than normal, producing simple diarrhœa. A

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continuance of the irritation alters the character of the secretion, and we have the various morbid discharges observable during this period. This increase and change in the secretion are effected by the agency of the altered function of the nerve upon the arteries from which these secretions are eliminated. Now, when the innervation of these arteries is still farther embarrassed by the long continuance of the *reflected irritation*, the state of things nearly approaches what was observed in Dupuy's, Reid's, and Pourfour du Petit's experiments, of actual destruction of the nerve, and we have ulceration of the intestinal mucous membrane; all these phenomena being the result of various degrees of injury sustained by the sympathetic nerve. It may here be asked, Why should the branches supplying the intestinal mucous membrane become more implicated than any other portion of the sympathetic system? And why do not similar irritations of the *fifth nerve* produce like results in the adult? To the first of these questions we answer that most probably the other portions *are* implicated; but the manifestations of such implication are greater and graver here than elsewhere, because these sympathetic branches are the sole sources of innervation to the viscus. Other organs are in all probability affected; but, receiving a certain amount of innervation from other sources, most of their functions not being entirely secretory, are still, though imperfectly, carried on. But in the intestinal canal the case is far different; the requisitions made upon it are of such a nature that it has need for no other innervation than that of the sympathetic system. Its functions are secretion and nutrition for the whole animal organism; and when these are impaired, its primary, indeed its only intents are altered, or completely nullified. The second question is answered by the greater development of this system in the growing than in the adult individual, for the purpose of supplying the more active nutrition and secretion at that time necessary. We know that disease is more apt to occur in many parts of the body during this period than at other times; this is the general admission. Thus, according to many authors, among whom are West, Churchill, &c., pneumonia and bronchitis are more apt to attack children during dentition than at any previous or subsequent period. Cutaneous eruptions, and many other aberrations of secretion occurring during this period, serve to corroborate our theory of the origin of the morbid intestinal secretion. The increased vermicular action and tormina attending this affection find a ready explanation in the fact, now well established, that the sympathetic receives both motor and sensitive filaments

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from the anterior and posterior roots of the spinal nerves, endowing the organs of their distribution, to a certain degree, with corresponding susceptibilities.

"In conclusion, let us define the position which, at the end of our investigation, we feel warranted in assuming. It is the following: That in the anatomy and physiology, as well as in the dependent analogies of the process of dentition, we find ample ground for the opinion that the diseases pertaining to this period may be dependent, and, in many instances, are entirely so, *upon the local irritation attending the process being transmitted through*, either the cerebro-spinal system of nerves, producing convulsive diseases in the motory apparatus, *or through the sympathetic, causing derangements of the secretory organs, particularly of the alimentary canal*, by the sway which it exercises over the arterial system from which these secretions are eliminated."

In the above brief quotation, it will be observed that the doctrine of the reflex function between the cerebro-spinal and sympathetic systems is plainly enunciated, and not only is the physiological fact noted, but we there also have surmised the *transmission* of *permanent* irritation, or of paralysis from the cerebro-spinal to the sympathetic system, giving rise to various aberrations in nutrition and secretion. This opinion we have held for several years, teaching to our classes that there *existed between the cerebro-spinal* and the *ganglionic* system of nerves, a relation *similar* to that between the sensitive and motor branches of the cerebro-spinal, and which Marshall Hall terms *excito-motory; while we have termed that between the cerebro-spinal and sympathetic* systems *excito-secretory.*

As we have before indicated in this report, we do not feel authorized to lay full claim to the above theory without farther investigation of the subject; but with all due courtesy to that highly distinguished gentleman, we can say that we feel assured that these views are not original with M. Bernard, unless he entertained them previous to June, 1850. There may have been other similar observations; but until the publication of M. Bernard's, we had not noticed them elsewhere than in our paper on Dentition.

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PRIZE ESSAY.

THE

SURGICAL TREATMENT

OF CERTAIN

FIBROUS TUMOURS OF THE UTERUS.

BY

WASHINGTON L. ATLEE, M.D.,
OF PHILADELPHIA.

"Palmam qui meruit ferat."

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THE

SURGICAL TREATMENT

OF

CERTAIN FIBROUS TUMOURS OF THE UTERUS,

HERETOFORE

CONSIDERED BEYOND THE RESOURCES OF ART.

In considering the surgical means for the removal of fibrous or fibro-cellular tumours of the uterus, it will be necessary to distinguish the different situations which they occupy in relation to that organ. The following classification, however, is not offered as strictly and pathologically correct, but adopted mainly to assist in illustrating the measures employed in the treatment of such tumours. I shall principally confine myself to the consideration of those tumours which have heretofore been supposed inaccessible to the knife, or not amenable to curative measures.

I ought to remark too, that the following essay is based wholly upon my own experience; I alone am responsible for the facts therein contained, the treatment adopted, and that which will be proposed.

It will be proper, also, before entering upon the treatment in detail, to associate with the classification the symptoms and diagnosis of fibrous tumours. Here, however, I shall confine myself to the most prominent features. No symptom enables the practitioner to anticipate with certainty the existence of such tumour, and it is often ascertainable only after it has acquired considerable size. Sometimes, a digital examination will discover it at a very early period, and when of small size.

These tumours may be classified as follows:—

1. Extra-uterine, or surface tumours.
2. Intra-uterine, or cavity tumours.
3. Intra-mural tumours of the uterus.

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1. EXTRA-UTERINE TUMOURS.

Extra-uterine tumours originate from the peritoneal or external surface of the body and fundus of the uterus, and beneath the peritoneal coating. They are projected, as they become developed, into the cavity of the abdomen, elevating the peritoneum and retaining it as an envelop, assuming often an immense size. They may or may not be pedunculated. When attached to the uterus by a stem, the size and character of this attachment may differ in different tumours. In some, the pedicle is very slender and membranous, consisting chiefly of peritoneum with long cellular tissue, and vascular. In others, it is thicker and more resisting, constituted of both fibrous and cellular tissue, yet differing in character from the tumour itself. Again, tumours are found entirely sessile, directly attached to the external wall of the uterus, without an intervening pedicle, but narrowing at the point of junction into a distinct neck. And lastly, we notice them without a pedicle, or neck, or any narrowing, imbedded as it were in the substance of the uterus immediately beneath its peritoneal coat. The first three varieties have been removed by the operation of gastrotomy. I am not aware that any operation has been accomplished, or proposed, for the removal of the last. I apprehend that they can be extirpated, after opening the abdomen, by making an additional slit in the peritoneal coat of the tumour, and then enucleating or turning out the mass from its bed.*

Opportunities are not unfrequently afforded in dissecting-rooms of examining these uterine tumours, and they are generally found to be invested with a species of cyst, formed by the peritoneal covering over one half, and the uterine substance beneath the other, to which cyst they adhere by very short cellular tissue. This tissue, by a moderate degree of force, can be lacerated, and consequently the covering can be peeled from the tumour. The same thing obtains in the living body. This will not be the case, however, if the tumour has undergone either inflammation or degeneration; as, in the first case, adhesions will be formed between it and the cyst, and in the latter case, the cellular structure of the uniting membrane will be destroyed.

* Since writing the above, I have proved this opinion by enucleating a tumour in this manner, from the posterior surface of the uterus, in the presence of Drs. Janney, Jewell, De Young, Keller, Drysdale, and Jackson. The operation was performed March 8, 1853.

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So long as these tumours continue small, they may give rise to little or no inconvenience, and consequently may pass unsuspected and undiscovered during life. As, however, they increase in size, they often occasion considerable distress and a sensation of weight in the hypogastric, iliac, and lumbar regions, accompanied by painful tension and bearing down. Frequently, though not invariably, menstruation is deranged, becoming more frequent and profuse, and the discharge often clotted. Flooding sometimes is extreme, and the patient is almost drained of blood. Urination and defecation may be more or less disturbed, sometimes difficult, and at times obstructed, and spasmodic pains affect the bowels. The tumours at first may be indolent and not sore to pressure, but finally they are apt to become painful and very sensible to pressure. The patient's general health is now observed to give way; lancinating, deep, and acute pains attack the tumour; the strength fails, emaciation is rapid, the skin assumes a straw colour, and the patient becomes anemic and dropsical. If the tumour has risen out of the cavity of the pelvis, it may be felt in the lower part of the abdomen as a hard, solid, and more or less round, movable mass. When this occurs, the patient usually becomes cognizant for the first time of the existence of such a tumour. A sudden jar of the body, as a fall, sometimes dislodges it from the pelvis, and the cause of the tumour is erroneously attributed to this accident. Sometimes the tumour above the pelvis is painful on pressure, more generally it is not.

The diagnosis of this form of tumour is not very difficult, excepting in corpulent patients. If the index-finger be passed into the vagina, the os tincæ will generally be found to have varied from its normal position; it will have left the axis of the vagina, and may be depressed or elevated. It sometimes is raised even beyond the reach of the finger, and in this case the tumour will probably occupy the cavity of the pelvis, having pushed or tilted the uterus upwards. If, while the finger is in the vagina, we place the hand upon the abdomen over the pubis, we can survey the contents of the pelvis, and may get a tolerably accurate idea of the size of the tumour and of its relations to the uterus. Indeed, in this way, assisted by the uterine sound, we can often detect tumours on the surface and in the walls of the uterus at a very early period. If, on moving the tumour between the two hands, the uterus moves uniformly with the mass, and as part of the mass, then it must be either an extra-uterine non-pedicellate tumour, or an intra-mural tumour, or a fibrous or other body occupying the cavity of the uterus. The sound will greatly

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aid in distinguishing one from the other. But during the child-bearing period of life, the sound may be a dangerous instrument. The recurrence of menstruation and the absence of ballottement, aided by auscultation, will usually enable us to determine between tumour and pregnancy, and justify or not the use of the sound. Still, it will be most prudent not to act hastily. Even in pregnancy sanguineous discharges may occur, there may be too little liquor amnii to allow of ballottement, and the sounds belonging to gestation may not, for various reasons, be discoverable. It will be best, where doubt exists, to wait until time decides the question. When there is nothing to forbid its use, the sound becomes a valuable means of diagnosis. When a tumour occupies the cavity or wall of the uterus, the sound, as a general rule, will pass beyond the normal distance of two and a half inches, and it will exceed these limits proportionably to the size of the tumour. When a tumour is extra-uterine the cavity, in general, is not lengthened. To this rule, however, there are exceptions. I have seen cases of very large intra-uterine and intra-mural tumours, where the sound would not enter beyond the natural distance, and also, where it was intercepted far short of it; and on the other hand, I have met with cases where the cavity of the uterus was elongated by the other variety of tumours. Again, sometimes the os tincæ is too small to admit the ordinary sound, or, having entered, it may be arrested by the narrowing of the cervix called the os internum, or by an acute angle in the cervix itself. These difficulties can usually be overcome by substituting a smaller sound, and by incising the cervix by a narrow bistoury. By means of the sound, aided by manipulations in the vagina and rectum, and over the abdomen, we can also usually ascertain the point of origin of the tumour, and its location in the walls of the uterus, and at the same time decide whether it is pedicellated or not. Suppose it occupies the posterior wall, the sound will be thrown forward, while the finger in the rectum will detect the fibrous mass placed between them. This state of things, when the tumour is not large, assimilates retroversion of the uterus. Two of the most distinguished accoucheurs of this city, were misled by this state of things in a case which subsequently came under my care. The sound would have undeceived them. Again, suppose a tumour occupies the anterior wall, the sound will be thrown backwards, while the hand over the pubis will discover its location. This may be taken for anteversion, and in one instance one of the practitioners just referred to was thus misled. If a tumour be attached to the external surface of the uterus

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by a long stem or pedicle, it can be moved in various directions independently of the uterus, the motions given to the former communicating no impulse or very little to the latter, and *vice versa*. A short pedicle, however, under the influence of similar manipulations, will impart more or less motion to the uterus, but yet not corresponding in extent with that given to the tumour. The introduction of the sound will enable us to distinguish this point much more accurately, as the impulse received by the uterus also affects the sound in the hand, and its motion may be watched by the eye of the operator. Besides, in reversing the method of examination, by noticing the influence upon the tumour arising from motions given to the uretus, the sound is almost essential. Thus, the sound will enable us to discover the existence of a pedicle, and also to estimate its length.

In examining extra-uterine tumours, particularly those that are pedicellated, it will be found highly advantageous to have the assistance of another surgeon, or an intelligent nurse. The several manipulations of the tumour should be made by one person, while another watches the effect they produce upon the uterus and sound, and this ought also to be the case when the method is reversed. The mind in this way more clearly defines the extent of the influences operating upon the parts under investigation.

2. INTRA-UTERINE TUMOURS.

Intra-uterine tumours arise from the internal surface of the uterus, project into its cavity, distend it, and vary, as do extra-uterine tumours in the length, thickness, and character of their pedicles. Like the latter, they may also be sessile, having no pedicle, merely narrowing at the point of junction into a distinct neck, and they likewise may be imbedded in the substance of the uterus, immediately beneath the mucous lining, having no narrowing or neck. All these tumours receive a coat from the mucous membrane of the uterus, and the vascularity of this envelop is greatly increased beyond its natural condition. When these tumours, in this situation, are composed of a pedicle or neck and a body, they are distinguished by the name of *fibrous polypi*. Their tendency is to be expelled from the cavity of the uterus into the vagina, whence they have been removed by the ligature and the knife. I prefer the latter mode.

When the tumour is imbedded in the internal wall of the uterus, and merely covered by mucous membrane, or when the polypoid tumour becomes sealed to the uterine surface, as it sometimes does,

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in consequence of inflammation, so that it cannot be expelled by the expulsive efforts, nor the os tincæ be opened by natural means, operative measures for its removal are supposed to be impossible. That this is not the case I shall endeavour to prove.

Intra-uterine tumours, particularly those that are imbedded and those that are adherent, produce many of the symptoms of extra-uterine tumours already stated. Other symptoms, however, are superadded, and may be considered pathognomonic. Menstruation is more irregular, generally more frequent, more profuse, often menorrhagic, the discharge frequently being stringy and clotted. Sometimes there is hemorrhage, sudden and overwhelming, rapidly draining the vascular system, and prostrating the vital powers. During the intervals the patient, to a greater or less extent, is likely to be annoyed with leucorrhœa. Even when the tumour is small, bearing-down pains may be experienced. As it enlarges it gradually distends the uterus. The distension may be confined to the body alone, or it may likewise invade the cervix, and dilate the os tincæ. The pain in the back and loins now assumes a dragging character, and a feeling of pressure and distress on the anus and neck of the bladder annoys the patient. Decided labour-pains often attack the patient, causing the most intense suffering; and these sometimes occur at regular periods. These attacks of pain are most likely to occur during the menstrual periods, and the expulsive efforts of the uterus, acting on the tumour within, frequently open the os tincæ. Hence a digital examination, at these times, will enable us to discover an intra-uterine tumour, when it will be impossible at other times.

In the very early stages of intra-uterine tumour, before the cervix and the os uteri are impinged upon, we can discover nothing by an ordinary examination, per vaginam, upon which to base a certain diagnosis. This observation will particularly apply to young or corpulent females, and indeed even those who are spare and relaxed in the abdominal walls. In the latter, the body of the uterus may be felt to be enlarged between the hand placed above the pubis and the finger in the vagina, and even an enlargement of the cervix may be recognized in addition, yet the existence of a tumour *in the cavity* of the uterus must necessarily be conjectural. It is true, that if the body of the uterus be uniformly increased in size, having no nodulated feel, and the cervix regularly expanded, the probability of the cause of the enlargement being internal would be very strong. Still, the diagnosis would be uncertain. If, however, there be no

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suspicion of pregnancy, the sound may be brought in aid of the decision, and the information furnished by it may often be considered positive. In a regular polypoid or pedicellated tumour, the sound will pass into the uterus between its walls and the tumour over its whole area, excepting at the point to which the pedicle is attached. If now, while the sound is introduced, the hand be placed upon the abdomen, its point may be easily detected through the thin walls of the uterus, and if we can feel it at all points we may fairly infer that the tumour occupies the *cavity* of the uterus, and our practice may be safely based upon such an inference.

When, however, the tumour, by enlarging, or in consequence of uterine contractions, begins to press against the os tincæ after it has expanded the cervix (and even before if the os be open), the diagnosis is greatly facilitated. We then feel the tumour immediately within the os uteri, being usually convex, smooth, and hard. If the condition of the parts will allow the introduction of the finger, we can pass it entirely around the tumour, unless adhesions have been formed, or unless its pedicle is attached to the cervix. The absence of any attachments, within reach of the finger, furnishes a strong ground of supposition that the tumour originates from the fundus of the uterus. Of course, when the pedicle is within reach it is readily recognized.

The natural tendency of the uterus, however, is to expel a non-adherent polypus from its cavity and lodge it in the vagina; and in this position, particularly when small, it is readily diagnosticated by an examination. The finger can be passed entirely round and above it, until its neck is recognized within the grasp of the os tincæ. When, however, the polypus is large, so as to fill and distend the whole vagina, it may be extremely difficult, nay, impossible, to pass it with the finger; so that the neck cannot readily, or at all, be surveyed. In this event, the diagnosis may be aided by seizing the mass with an obstetric or a Musieux forceps, dragging it down to, or even through, the vulva, and, at the same time, passing the finger up to the os tincæ.

The tumour, in consequence of inflammation of its envelop, or through a peculiar process of degeneration in its own tissue, may adhere to the internal surface of the uterus, or to the circle of the expanded cervix, so that the os tincæ cannot open, nor the cavity of the uterus be entered. Here the above means of diagnosis—the finger and sound—will not avail, and our opinion of its location must necessarily be more or less conjectural. So far as its treatment is

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concerned, however, we can act in the same way as in the intra-mural tumours, which we shall next consider.

8. INTRA-MURAL TUMOURS.

Intra-mural tumours originate in the very substance of the parietes of the uterus itself, within the muscular interstices. This may occur at equidistances from the external and internal surfaces of the uterus, or at any point between them. The proper tissue of the organ does not enter into their composition; it is merely pushed aside by the tumour, the muscular fibres being separated and surrounding the tumour like a cyst. This remark, however, does not apply to fibrous tumours after they have assumed a malignant character, and have become medullary, as the uterine tissue itself loses its normal character also. But when the tumour has not thus degenerated, and when there has been no inflammation during its growth, the uterine covering can usually be peeled off from its surface as the rind from the pulp of an orange. Of course, these tumours have no pedicle. As a general rule, their growth is less rapid than extra-uterine tumours. Sometimes they develop themselves uniformly, at others unevenly, so as to assume a lobulated or nodulated form. It is supposed that these tumours admit of no remedy. I shall endeavour to prove that they are amenable to surgical treatment.

The symptoms of this class of tumours must necessarily partake of the character of those belonging to the first, and also of those incident to the early stages of the second class.

As the tumour enlarges it rises into the cavity of the abdomen, and generally assumes a more or less central position. The hand, placed above the pubis, will detect it, and if the finger at the same time be brought against the os tincæ, it will be found that their impulses will reciprocally affect each other. The tumour and uterus invariably move in unison. A sound passed into the uterus, will enter generally beyond its natural distance, and partake of all the motions given to the tumour. While the sound is there we may ascertain the exact location of the tumour, as it will give a direction to the sound, and this can be felt through the unoccupied walls of the uterus. When the tumour is developed in the wall of the fundus uteri, the cervix may be uniform and in no way changed, but usually we find it deformed, and even turned upon itself, sometimes at an acute angle. When the tumour enlarges downwards so as to invade

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the cervix, the posterior or anterior lip may be entirely obliterated and distended into a covering for the tumour. The posterior wall of the uterus, and consequently that of the cervix, most frequently are affected, and hence the os tincæ is often thrown forward, under the pubis, or even raised above it, so that it is difficult, and at times impossible, to reach it with the finger.

Besides the above classification, there is another distinction which it is important to bear in mind in connection with the prognosis and the period of treatment. I believe there is, at times, a tendency in fibrous tumours of the uterus to take on cancerous action. I know a diversity of opinion exists on this point among pathologists of eminence, but the results in certain cases under my own care lead me to think that the true fibrous tumour will occasionally degenerate into medullary tissue. It may be that *scirrho-cancerous* tumours have in these instances been mistaken for *fibrous* tumours, as the latter, by excellent observers, are deemed incapable of becoming cancerous. Yet they were diagnosed to be fibrous, and they possessed all the physical characteristics of such tumours. If they were cancerous tumours, then I know of no method of distinguishing them in their early stage from fibrous tumours, except by the microscope, and the microscope, in many of these cases, cannot be employed as a diagnostic means. In some cases the exploring-needle will enable us to get sufficient of the structure to be submitted to the test of the microscope. If, however, I am correct in the opinion that the true fibrous tumour occasionally degenerates into cancerous disease, then it is important to know the symptoms which designate this change. So long as it remains hard and indolent, it does not greatly interfere with the general health; but so soon as it begins to soften or degenerate, it rapidly impairs the health and terminates in death. Hence the necessity of distinguishing this point, and the urgency of early treatment.

The symptoms, indicating this important mutation in the character of the tumour, vary with the class to which it belongs.

The intra-mural and extra-uterine tumours, when neither large, nor disposed to encroach on any important organ, nor to produce great hemorrhages, do not greatly impair the general health, so long as they remain hard, painless, and insensible to pressure. Their removal at this time may result in a perfect cure. But as soon as they begin to soften, are attacked with paroxysms of acute pain, are very sore to the touch, and throw off cysts, containing liquid, from their surface,

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the general health fails, as indicated by the rapid diminution of strength, emaciation, anasarca, and ascites, and that peculiar straw-coloured aspect of the skin which marks the malignant type of disease. It is now almost, if not quite too late, for a successful operation.

In the intra-uterine tumour other symptoms in addition mark this change. So long as the patient has a red or white discharge from the vagina without fetor, and the tumour presents an equal hardness, while the speculum exhibits a white or flesh-coloured body, with a smooth, polished, and even surface, an operation for its removal is likely to be successful, notwithstanding the patient may have been considerably prostrated by the effects of the disease. If, however, the discharge becomes sanious and very fetid, the tumour assumes a brownish, grayish, or black colour, and softens at its lower surface, where it comes in contact with the air; and if, farthermore, these symptoms are associated with the rapid development of those characteristics which indicate the effect of malignant disease upon the general system, then the success of operative measures must be very doubtful.

These tumours, although they often have highly vascular pedicles, and are covered with a very vascular membrane, whether extra or intra-uterine, are yet, in themselves, very imperfectly organized. Consequently their vitality may be readily destroyed. A section made through their investing membrane, will sometimes be followed by the death of the whole mass. This may be owing to the admission of atmospheric air into the substance of such a tumour, causing it to degenerate. Indeed, it would appear that the action of the oxygen of the air, like a portion of yeast in a fermentable mass, may originate in any part of a fibrous tumour, an action of eremacausis which may extend throughout the whole. If this opinion be corroborated by future observation, we shall then have one means of destroying such tumours in their incipient stages, and thus avoid the dangers arising from the degeneration of large masses. If, also, we can get rid of tumours whose tendency is cancerous, before they take on this action, and before the general health breaks down, the importance of such a fact becomes manifest.

The several cases reported in connection with this paper will be sufficient to substantiate the above view, but the exclusive influence of oxygen gas in establishing eremacausis in these tumours when introduced into their substance, will more certainly appear from the details of the following case.

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Mrs. F. K., aged 36 years. July, 1852, I first examined her in company with Dr. William Gardener. A very hard uterine tumour occupied the abdomen, its shape and size very much like the uterus at the seventh month of gestation. September following I examined her again. The same tumour could be discovered, but she had enlarged greatly. She was now larger than is usual at the full period of pregnancy, and the tumour had changed its character, throwing off cysts from its superior surface. The upper part of the abdomen particularly, had become elastic, and a fluid could be distinctly fluctuated. October 4, the patient having increased much more in size, and suffered great agony, I attempted to relieve her by paracentesis. I introduced a very large trocar into the linea alba, about three or four inches below the umbilicus, and pushed it in nearly its whole length. Only a few drops of fluid escaping, I passed the uterine sound through the canula, and it was arrested by a resisting mass. After probing in several directions, the sound, aided by considerable pressure, finally passed on backwards and upwards into a cyst, from which escaped slowly about three pints of dark-coloured fluid and a small quantity of blood. After this cyst was emptied I partially withdrew the canula, and again replacing the trocar, I directed it upwards and towards the right side to its utmost extent, with a view of striking a cyst in that direction. But no fluid escaped. I again used the sound as a probe, pushing it beyond the canula into another cyst, from which flowed about three pints of quite clear transparent fluid. After tapping, both sides sunk, leaving a central mass, hard and resisting, and covered with protuberances. This was the original tumour. The trocar had passed through this hard mass to reach the cysts, and consequently atmospheric air, in consequence of the continued and free manipulations, was readily admitted. October 12. The patient was larger than ever. In twenty-four hours after the tapping she was as large as at first. The cysts, instead of being filled with fluid, were now greatly distended with gas. To relieve this flatulent distension, I introduced in two places a small trocar and canula, through which a large quantity of fetid gas and some fluid escaped. This had the effect of relaxing the walls of the abdomen. The solid tumour could now be felt, but it evidently was softening. October 23. The abdomen having become greatly distended again, I passed the small trocar through the old orifice, and a large quantity of the most fetid gas escaped, and a considerable amount of offensive fluid, resembling thick soap-suds or rich chocolate. A large quantity of fluid still remained too thick

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to pass the small canula. To remove this, I introduced the large trocar through the cicatrix left by the first paracentesis, and extracted fourteen pints of a similar fluid, much thicker, and filled with broken-down degenerated tissue, extremely fetid. The fibrous tumour had evidently softened and liquefied, the most of it having disappeared. The canula was permitted to remain *in situ*, and the whole mass disappeared in an offensive discharge in the course of five or six weeks.

Treatment.—The treatment of the various forms of fibrous tumours of the uterus will be best illustrated in the report of the following cases, and consequently I need not here refer to that which will more properly be given in connection with each case. I wish, however, to call special attention to a fact, which is of great importance, in relation to the treatment of patients afflicted with these tumours. It is this: *That the excessive hemorrhages, which sometimes occur, arise, not from the uterus itself, but from the vessels of the membrane which cover the tumours.* These floodings, I think, occur in this way: the veins of the investing membrane become, at times, greatly engorged, in consequence of their circulation being impeded by the muscular action of the uterus, while the arteries, by reason of their more resisting coats, continue to supply them with blood. This engorgement, also, may be induced or greatly aided by any obstruction to the venous circulation of the vessels of the abdomen. It may increase more and more until the vessels of the coating burst at a point where the resistance is least, and sudden and violent hemorrhage is the result. The point of least resistance must necessarily be at the os uteri, as all other parts of the tumour are compressed by the contracting uterus. This condition of things may be compared to that of an arm bandaged for venesection: the ligature arrests only the venous circulation, and the veins become tensely tightened in consequence of the persistence of the arterial supply. The mucous tissue covering these tumours, having been derived from the mucous lining of the uterus, is necessarily extremely delicate, and offers but little resistance to the rupture of these vessels. Now, the practice which I wish to inculcate as based upon the above fact, and which has invariably arrested hemorrhage instantaneously, may at first view seem startling. It is: *During hemorrhage, to pass the bistoury along the vagina into the cavity of the uterus, and make a very free incision into the most exposed portion of the tumour.* A small gush of blood will follow, and the bleeding will soon cease. The cut

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ends of the vessels retract, and the blood soon coagulates in their mouths.

CASE I.—*Mrs. M.*, 49 *years old; tumour intra-uterine, nearly its entire surface sealed to the interior of the uterus, even down to the edge of the os tincæ; the whole tumour removed; supposed weight ten pounds; recovered; death subsequently from inflammation of the lungs.*

April 14, 1845. I was requested by Dr. Kerfoot, of Lancaster, Pa., to accompany him to see Mrs. M., a lady aged about 45 years, who had come from one of the western counties to place herself under his professional care. On examination externally, it was very apparent that a large tumour occupied the cavity of the abdomen. It was uniformly globular, very firm and resisting, extended between two and three inches above the umbilicus, spread out upon each side equally nearly to the spinous processes of the ilii, and produced an unusual anterior prominence. In fact, it was an equally developed globe, and seemingly balanced upon the brim of the pelvis. Pressure upon the tumour produced no pain except over the left side, where one spot had always been sensitive on pressure. The skin of the umbilicus was also very tender to the touch, and slightly tumefied and discoloured. Percussion of the tumour returned a very flat sound.

By the toucher, the os tincæ was discovered to be well defined, rather uneven, thickened, and firm, and free from tenderness. It was considerably elevated in the pelvis, and seemed to be fixed upon the centre of the convex surface of a tumour resting upon the brim of the pelvis. It was closed, at least the projecting lips lay in apposition, but the point of the finger could, by forcing, be insinuated between them. The finger thus introduced immediately came in contact with a very solid body, occupying the cavity of the uterus. In endeavouring to insinuate the finger between the tumour and cervix uteri, a barrier, in the form of strong fibrous bands running from the edge of the os tincæ to the lower point of the tumour, was met with on the left side, and which bound the two firmly together. On the right side, however, opposite the right acetabulum, the finger could be forced higher up without meeting such opposition. This led to the inference that the tumour was adherent to the uterus on the left side only, and free upon the right, and it was proposed to ascertain the fact by the introduction of sounds. No blood stained the finger or followed the examination.

15*th*. The uterus was surveyed with curved sounds—the ordinary

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calculus sounds. They were readily passed up through the opening on the right side, and after a little delay they ascended between the tumour and uterus towards the fundus until the handles struck against the os tincæ. The instruments were too short to reach the fundus, neither could they be carried anterior to the tumour, but we supposed they swept over the tumour for some distance posteriorly and laterally, which, however, was only apparent, as will hereafter appear. The great angle formed by the anterior projection of the tumour, explained, satisfactorily at the time, the reason of the failure in getting the instrument in front of the tumour. Very little blood followed this examination.

The inference now became stronger that adhesions existed only upon the left side, and that the right side of the tumour was entirely free from them, even up to the fundus.

In this state of things what ought to be attempted? What could be accomplished? The patient was exceedingly desirous that some effort should be made for her relief, that she should not be sent home unrelieved from the tumour. It will be perceived that, in consequence of the great angle formed by the anterior face of the tumour with the axis of the inferior strait, it would be utterly impossible to throw a ligature over the fundus of the former in that condition of the os tincæ, even if such a course would have been sanctioned by correct surgery. All surgical authority that I have referred to, condemns any interference with polypus of the uterus in cases where the os tincæ is closed, and where the tumour has not descended into the cavity of the vagina, or exterior to it. In this extremity, I proposed to administer the vinous tincture of ergot in small and repeated doses, in order to stimulate the expulsive power of the uterus, and thus gradually effect dilatation of the os tincæ to a sufficient degree to admit the free use of instruments constructed specially for securing the supposed pedicle upon the left side. This was administered by her physician for a considerable period with the effect of producing decided evidence of uterine action, although no dilatation of the os uteri was accomplished. The use of ergot was suspended during the ensuing menstrual period, and recommenced afterwards with no better effect; and in consequence, early in the month of May, Dr. Kerfoot reported the fact of failure in producing the condition desired.

May 5. I met Dr. K. again in consultation. A very careful examination was now made with a view to ascertain why ergot, after evincing specific action on the uterus, had failed to dilate the os tincæ.

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As the administration of the medicine had been suspended for about twenty-four hours previously, and the uterus was free from its influence, the cause of failure was soon plainly discoverable. The os tincæ was not any more open than before, although it was now dilatable. It resembled resisting caoutchouc, capable of being forcibly stretched; but, after the force was removed, returning again to its original condition. During the action of the ergot, however, Dr. K. assured me there had been no alteration in the os tincæ. In consequence of this change, a much more satisfactory examination could be instituted. Adhesions were now detected not only upon the left side, but continuing completely round the os tincæ, except a small space upon the right side which admitted only the point of the finger. These adhesions consisted of bands passing directly from the edge and internal face of the os tincæ to the tumour, and binding them so closely together that no efforts of the uterus could have possibly dilated its mouth. The bands were very strong, tendinous-like, making numerous edges with sulci between them, and entirely closing the uterus except at the point referred to. Here there was an extraordinary, and, so far as records show, perhaps a unique condition, which required a new mode of treatment in order to effect a delivery from the burden. It was very evident that, to follow the established rule of surgery, and wait for the uterus to expel the tumour into the vagina, before attempting its detachment, would be to consign our patient to irrelievable suffering. It was also evident that ergot could accomplish nothing for our assistance. I therefore proposed to Dr. K. to cut these bands with a bistoury, and after this to administer the ergot with the original object, and, as other adhesions would be brought within reach by its action, to repeat the operation of detachment; and thus, by successive resorts to ergot and the knife, to remove the tumour entirely, or to place it in such position as to make it accessible to the ligature.

8th. We now agreed to associate with us in consultation Dr. F. A. Muhlenburg, to whom we communicated our views. He united in our opinions, and arrangements were made for putting them into immediate practice, as follows:—

The patient was placed upon her back across the bed with her hips at the edge and her feet resting upon two chairs; I then proceeded to sever with Cooper's hernia bistoury all the adhesions within reach. While operating, the os tincæ was carefully protected from the edge of the knife by the index-finger of the opposite hand, and secured

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from injury, in addition, by turning the back of the knife rather towards the uterus, and cutting through the bands with the edge inclined upon the tumour. The adhesions were exceedingly firm and sinewy, and grated audibly under the knife, requiring considerable force to sever them. The separation of the os tincæ from the tumour was carried up about three-fourths of an inch or more, but it still remained firm, unyielding, and undilated. During the operation the patient complained very much of her back, and also of a spot upon the left side. Not more than three or four ounces of blood were lost in the operation, which was tedious and difficult, producing an urgent desire to urinate, but followed by no extraordinary exhaustion.

Slight febrile excitement supervened, but yielded to laxatives and antiphlogistic regimen, and passed off entirely in two or three days. After this ergot was again administered for several days, producing, however, scarcely any effect upon the os tincæ.

June 5. This day was fixed upon for a second recurrence to the knife. The os tincæ, although it had opened very little, was rather more dilatable, and could be extended by the point of the finger, which could be introduced with more ease. There seemed to be a disposition, also, for the uterus to descend more into the cavity of the pelvis. The adhesions were consequently brought more within reach, and many of them could be detected around nearly the whole circumference of the cervix uteri, passing off from its internal face to the tumour, binding both firmly together, and preventing the farther dilatation of the os tincæ. To facilitate the section of these bands, I determined on the use of a hook, which could be passed into the uterus, through the opening on the right side, and carried high enough to ride over the bands, then bringing them down by traction, so that they could be firmly holden until severed by the knife. The hook was placed in Dr. Kerfoot's hand, who passed it into the uterus with the view of using it as above, but finding its employment unsatisfactory, he requested me to try it. I found that, instead of attaching itself to the bands, it hitched upon the rigid edge of the os tincæ, and hence, as it was likely to be unsafe in its use, this expedient was abandoned. Drs. K. and M. now desired me to proceed with the operation, which I did, as originally proposed. With Cooper's bistoury I severed numerous strong, tendinous-like, and very thick bands, using the index-finger as the guide to the knife, and as a shield to the uterus, and carefully keeping the knife between it and the tumour.

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The adhesions above the pubis anteriorly were very difficult to manage, in consequence of the inconvenient angle made by this point with the axis of the inferior strait of the pelvis. I, however, succeeded in detaching the bands quite round for a considerable distance above the point reached by the finger. This was safely done by pushing the end of the bistoury high up, retaining its blade upon the face of the finger, and cutting always towards the tumour in the act of bringing down the knife. It was equally surprising and gratifying, after severing these elevated adhesions, to find how immediately and rapidly the mouth of the uterus dilated, allowing the tumour to descend into the superior strait of the pelvis. More blood was lost during this operation than the first, but still the quantity was inconsiderable, and was followed by no unpleasant prostration. The patient complained, as before, greatly of her back and left side, and of an uncontrollable desire to pass water. Less excitement followed this operation than the former.

10*th*. In consequence of the remaining adhesions being beyond reach of the index-finger, and the os tincæ being much dilated, the parts were explored by introducing the hand. Each of us made this examination, deeming it important to obtain an accurate idea of the situation of the tumour before proceeding with the third operation. The lower portion of the tumour was discovered to occupy the upper part of the vagina, and was protruding through the os tincæ very much as the membranes in parturition. It was also losing its resisting and indurated feel. By insinuating the fingers between the cervix uteri and the tumour, adhesions of the same character could be detected around the whole circumference, except a small space upon the right side, binding the tumour closely to the interior surface of the uterus. In the space unoccupied by these bands, I could, with considerable pressure, pass three of my fingers up between the tumour and the uterus, and, as far as I could reach, I discovered the existence of similar adhesions, both anteriorly and posteriorly to the fingers. This, therefore, must have been the only outlet for the catamenia.

The patient complained more of this examination than of the two previous operations, and suffered more constitutional disturbance for several hours following.

14*th*. The lower portion of the tumour was found to be descending, and the os tincæ becoming softer and more dilatable.

19*th*. Since the operation of the 5th instant, there was evidently a gradual degeneration of the lower portion of the tumour, accompanied

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by a fetid discharge from the vagina, such as is peculiar to putrefying animal matter when being thrown off from vital parts, and which impregnated the air of the chamber so as to render it quite offensive to the patient and others. This was in some measure rectified by the use of the hypochlorite of lime. This period was likewise marked by certain hysterical symptoms, and slight irritative fever, which seemed likely to continue so long as the tumour remained *in utero*. It was therefore deemed advisable to make a final effort for its complete destruction and removal.

The great bulk of the tumour occupying the abdominal region, and the remaining adhesions being above the superior strait, the instruments heretofore employed would not answer for the completion of the operation. I therefore constructed a knife, specially for this purpose, by taking a long, curved, probe-pointed English bistoury from its handle and affixing instead a long and strong handle of hickory-wood, and then wrapping the proximal two-thirds of the blade. This made a knife about ten inches long, with a cutting edge about one inch in length.

The patient having been placed in the same position as before, my colleagues took a position to render assistance if needed. Taking a seat immediately in front of the patient, and anointing the left hand well with oil, I introduced it into the vagina and grasped the lower portion of the tumour; then passing in the long bistoury along the hollow of the hand, I bisected this portion of the tumour, and detached the divisions on a level with the os tincæ. This portion of the tumour was void of vitality, and much resembled a mass of wet tow. In consequence of its degenerated character, it had too little rigidity and too much tenacity to be easily cut, and it was with the greatest difficulty that it was detached. The removal of this part now afforded a good opportunity for examining the internal face of the cervix uteri. This result marked the difficulties to be encountered in the subsequent steps of the operation, because it was manifest that a continuous tissue of adhesions sealed the tumour to the whole interior face of the uterus, except the small space before noticed. I now determined to destroy the whole internal structure of the tumour. With the left hand within, as a guard to the walls of the uterus and a guide to the knife, I cut, with the long bistoury, into the tumour, in various directions, detaching and removing portions repeatedly. In doing this, I was aided very much in the use of the knife, by fixing the tumour with one blade of the old-fashioned crotchet passed up through the incised interstices and imbedded into its substance, thus

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enabling me to put the fibre upon the stretch so as to react with more effect upon the edge of the knife. I made use, also, of the old style cranial perforator with great advantage, employing it as scissors within the lacerated cavity of the tumour in detaching pendulous portions. After persisting in this course a considerable time, the uterus being supported externally by my associates, I succeeded in breaking up the whole internal structure of the tumour, and in scooping out a large quantity of it. The shell of the tumour still remained undisturbed by the knife, and was firmly held by the remaining adhesions to the uterine walls. With the back of the bistoury kept carefully turned towards the parietes of the uterus, and directed by the hand within, I cautiously ploughed it through this shell in every point. I next sought for the free surface of the tumour upon the right side, into which I introduced my fingers and endeavoured to break it down from the uterine walls, but failed in consequence of the density of its investing membrane, and the firm unyielding bands which bounded this space. In this manœuvre, it was evident that my fingers approached nearly to the fundus of the uterus, which had descended in consequence of the contractions that followed the removal of the large interior portions of the tumour. In order, however, to detach this free part, I substituted for my fingers the crotchet, passing it high up and fixing it into the top of the tumour; and then, with the long bistoury anteriorly and posteriorly to the crotchet, its back riding against the uterine walls, I severed this segment of the shell, which was the only free portion in the cavity. Having now accomplished all which could safely be done with the knife, I next introduced my hand, and attempted to destroy more effectually the integrity of the shell with the ends of my fingers, scooping into its lacerated substance, comminuting it, and destroying many of its connections, and almost wholly disintegrating the entire mass. Many of the bands exceeded an inch in diameter, requiring considerable force to rupture them; but by operating on these with the edges of the nails, and making traction, they gradually yielded. In this way, I went over the whole interior of the uterus, breaking up the structure of the tumour. During this prolonged part of the operation the patient suffered most, and became considerably prostrated. This was followed by a chill and nervous tremor.

The operation lasted about two hours, and was exceedingly difficult, troublesome, tedious, and fatiguing; the uterus repeatedly contracting upon the hand and arm, incarcerating and paralyzing them. After this operation, the uterine tumour was found to have dimi-

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nished very much in size and firmness. The portions of tumour removed from the right side had lost their vitality; while some of those taken from the left side looked like living organized tissue, and were manifestly of a fibrous character. Little or no hemorrhage occurred.

Immediately after the operation, wine and camphor were administered, hot applications made to the feet and legs, and more clothing thrown over the patient to relieve her of a nervous chill, out of which she gradually passed in the course of half an hour.

We visited her two hours after. Reaction had come on rapidly, the pulse was 130, and the skin and tongue were dry. This excitement subsided in a few hours on the use of ice-water, and her pulse settled down to about 100, being open and full. Having in the evening complained of a severe burning pain in the back, an injection of flaxseed tea and laudanum was given with decided relief, and she passed a tolerably comfortable night, getting short naps.

So soon as she had passed the period of excitement resulting from the operation, she was placed upon the use of bark and the mineral acids, and good, wholesome, nutritious diet. The tonic contraction of the uterus was sustained by ergot, and the fetid discharge which followed for about two weeks was corrected by injections, per vaginam, of a solution of the hypochlorite of lime. With this discharge, small lumps and shreds of dead tissue came away.

The uterus was now smaller than is found after a first parturition, and the remaining enlargement was supposed to be dependent upon the hypertrophy of its walls, which, it was anticipated, would gradually disappear.

July 4. The patient had recovered strength rapidly, was up and about, but had imprudently been exposing herself for a considerable time at an open window.

7th. Before day, Dr. Kerfoot was sent for in haste. The patient had been troubled with diarrhœa (the usual effect of ergot in her case); and, after rising several times during the night, she discovered something coming from the vagina, which she supposed was the uterus. The doctor examined her, and removed a considerable portion of the sphacelated tumour, which had been thrown down into the vagina, and was lying there loose. After removing it, he examined her again, and, alarmed at finding what he supposed to be an inverted uterus, a consultation was called at 8 o'clock A. M., an hour earlier than usual, and upon our arrival we found the patient and family in a high state of alarm. A very careful examination

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was now made, and what was supposed to have been the inverted fundus of the uterus grasped by the os tincæ, proved to be a portion of organized tumour presenting in the vagina, surrounded by the os tincæ, and mainly attached to the left side of the cervix. The patient and friends having been assured of the true state of things, her apprehensions were allayed, and she passed a better day than any since her visit to Lancaster. The whole of the dead matter had been removed, no more fetid discharge followed, and the depression arising from such a source of irritation of course ceased.

It was at once decided that this tumour, which was as large as a medium-sized orange, should be removed; but preparatory to the excision, the patient was ordered to take, during the following day, a dose of ergot every four hours.

9th. At 4 P. M. Dr. K. made use of the knife, and also the crotchet, continuing the operation about an hour; and after cutting into the tumour in several directions, he desired me to examine the parts. I found the tumour itself incised in different places; but there still existed a large, broad attachment at the fundus of the uterus, running down the left side. At his request, I proceeded with the operation; and, seizing the tumour with the crotchet, I had pretty firm traction made upon it, and then severing the lateral bands with Cooper's bistoury, I made several transverse sections of the attachment to the fundus, and thus readily removed the mass. It was of a fibro-cellular structure, globular, covered with a thin, apparently recently formed membrane, with small nodules or granules upon its surface, and having an extensive base, between two and three inches broad, at which point it had been attached to the fundus uteri. This evidently was a portion of the original tumour, the shell of the organized portion on the left side, which, by the contraction of the uterus, had become aggregated and condensed into a single tumour, with all its original attachments. The amount of blood lost was from four to six ounces. There was less suffering during this than the preceding operation; but some prostration followed, from which the patient soon rallied.

On the 7th instant, after the great alarm already referred to, the patient was seized with cough, followed by copious expectoration. The pneumonic symptoms having gradually increased, a blister was applied to the chest, and all our treatment was directed to this new state of the system. During this period, a recumbent posture always induced incessant cough, while an elevated position afforded comparative relief. The pulse became more and more frequent and

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feeble, and respiration more accelerated, while expectoration diminished. On the 12th instant, the lower part of the left chest was noticed to be enlarged, over which there was drum-like resonance on percussion, absence of the respiratory sound, and most perfect metallic tinkling corresponding with the inspirations. The disease progressed, and the patient died at 3 o'clock A.M. of the 16th.

An examination of the body was made after death. The viscera of the thorax, abdomen, and pelvis were examined, and all were perfectly healthy in appearance except the lungs. The lower lobes of both lungs, and a portion of the adjacent lungs, were in a very high state of inflammation. The disease in the left lung seemed to be more advanced than that on the right; in the latter, the vessels were filled with cylinders or plugs of fibrin. The whole pulmonary tissue seemed to be affected. It presented a vermilion colour, which greatly contrasted with the grayish colour of the more healthy upper portions. On making incisions into the diseased parts, an immense quantity of reddish, purulent, frothy serum flowed out, amounting to about one quart. The diseased tissue was more easily torn than the healthy, and crepitation was absent, except by very deep pressure, and then scarcely perceptible. In other parts, the pulmonary parenchyma was dry. An old adhesion of the pleura existed in the lower part of the left side. Notwithstanding the symptoms of pneumothorax and perforation of the left lung strongly existed during life, no evidences of them were found in the examination after death, and yet the peculiar resonance and distension existed immediately before the body was opened. Neither was the stomach so much distended as to account for it, although it appeared to be the only thing which could explain the resonance. Upon turning up the sternum to examine the chest, a small quantity of brownish pus was discovered lodged under its upper extremity.

The viscera of the abdomen and pelvis were remarkably healthy. The uterus was entirely within the cavity of the pelvis, about four or five inches in diameter, free from adhesions, of a white flesh colour externally, and was *in situ* with its ovaries perfectly healthy. Its external surface was uniformly smooth and even. Its wall varied from half an inch to over one inch in thickness; of a dense structure, whitish, and appeared normally hypertrophied. The os tincæ was smooth and even, excepting on the right side there seemed to be a little excavation-like omission in its extremity. The interior of the uterus was free from inflammation. The remains of the pedicle of the last tumour, partly sphacelated, was recognized upon its

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fundus, and there was attached to the anterior part above another small tumour of the same character, and the size of a large hen's egg. The base of this tumour ran into the pedicle of the last, and it was evident that, like the last, this was a remainder of the original shell, condensed into a globular form by the contraction of the uterus. There was no distinct mucous membrane covering it. The same smooth passage, opposite the right acetabulum, was still very evident, and this was the only portion covered by mucous membrane, and free from adhesions. The remains of the tumour were closely attached to the muscular tissue, indicating that the tumour had its origin under the mucous coat.

The original tumour had existed for three years. In the month of January preceding the operation, by means of ergot, a large quantity of hydatids had been discharged, but notwithstanding this the tumour had still remained. Menstruation had been regular, with only slight menorrhagia, and health tolerably good. She was married and had had children.

Remarks.—The above case is presented in the same language in which it was recorded in the year 1845, at the time I was in attendance upon it. I had hoped that Dr. Kerfoot would have published it soon after its occurrence, and with that expectation I gave him a copy of my notes. It has not been done, and as he has been taken from his sphere of usefulness in this life, I feel bound, by its own importance, as well as by its relation to the cases which follow, to give it publicity. The complete success attending the whole course of treatment fully disproves the position, hitherto esteemed as an axiom by surgeons of authority, that polypus of the uterus cannot be subjected to operative measures until it has escaped from the uterine cavity.

It is much to be regretted that the exposure on the 4th, and the great alarm on the 7th, interfered with the recovery of the patient, by establishing a fatal disease in the lungs, at the very moment when the patient herself, her friends, and the surgeons, were congratulating themselves on the successful issue of this unique case.

CASE II.—*Mrs. J. M., aged 49 years; tumour intra-mural, having been developed in the posterior wall of the uterus, and expanding that wall into a cyst inclosing it; the whole tumour removed; supposed weight four or five pounds; recovered.*

Mrs. J. M., aged 49 years, resided in Harrison County, Virginia,

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left home on the 15th of May, 1849, in her own conveyance, and arrived in Philadelphia May 28, for the purpose of consulting me in reference to a tumour of the uterus. She gave the following history of herself:—

She commenced menstruating at 14 years of age, and continued to be regular afterwards, although generally the discharge was rather copious, and always attended with pain. She married when 18 years old, and had four living children and one miscarriage. Her last child is 28 years old. She nursed all her children, the last one for four or five years, when it weaned itself. Menstruation was not suspended by lactation. After the birth of her last child, the menstrual fluid was occasionally clotted during the first and second days, but not afterwards. Before marriage, for several years, she had leucorrhœa. In 1844, she observed an "inward weakness," with a bearing-down sensation in the groins, hips, thighs, and across the lower part of the body, which continued to distress her for about one year. During this time the fatigue of over-exertion was intolerable, and she was forced to get assistance in her household duties. If she was on her feet all day, she would be seized at evening with very severe pain, which always lasted until she assumed a recumbent posture. She described the pain as if something in the left side wanted to break. The pain returned for several days, and then there would be an interval of some days' ease. After this, she discovered a hard smooth tumour, in the left iliac region, as large as a hen's egg. She could not seize hold of it, but could pass her hand in above it, though not below it, nor readily between it and the hip-bone. When lying on the opposite or right side, the bowels would fall towards the declining side, but not the tumour—it seemed to be fixed to the hip-bone. At this time menstruation was exceedingly painful and copious—there was flooding, escape of large clots, and very great prostration. The first attack of flooding was at a menstrual period, and continued copiously for fourteen days, after which, there was a weeping until the next period, when hemorrhage again occurred, and this was renewed at every menstrual period for five or six months. She has greatly emaciated, in consequence, she supposes, of the necessary confinement, as she always rapidly lost weight under such circumstances, and rapidly increased again when about.

Before discovering the tumour she was troubled with great distress and confusion in the head, but which did not amount to pain. This has diminished, though sometimes it gets dark before her eyes, like

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the impression left after looking at the sun, particularly while engaged in sewing steadily.

The tumour rapidly increased for twelve months. During this time menstruation was regular, although she occasionally had severe pain, resembling labour-pains, accompanied with sick stomach, vomiting, &c.

In the spring of 1848, her health being very bad, her physicians ordered her to travel, and she started for Lynchburg, Va., a distance of two hundred and fifty miles. The first three days she travelled without much change in her health; the fourth day she suffered much from pain, which so increased by the fifth night that she was forced to stop near Staunton, where she became exceedingly ill with rigors, cramps, and vomiting, resembling cholera. She was conveyed to Staunton on a bed, where she received the attention of an eminent physician. Her menses came on with great pain and sickness, and continued for a week or ten days, accompanied with sinking spells and every appearance of death. In about seven or eight days she began to recover, and on the 20th of May started for home, still very feeble, but regained strength daily afterwards, and made the journey in eight days.

She decided to leave for Philadelphia on the 1st of September, 1848. A few days before, however, she again became extremely ill. Her physician, Dr. Austin, first considered this an attack of bilious fever, complicated with uterine disease, but afterwards concluded that the peculiar symptoms originated from the severity of the pain. He supposed that the tumour could be expelled, and for this purpose he gave her two doses of medicine in vinegar, which induced excessive pain, and brought down a tumour, which he said was in the walls of the uterus. He remarked at the time that any physician who had not known her situation, would have supposed that she was about bringing forth a child, the presenting part so much resembled a child's head. The os uteri, all this time, never opened. The pain commenced about midnight and arrived at its height about daylight in the morning. He saw her about 8 A. M. and endeavoured to control the suffering by opium. About 2 o'clock the same day, she sunk, having every appearance of death, but by stimulating frictions she again revived. The doctor, believing that some of the symptoms were traceable to constipation, gave croton oil, which operated suddenly in the course of five minutes. Next day the pain left the tumour, and attacked the left leg, causing excruciating agony. The leg swelled very much as far as the foot—it was white, and did not pit on pres-

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sure. Dr. A. called it milk-leg. She was long confined with it, and had not yet recovered when I first saw her. The leg was swelled badly for about three months, from which time there was much less pain in the tumour. Immediately after this attack, menstruation was suspended for three periods, then became regular, and, at the same time, the swelling disappeared slowly.

Sometimes there was difficulty in passing urine. It was attended with pain, burning, and a sense of obstruction. Sometimes while flowing in a stream, it stopped suddenly as if obstructed by a dense body, and then by elevating the tumour with the hand it again flowed. At times the urine was small in quantity and high coloured, containing a clayey sediment. She took a great deal of nitre, and with relief. Her bowels were regular.

Besides the above history of her health she had a severe attack of fever sixteen years since, and she had also been salivated three or four times. From the time of marriage she had had cough in the morning, attended with the expectoration of a frothy mucus. The cough latterly was not so troublesome. Her rest was very much disturbed, sometimes not sleeping at all during the night. She, however, averages about four hours' sleep.

Mrs. M., in passing through Lancaster on her journey to Philadelphia, called to see my brother, Dr. J. L. Atlee, who examined her, and wrote to me as follows: "I made an examination last night without an assistant, and repeated it to-day, assisted by Dr. Parry. . . . The fundus of the tumour now extends, especially in the morning, as high as, and to the left of the umbilicus; it is partially movable; there is no apparent fluctuation. The os tincæ is very high up behind, and rather *above* the symphysis pubis, and the pelvis is filled with the round-shaped tumour, resembling the fœtal head covered by the uterus. The os is oval-shaped, the long diameter extending laterally. Before menstruation there is always an *enlargement* of the tumour, and a subsidence afterwards. It has not increased much for the last six months. The examination per rectum discloses nothing new, the tumour being nearly central in the pelvis, although inclining to the left side above the pelvis. I introduced a uterine sound, Simpson's, 3⅞ inches. It passed up that distance above the symphysis on the anterior face, and could be felt on the most prominent portion of the tumour, and near the fundus of the tumour. The sound could not readily be turned when in the cavity, but by pretty strong torsion of the handle, the bulb of the sound could be felt in the mass by the fingers externally. Dr. Parry, by moving the tumour

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from side to side, readily produced a corresponding motion of the sound in my hands, and to apparently the same extent. I do not know that I need enter into any farther details of the case, but will state that I am of opinion that it is a fibrous tumour developed in the *posterior wall* of the uterus, and inclining a little to the left side. Were it *intra*-uterine, a freer motion would be given to the sound, which is now limited to the real or normal cavity, slightly elongated by the enlargement of the posterior wall. . . . Having formed this opinion, I told them that I thought an operation for the *immediate removal* of the tumour would not be advisable; that, after menstruation ceased, there might be no farther development of it; that she should keep her bowels regular, and take as much exercise as her strength would permit, her health having already been very much benefited by her journey."

May 28. I examined the patient. When on her back very little protuberance of the tumour was observed. The walls of the abdomen were very much loaded with fat, and a round, smooth, hard, unyielding tumour could be felt on the left side, extending up from behind the pubis to the umbilicus, filling the left iliac region and most of the hypogastric, extending towards the right, beyond the linea alba. It is movable, but not readily. Per vaginam the tumour could also easily be perceived, projecting into the pelvis like the head of a child passing the superior strait, and having much the same shape and feel as it has when covered by dense membranes, free of liquor amnii. The os uteri can be felt very high up, about one inch above the symphysis, and is reached with difficulty. The front lip of the os appears to be free, and can be raised upon the top of the index-finger, but coalesces, without much margin, into the anterior portion of the vagina, while the posterior lip has no margin, but appears, in connection with its part of the cervix, to be bulged and stretched over the tumour, and continued into the posterior wall of the vagina, which forms a covering for the tumour. The tumour can also be readily felt through the rectum, and seems to occupy, with its lower portion, a position between the rectum and the vagina. A hand placed on the abdomen, and a finger in the vagina, can play the tumour between them. Simpson's sound, passed into the uterus, enters to the distance of two and a half inches in front of the tumour, and can be felt through the walls of the abdomen. It is moved by every motion of the tumour. A sound passed into the bladder takes a direction to the right side. The speculum indicates a healthy con-

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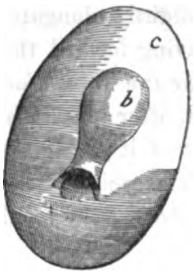
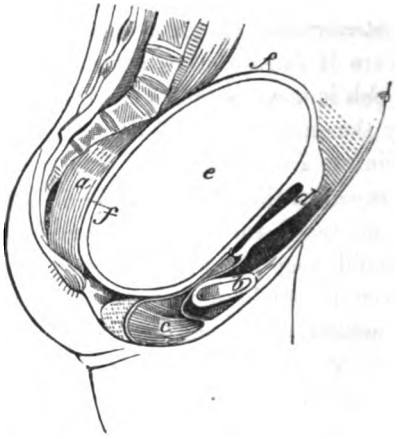
dition of the vagina. The accompanying drawings, figures 1 and 2, represent the position of the parts.

Fig. 1.



b. Uterus. *c.* Tumour.

Fig. 2.



a. Rectum. *b.* Pubis. *c.* Vagina. *d.* Uterus. *e.* Tumour. *f. f.* Posterior wall of the uterus expanded by tumour.

June 6. I examined Mrs. M. again in company with Drs. E. A. Atlee and Grant, who coincided in opinion with me respecting the character of the case. I now proposed an operation to these gentlemen, of the following character: placing the patient first fully under the influence of ergot, and then passing a bistoury into the cavity of the uterus about one and a half inches, I would cut down upon and into the tumour, slitting open the cervix and os uteri, extending the incision downwards through the posterior wall of the vagina about one inch and a half below the os tincæ. After making this section, I would attempt to detach the uterine envelop from the tumour by means of the point of the finger and otherwise, and thus gradually endeavour to enucleate the whole mass from its bed, and deliver it per vaginam.

My associates approving of this proposition, and it having been readily assented to by the patient and her husband, I accordingly ordered three grains of ergot every two hours. Four doses having been administered without effect, the quantity was doubled and continued until next morning.

7th. There having been no action from the ergot, fifteen grains were ordered every two hours, which produced no other effect than pain and swelling in the left leg. The limb was now placed in a

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bandage. Wine of ergot in drachm doses was substituted, continued until 10 o'clock P. M., and ordered to be resumed early the following morning.

8th. As there now seemed no probability of the action of the ergot in any way than by pain in the leg, which no doubt was induced by uterine contraction, I determined to proceed with the operation.

At 9 A. M. the patient having been placed with her hips on the edge of the bed, assisted by Professor Grant, I passed the index-finger of the left hand into the os tincæ, on which I passed the blade of a long bistoury into the interior of the uterus to the distance of an inch and a half. Dr. Grant at the same time supported the tumour with his hand placed upon the abdomen. I now turned the edge of the knife directly back against the tumour, and cut down upon it, drawing the instrument out of the uterus, and cutting into the tumour as I withdrew the knife. As I cut, the parts returned a grating sound, manifesting the existence of a fibrous tumour, which I think was incised to the depth of three-fourths of an inch. I now ordered one drachm of wine of ergot every half hour, and it was continued until 10 o'clock P. M., without any decided action upon the uterus. Towards evening the limb was less swelled. During the day I made two or three efforts to detach the cyst with the point of the finger, but the attachments were too strong to admit of it. The incision was followed by the loss of not more than half an ounce of blood, and was accompanied with very little pain. At 10 P. M. the ergot was discontinued, and ordered to be resumed early in the morning.

9th. 10½ A. M. Ergot having been administered every thirty and fifteen minutes, I severed half an inch of the posterior border of the cyst from the surface of the tumour, which was sealed to it by very strong adhesions. Very little blood followed. Before this, she complained of pain in the small of the back with some shooting pain through the tumour, which she had frequently experienced before. About 3½ P. M. I repeated this operation, cutting loose about one inch of the covering of the tumour, and about half an ounce of blood was lost. She suffered most of the day a good deal from pain in the back, and in consequence the ergot was repeated in the afternoon only every two hours. Ordered at bedtime 30 drops of laudanum.

10th. Discontinued the ergot. Operated twice to-day, detaching more of the cyst, particularly in front, and also passed the knife into the substance of the tumour. It is evidently softening where the covering has been detached, and the top of the tumour can be

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brought down one inch below the umbilicus—it is more movable than before. The last operations were more painful than the first.

11*th*. No ergot administered. Recommencing the operation, I seized the edge of the cyst with a blunt-hook, and brought the tumour down an inch and a half below the umbilicus. A string was now attached to the handle of the hook, and passed through an ivory speculum, which was introduced up to the tumour over the handle of the hook. We were thus, by means of a light, able to inspect the tumour for the first time, and found it degenerating.

12*th*. Hooked the cyst again and introduced a Ricord speculum, but found it inconvenient to operate through it. The tumour was softening and the size diminishing. I cut it up to some extent, and removed several small portions. They were fibrous and had lost vitality. A teaspoonful of wine of ergot was again ordered three times a day. After taking two doses, the patient complained of bearing-down pains, and the effects were more decided than heretofore.

14*th*. Ergot was continued yesterday, but with less effect. I operated again; and in making a section on the right side through a thick band, I came upon a plain surface over which I could pass the point of my index-finger. On the left side and in front, I severed a number of additional bands, and passed the knife higher up than before, so that Dr. Grant could readily distinguish the instrument through the walls of the abdomen.

17*th*. Incised the tumour through its centre to within about one inch and a half of the fundus.

23*d*. Gave ergot with some slight effect, and afterwards administered, by inhaling, my mixture of chloroform and ether; under the influence of which I operated again satisfactorily. The tumour has evidently diminished a good deal on the right side.

25*th*. The patient was menstruating, and with this was a discharge of degenerated portions of the tumour. When the tumour was pressed upon externally matter flowed from the vagina.

29*th*. I was sent for early in the morning to see Mrs. M. She was seized with a nervous tremor, or chill—a state of congestion, indicated by coldness of the surface, blue lips, and great distress at the epigastrium. I immediately applied sinapisms to the extremities and stomach, and gave morphia; the tremor subsided in about an hour. This was followed by some febrile excitement, and considerable distress of stomach, with occasional vomiting, which continued all day and the succeeding night. The patient attributed this attack

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to constipation, and said it was similar to, though less severe than the attack at Lynchburg.

30*th*. The sickness and distress of stomach still continuing, Mrs. M. took two cathartic pills, which were followed by a dose of oil and laudanum in the evening. Two or three evacuations were induced with decided relief. She regained her ordinary condition in two or three days.

July 5. Another operation was performed under the influence of chloroform. Several firm adhesions, between the front part of the tumour and the uterus, were severed; the vaginal section of the cyst was extended towards the rectum, and, with the point of the index-finger, the tumour was detached or enucleated from its covering as far as could be reached. The tumour was now comparatively soft, and presented to the touch a sensation very much like that of a polypus in the interior of the uterus; the whole lower portion being separated from its attachments. More bleeding followed this than previous operations. There were also more bearing down and soreness in the tumour; to relieve which a dose of laudanum was given. Ten grains of powdered ergot were ordered every two hours.

6*th*. The patient had rather a bad night. The ergot and some raspberries, which she imprudently ate last evening, distressed her stomach and bowels considerably; pain continuing all day, and ending in bilious diarrhœa at evening.

7*th*. She had four or five copious yellow, thin discharges last night and this morning, which weakened her a good deal. The stomach was still much distressed; and the attack assumed the character of cholera morbus, which excited the patient very much, from the fact that cholera asphyxia was prevailing in the city. A sinapism to the stomach with an infusion of peppermint taken in small quantities, and iced, quieted the stomach, so that at 10 o'clock P. M. it tolerated a dose of castor-oil and laudanum.

8*th*. The patient slept well during the night. The oil operated freely, with entire relief to all the symptoms. In the evening she was quite well.

9*th*. The patient was very well. Examined per vaginam and found some agglutination between the tumour and the sac where the separation had last been made—but this was soon destroyed by means of the index-finger.

11*th*. Examined again while under the influence of chloroform. Passed the finger around the tumour, breaking up all the adhesions

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within reach. This was followed by considerable discharge of an offensive character, with small pieces of broken-up coagula.

18*th*. Offensive discharge continues.

18*th*. The patient has been regaining health rapidly, the tumour has diminished in size, and the offensive discharge continued. She felt well enough to set out for her home; and as I did not intend to carry operative measures any farther, and as she had a great dread of cholera, she left for Virginia.

I received a letter from her husband after their arrival at Lynchburg, as follows: "We arrived here in 13 days, a distance of 348 miles. For the first 100 miles, Mrs. M. seemed to be very feeble, had no appetite, her legs swelled very much every day; but the swelling would go down at night. After we passed Baltimore, she began to improve, but complained a good deal of soreness about the tumour. Since we arrived here, say for three days only, she seems to improve rapidly. She is in much better health now than she has been for three years; walked at least 300 yards to a spring and back yesterday; the lameness has left her leg; no swelling in her legs of notice; she has a fine appetite; seems to be in fine spirits; gets up out of her chair and walks about the house like a girl of sixteen. The tumour seems near the same size as when we left; I think at this time it seems to be on the decrease, and seems very low down. On the whole we are in spirits respecting the case; we are yet afraid to say she is well, though we almost believe she is. We can write more satisfactorily to you by the time we get home; she is improving in appearance as fast as any one I ever saw."

December 30, 1849. The following is an extract of a letter of this date written from Flowerdale, Harrison County, Virginia: "I have delayed writing until the present in order to give you some definite information in regard to Mrs. M.

"We resumed our journey homeward, from Lynchburg, some 250 miles farther. During the progress of the journey she continued to improve, had a good appetite, could get in and out of the carriage easily, and would walk frequently a considerable distance. On arriving home, we found our only daughter just recovering from a severe spell of sickness. This had a tendency to depress her spirits for a short time. With the speedy recovery of the daughter, however, she commenced recovering also. The tumour gradually decreased, and she became in the perfect enjoyment of health. She was so much restored by the 15th of October, I thought it safe to leave her upon business. I returned on the 27th of last month, and am most happy

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to say to you that she is entirely recovered, and in better health than she has been for the last fifteen years. The tumour is entirely gone, to all appearance, and she has good use of all her limbs; can ride on horseback, &c."

1853. Mr. M. has visited Philadelphia repeatedly since the period described, and says his wife continues in the most perfect health, there being not the least vestige of the tumour.

Remarks.—It will be observed that in the above case the tumour was developed in the posterior wall of the uterus, having no connection with its cavity—a state of things that precluded all idea of its accessibility to the ligature or the knife on any established plan or rule of surgery applicable to uterine tumours. It will also be observed that although a long and deep section of the tumour and its coating was made, no hemorrhage followed, notwithstanding severe floodings had previously occurred. The operation attempted was that of enucleation, but the greater portion of the mass gradually wasted away by a species of decomposition. The phlegmasia dolens likewise disappeared as the tumour diminished in size.

CASE III.—*Mrs. J. McB——, aged 30 years; tumour intra-uterine; os tincæ thick and closed; whole tumour removed; supposed weight six or seven pounds; recovered. Subsequent reproduction of the tumour; again removed; recovered.*

June 18, 1849. I examined Mrs. McB——, of Carlisle, Pa., aged about 30 years, who had a large tumour in the abdomen. It occupied the centre of the abdomen, and extended above the umbilicus, inclining with its fundus to the right side. It was quite firm, semi-elastic, not fluctuating, and sore to pressure over the right superior part. Per vaginam, the cervix uteri was found to be expanded at its upper part, the os tincæ thick and ring-like, and of scirrhous hardness, with an opening that would receive merely the extreme point of the index-finger. There was no tenderness in the parts. With the speculum, the os tincæ could be readily seen, but it was quite free from abrasion, or any peculiar marks of disease.

19*th.* I examined the patient again, using the uterine sound, which I could pass between the tumour and walls of the uterus in almost every direction up as far as the fundus. The point of the sound could be felt through the parietes of the abdomen above the umbilicus. While exploring with the sound, it seemed as if loose adhesions between the two surfaces were giving way. The pedicle ap-

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peared to be near the fundus on the right side, as the sound could not be made to pass up in that direction. After finishing the exploration with the sound, I attempted to force the index-finger through the os uteri into the interior of the uterus by a kind of boring operation, while fixing the tumour by external pressure on the abdomen. By continued effort I succeeded in introducing it up to the second joint, when I felt the tumour against the end of the finger. The os tincæ was very rigid and thick, and grasped the finger very tightly, while the force required to introduce it gave the patient considerable pain.

This examination was followed by slight pain and very little bleeding. The diagnosis was polypus of the uterus.

28*th*. Having proposed to place the patient under the influence of ergot, and when the contractions of the uterus were established, to cut the os uteri to enable the tumour to descend, I ordered that she should take 10 grs. of powdered ergot, in infusion, every two hours until evening, and then to omit until morning.

30*th*. The ergot acted effectually, and, as tested by an examination per vaginam, was found to make an impression upon the cervix. The whole mass descended lower in the pelvis and its fundus was not so high in the abdomen.

July 1. The ergot continued to act powerfully. The abdominal tumour was three or four inches below its original level, and more globular and firm. It seems as if the fundus had been tilted outwards in order to enable the lower portion to pass into the superior strait of the pelvis. The tumour can also be more readily detected through the os uteri.

In the presence of Dr. Grant, with the long bistoury I nicked the os uteri quite around, which caused it to relax to such an extent that the whole index-finger could be introduced and passed easily round the entire lower portion of the tumour. It had a soft and quite elastic, semi-fluctuating feel.

2*d*. The ergot still acted finely; the cervix was nearly expanded, and the os uteri exhibited an opening about one inch and a quarter in diameter. The tumour was much lower in the pelvis and abdomen. The os uteri being still firm, I cut it more extensively than at first, so that it yielded and expanded to a much greater extent. The use of the ergot was ordered to be discontinued after midday until the morning following.

This evening, the tumour was found to have descended more and

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more, and was pressing upon the neck of the bladder so as to embarrass the passage of urine.

3d. The os tincæ was still dilating, and urination was difficult. Three 10-grain doses of ergot were given during the day. Ordered castor-oil, which operated well.

4th. After taking 10 grs. of ergot, very violent contractions of the uterus were induced, so that it required to be controlled by morphia. The os is still dilating, and the tumour is beginning to press against the perineum. It is about one inch and a half from the os externum.

5th. This morning, I passed the sound into the uterus with the same result as before. I again slightly nicked the os tincæ. In the afternoon the os was more dilatable, capable of being stretched to three or four inches in diameter. Ordered but one-third the former dose of ergot every two hours, in consequence of the violent action the previous day.

6th. This morning, I endeavoured to grasp the tumour by Denman's small obstetric forceps. They were readily introduced, but could not be locked easily, the difficulty appearing to depend principally upon the os tincæ not having been sufficiently dilated. They were also too small to grasp the tumour, and too straight to apply to it above the brim of the pelvis.

In the afternoon, assisted by Dr. Grant, I attempted the use of Seabold's large forceps. The blades were introduced; for the same reason there was also a difficulty in locking them, but finally I succeeded; then placing the patient under the influence of chloroform, I made gentle and continued traction in order to get the tumour down through the superior strait, but it was too large to admit this. After endeavouring for some time to accomplish its delivery in this way I withdrew the forceps. Upon examining now, it was found that a considerable change had occurred in the tumour; the pressure of the forceps had very much softened it. I now passed in the two blades of Smellie's crotchet, and after the same difficulty, locked them, sinking their points into the substance of the tumour. On making traction the tumour tore under the pressure of the instrument, which of course lost its purchase, so that I had to withdraw it. I now employed only one of the blades, passing it up very high upon the tumour, particularly on the left side opposite the pedicle, and, sinking it deeply into its substance, made careful traction. The tumour again gave way; but, aided by the strong pressure made by Dr. Grant upon the hypogastrium, at the same time opposing the

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point of the instrument with the left index-finger in the uterus, I succeeded in extracting a considerable portion of the mass through the os externum, where I held it to prevent its retreat into the vagina. Now, passing the crotchet repeatedly into the uterus on all its sides, I continued to bring down portions of the tumour, until I finally succeeded in detaching about one half the entire mass. The portion remaining in the uterus I afterwards divided considerably with the hooked end of the crotchet, aided by a hook contrived for the purpose. This was done with the view of disorganizing the remainder of the tumour, and inducing it to slough away. During the whole operation the patient was kept under the influence of anæsthetics. The external bulk was very much diminished. Not more than two or three ounces of blood were lost during the operation, and no shock followed it. Ordered a small dose of ergot every two hours.

7*th*. A large quantity of bloody water followed the operation yesterday, sufficient to wet ten or twelve napkins; but it had not the effect to debilitate the patient. She complained of slight soreness over the hypogastric region; for the relief of which I ordered cold-water compresses, under which treatment both it and the discharge diminished. At evening, I examined the patient, and found the balance of the tumour descending into the vagina. Flakes of flesh were discharged with matter from the uterus. The patient was quite cheerful.

9*th*. The discharge had now become fetid, showing a decomposing process in the tumour; and I removed another large portion, the masses being partially putrid, having the putrid odor. After this, the uterus contracted to the size usually observed after first parturition. The loss of blood did not exceed two ounces. Chloroform was administered with good effect. There still remained membranous and tough portions of the tumour, probably the remains of the pedicle, and which could not be torn away. Some of them, while held on the stretch with the forceps, were cut away with the bistoury.

12*th*. The uterine tumour had gradually diminished to the size of a large orange, and could be felt at the top of the pubis. Considerable discharge had occurred since the last operation, accompanied with shreds of degenerated tissue of a fetid odor. To-day, after giving chloroform, I removed several small offensive portions, and broke up the balance of the pedicle. I imagined the present apparent size of the pubic tumour resulted more from the thickened walls of the uterus than from anything within its cavity.

18*th*. The patient had improved in strength rapidly. She rode

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out on the 17th, and now left in the cars for Carlisle. She had been taking carbonate of iron for several days, and was ordered to continue it after her return home.

August 20, 1849. I received a letter from her husband, who says: "We are getting along as well as could be expected from so severe an operation. A few days after we left you, there was a very severe pain in the right groin, extending over the place where the tumour was fast, and as far up as the ribs. We were a little afraid of the consequences. Different things were made use of for its removal, but to no effect." She was advised "to bathe twice every day, which in the course of a few days relieved her entirely of the pain. Since that, she has been more hearty than for seven or eight years. Her appetite is very good, and she can go up and down stairs without sitting down as before," &c. &c.

Some time after this Mr. M'B. was in the city, and stated that his wife was enjoying excellent health. Menstruation had occurred quite regularly.

The following are some of the features of the history of the above case. When about thirteen years old she first menstruated. Soon after she had an attack of intermittent fever, which continued about a year, during which there was an entire suspension of the menses. After this, menstruation returned and remained regular up to the year 1839, after which the discharge assumed a black appearance. For years before, however, she was weak and delicate. She never suffered from dysmenorrhœa or leucorrhœa. In 1842, she was seized with severe pain across the kidneys, and in the head and face, accompanied with some derangement of menstruation. The patient thinks that the uterine disease commenced at this time. No treatment relieved her. She was married on the 28th of May, 1844, with the hope of regaining her health. She was, however, disappointed. On the 4th of July succeeding, she was seized with excessive flooding, accompanied with large coagula. After this, she rapidly lost health and strength; had repeated floodings, particularly at the menstrual periods, and continued to suffer great pain in the back, head, and jaws. On the 12th of June, 1846, a very acute pain seized her in the lower part of the abdomen. She was at this time, under the care of a "Sugar Doctor," as Mr. M'B. called him. Small portions of decayed flesh came away in the discharge from the uterus, giving out a very offensive odour. This continued until the morning of the 20th of June, when the passage of the urine was entirely arrested. The "homœopathic" practitioner was sent for and examined the patient;

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he pronounced her to be in a state of parturition, and that a child was about to be born! He applied the forceps, as he supposed, to the head, and dragged upon the subject until 4 o'clock P. M., without being able to accomplish the delivery. By this time, the patient and friends having lost all confidence in the operator, sent for Dr. C. of Carlisle, an aged and excellent physician. On the announcement of Dr. C.'s arrival, the SUGAR DOCTOR made a precipitate retreat, *not even taking time to remove the forceps from the head of the child in the pelvis!!* Dr. C. now took charge of the case, removed the instruments, and found the pelvis occupied with a very offensive and putrid polypus, which he removed in small pieces in the course of half an hour. Under Dr. C.'s care she gradually recovered pretty well, had very little flooding, and regained strength tolerably well. In the autumn of 1848 she had a return of the old symptoms, and the abdomen rapidly enlarged. The sanguineous discharge was so great that she became perfectly anæmic; and when brought to Philadelphia, she seemed quite drained of blood; the skin having that peculiar straw-coloured and blanched condition so often observed in malignant and other chronic diseases that interfere with the functions of nutrition and sanguification.

The following is the copy of a letter received from Dr. H. Hinckley, an intelligent physician of Carlisle, in reference to the above case.

"CARLISLE, *Nov.* 29, 1851.

"WASHINGTON L. ATLEE, M. D., PHILADELPHIA.

"DEAR SIR—I have a patient just come under my notice, upon whom you operated for uterine tumour some three years since—Mrs. R. M'B. I shall be under an obligation to you, if you will do me the favour to inform me of what nature the tumour was, whether fibrous, scirrhous, or polypus; what were its relations to the womb, and in what manner you removed it; also to what extent it had gone. Please state also if the operation was accompanied with much hemorrhage. She is now suffering a good deal from the presence of a tumour, intra-uterine, I believe. I have not made a special examination, intending to do so to-morrow, having only seen her this evening. I am apprehensive she will have to incur another operation, but am desirous to hear from you before I make a final diagnosis or give the patient advice. What information you may be pleased to give relative to the tumour removed by yourself will materially aid me in an examination.

"With respect, I remain, &c.,

"H. HINCKLEY."

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Dr. Hinckley wrote again as follows, Dec. 7, 1851: "Yours was duly received in answer to my interrogatories, for which I am obliged. On the 30th ult. I made an examination, and discovered what I supposed was a fibrous tumour of the uterus. The os was dilated to the size of a dollar, soft and expanding—bearing-down pains present and very severe. Concluded if possible to operate by ligature. On the 1st instant I made nearly straight two male silver catheters, made a hole in the end of each, and passed a strong silk ligature through them, bringing the free ends out next the hand. Ergot was administered two days in succession, but the pain was so severe that I had to administer opium freely, and stopped the ergot. On Monday I attempted to pass the ligature. The os much dilated, soft, and the tumour protruding somewhat; could not succeed, on account of finding it attached by at least its upper half to the uterine walls. On Friday, proceeded to operate by avulsion, tearing up the tumour and bringing it away with a retractor or extractor [obstetrical], sharp only on the inside of the hook. The patient was under the influence of chloroform. I succeeded in getting all of the tumour except a few shreds of the attaching membrane, though it took me two hours and more, the womb sometimes contracting forcibly, obliging me to desist for the time being. I did not carry the chloroform to its full effect, but lulled the pain. Four hours after the operation, saw the patient—pulse 140. Irritability of stomach very great—vomiting—prostration great, though not from hemorrhage, as there was little or none from the operation, and none previously. I attributed some of the symptoms to the long use of chloroform. Ordered ice to be given in small pieces, thirst being intolerable—mustard to epigastrium—warm bricks to feet and warm clothes to vulva; aqua calcis and milk. The patient passed rather a restless night—somewhat better in the morning—pulse 120—irritability still present, being perfectly free from pain, however. Continued lime-water and milk—ordered injections per vaginam of warm opiate water to wash out debris, &c. Patient somewhat stronger in voice, and anxious to eat—ordered barley-water, a few spoonfuls. Being called to the country some distance, I did not see her the following evening. This (Sunday) morning I find the patient improving—pulse 80—irritability of stomach lessened, and symptoms generally better. Ordered a small quantity of chicken-water, with a cracker. The womb has been well washed out with injections, and there appears no danger now from metritis; but I can hardly pronounce her yet safe."

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Dr. Hinckley farther writes, under date of December 27: "I am pleased to inform you that the result has been favourable, and the patient is fast regaining health and strength. There was some slight effusion or rather œdema of the lower extremities *before* the operation, but since, it has disappeared and there seems now to be no indication of its reappearance."

Remarks.—This case is interesting in several points of view. It shows that in some cases there is a great tendency in the disease to return, as there was a reproduction of the tumour twice—once after it was thrown off by nature, and again after it was removed by the knife. It also shows that an intra-uterine tumour of large size may be expelled by the uterus, may degenerate and lose its vitality, and thus be destroyed by a natural process unaided by art. It also encourages us to adopt curative measures, even in extreme cases of anæmia, and when there are strongly marked indications of a malignant taint of the system; as, after the operation, the health was restored, and the healthy functions of the uterus returned. Not the least interesting point in the above case is, that it very conclusively proves, what has been denied, that ergot will most effectually excite the muscular action of the uterus, not only while in labour, but also in an unimpregnated and tranquil state.

Case IV.—*Miss M. T——, thirty-three years old; tumour intra-uterine, its entire surface intimately incorporated with the interior of the uterus; its removal attempted by gastrotomy, which failed; subsequent recovery, and an attempt made to remove it per vias naturales; death from erysipelas.*

Miss M. T——, aged thirty-three years, of Buffalo, New York, came to Philadelphia May 12, 1849, to consult me about a tumour in the abdomen. The patient gave the following history of her case. Seven years ago she had an attack of typhus fever, which confined her six or eight weeks. Eighteen months after, she had what her physician called yellow fever, and she has had several slighter attacks of fever since. Two years before discovering the tumour, she had a cutaneous eruption, which lasted three or four weeks, and was treated by Dr. T——, of Buffalo, with bichloride of mercury and sarsaparilla internally, and ointment of red precipitate externally. The catamenia made their appearance at sixteen years of age, and were painful until the period of the attack of typhus fever, after which they were less so; they have also been regular to the time of the

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discovery of the tumour. July, 1845, she first noticed the tumour in the right side, midway between the ribs and pelvis. It was then as large as the fist; oblong in shape; the longer part projecting upwards. It seemed smooth, hard, and unyielding; was perfectly movable; could be pushed over to the linea alba; be depressed and elevated, and upon removing the pressure, it would again return to its original position. About three months after this, Dr. P——, of Providence, Rhode Island, examined the patient, while lying on her back, with her abdomen exposed. He described the tumour as decanter-shaped, with a narrow attachment to the right side of the uterus. He could play it about, and considered it an ovarian tumour. The succeeding winter, 1845–46, it grew to half the size it is now, having enlarged gradually. The following spring, 1846, it again gradually diminished to about one-third its previous size, without treatment. The next summer it again increased, and continued to do so until the latter part of the fall, when it again diminished, also without treatment. It now remained stationary until the next fall, 1847, then increased gradually to a greater bulk than ever.

In the spring of 1848, she placed herself under the professional care of Dr. S——, of the city of New York. He ordered chloride of gold and citrate of iron internally, and solution of the acetate of iron as a vaginal injection. A magnetic plaster was placed on the back, and the electro-magnet, with one pole on the abdomen and the other on the back, was repeatedly employed. This treatment was associated with cathartics of croton oil; and, during it, the flatulent distension of the epigastrium subsided, but the tumour diminished very little.

After this treatment was continued about three months, Dr. S. added Indian hemp and prussic acid. The hemp acted powerfully as a diuretic for several weeks, and the patient diminished in size. This treatment was continued uninterruptedly for six months; after which it was suspended, in consequence of the death of her physician. Dr. S. called her disease ovarian dropsy. From the fall of 1848 to the present time, there has been a gradual increase of the tumour.

March, 1849, the patient went under the charge of Dr. M——, of Albany, New York, who named her disease ovarian tumour.

Believing it to be filled with fluid, he, early in April, introduced a large-sized trocar, for the purpose of tapping it. Nothing flowed through the canula, except a teaspoonful of blood. The trocar was introduced about three inches, and it required considerable force to enter it. On putting a probe through the canula, it came against a

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hard unyielding substance, jutting against the internal end of the tube. The wound healed up kindly. Dr. M., now considering the tumour to be fibrous, but still ovarian, recommended the application of caustic to the abdomen, in order to establish adhesive inflammation between the parietes and the tumour, and coetaneous ulceration—no doubt the new treatment recommended by Dr. Tilt, of London, for ovarian dropsy. The patient, however, did not undergo this treatment. After tapping, she had a slight bilious attack, for which she was attended by Dr. C——, of Cohoes, Albany County, New York, where she then resided.

In the summer of 1847, the patient had menorrhagia for about two months, with only one week's intermission, and afterwards the menstrual discharge was less in quantity. It was, however, regular, but slightly clotted, as it always had been. It was also succeeded by slight leucorrhœa. Urination and defecation were never troublesome or difficult. Until the spring of 1849, the bowels were disposed to relaxation. Sleep was broken; she dreamed a great deal; occasionally started, and did not feel refreshed in the morning; had a sensation of great fatigue, and liked to indulge in bed in the morning. The appetite was good, and she felt well after eating. The tongue was coated with a dyspeptic fur, and she was troubled with flatulency. The respiration was frequently affected; she could not, at times, get a full breath; had a frequent disposition to sigh, but could not accomplish it until she had made several attempts. She described this difficulty as a feeling of partial paralysis. A very uncomfortable feeling through the body distressed her, with considerable soreness of the tumour, particularly at its upper part. Last winter, and especially since last spring, headache had annoyed her. The pulse, when free from excitement, was ninety-five to the minute, and of good tone and volume. Auscultation discovered nothing wrong in the functions of the heart and lungs.

I have been thus particular in detailing the early history of this case, for reasons which will be explained by the result of the operation, as well as in consequence of the intelligence of the patient, whose truthfulness and accuracy I could fully confide in.

On the evening of May 12, I examined Miss T. When lying on the back, the abdomen, exposed, showed an uneven prominence. A tumour occupied the left iliac, hypogastric, umbilical, and right hypochondriac regions, part of the right lumbar, and part of the right iliac regions. Its longest diameter extended from the right hypochondrium to the left iliac region, and measured thirteen inches; its

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shortest diameter cut this at right angles, and was eleven inches; across the tumour horizontally, and intersecting the umbilicus, it was twelve inches. The abdomen was resonant on percussion over the epigastrium, the left hypochondrium, and the left lumbar regions—all other parts were flat. The walls of the abdomen were considerably loaded with fat; and the lower part, inclining towards the left, opposite the most prominent part of the tumour, had its subcutaneous cellular tissue somewhat infiltrated, so as to present a doughy appearance and feel. The cicatrix made by the trocar was seen about three or four inches to the right of the umbilicus, and about one inch below it. The tumour was firm; slightly elastic; did not fluctuate; could be moved in all directions by using considerable force—the opposition to its motion depending apparently on the tightly distended abdomen, and the size of the tumour.

Examining per vaginam and per anum, the patient being on the left side, the tumour was found to occupy a large portion of the pelvis, and the uterus could not be distinguished. I was therefore impressed with the idea that the tumour was entirely uterine, and that no os tincæ was discoverable. The tumour could be slightly played between the finger in the vagina and the hand upon the abdomen. Placing the patient upon her back, however, and passing the index-finger far back, the os uteri could be detected, but the cervix uteri could not be traced; it seemed to be spread out and flattened, while the os uteri was sufficiently opened or stretched to permit the point of the index-finger to enter partially. It was only with great effort, and by pressing back as far as possible, that the index-finger could be made to reach the os tincæ.

May 13. In company with Drs. E. A. Atlee and W. R. Grant, I examined the patient again, using the sound both for the uterus and bladder. The uterine sound readily entered the patulous mouth of the womb, but would not pass up more than about an inch until the tumour was elevated by pressure externally, when it entered to the distance of two and a half inches. It took a direction towards the right side. While in the uterus, motion, communicated externally to the tumour, gave scarcely any impulse to the sound, and *vice versa*. Very little motion, however, could be made with the point of the sound in consequence of the uterus appearing to be fixed in its position. The vesical sound also took a direction towards the right side, indicating that the bladder occupied the right side of the pelvis. The patient complained of both sounds giving pain.

It will be apparent that the history of this case, compared with

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the present condition of the patient, rendered the diagnosis difficult and obscure. The prominent indications of its early history were those of ovarian tumour, and the eminent surgeons, who had examined it, so considered it. It is to be regretted, however, that heretofore no examination, per vaginam and per rectum, had been made, as it is probable that its real nature would have been more readily disclosed by such an examination before it assumed so formidable a size. Apart from its history, I should have considered the tumour to be either in the cavity of the uterus, or in its walls; but, viewing it in that connection, and respecting the opinions of those gentlemen who had examined the case before, I was willing to believe it to be extra-uterine, consisting of either the right ovary, which had undergone fibrous degeneration, or a pedunculated fibrous tumour of the uterus itself; and that its extirpation by gastrotomy was possible. It was, therefore, decided that, as the patient came a considerable distance, and had an urgent desire to be relieved by an operation, the nature of which had been fully explained to her, it should be attempted on certain conditions to be presently stated.

14*th*. I informed the patient of the result of the examination, and of the obscurity in the diagnosis, and that, in case an operation was performed, there must be a clear understanding on her part, as well as that of her friends, that the tumour possibly could not be removed. I also told her that I would operate with the expectation of removing the tumour, but that if, upon opening the abdomen, it was found impossible to remove it without extirpating the uterus, I should not feel justified in doing that, and would close the wound without delay. The dangers of the operation, in any event, were also fairly and fully stated.

My intelligent patient seemed to have made up her mind to undergo the operation; desired that it might be performed, but felt much depressed at the announcement that there was a possibility of the tumour not being removed. The operation, therefore, was determined on.

21*st*. My brother, Dr. J. L. Atlee, of Lancaster, examined the patient, and took the same view of the case.

22*d*. This morning, at ten o'clock, was the time fixed for the operation. Before proceeding with it, however, I stated to the medical gentlemen present the obscurity in the diagnosis, that the tumour probably could not be removed, but that I expected to accomplish it, even if I had to enucleate it from a cyst in the walls of the uterus. The only obstacle to its removal that I conceived probable, was, either that the tumour consisted of a hypertrophy of the uterine walls them-

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selves, or that it was formed by a tumour within the cavity of the uterus.

The abdomen was opened, and the mass was turned entirely outside of the cavity of the abdomen, and could be readily surveyed with the eye upon all its sides. It consisted of the uterus itself, which was either hypertrophied or else distended by a solid body within it. The peritoneal coat was elevated in several places from the surface of the tumour into cystiform bodies, filled with a yellowish fluid. One of these, about the size of a walnut, was at the top of the fundus, another larger and more irregular on its left side, and a couple of smaller ones lower down. The right ovary was enlarged to the size of a small orange, and the left was about three times its natural bulk. Both ovaries seemed to be attached to the uterus so low that the great body of the tumour was above the points of attachment. Such being the condition of things, it was considered improper to remove the tumour. It was returned, the wound closed, and the patient rapidly recovered.

After the patient had recovered from the operation, she was exceedingly desirous that something more should be attempted for the removal of the tumour. During her convalescence, it happened that a lady arrived in Philadelphia from Virginia, with a large fibrous tumour imbedded in the posterior wall of the uterus, and another lady from Carlisle, Pennsylvania, with a tumour as large as her own, sealed within the cavity of the uterus, both of which I succeeded in removing safely by the knife, *per vias naturales*. Hearing of these cases and their successful results, she was quite solicitous to have a similar operation upon herself.

Accordingly, June 25, 1849, I examined her in reference to the propriety of such an operation. I again tried the uterine sound; it passed in, as before, to the distance of two and a half inches, when its progress was arrested. By urging it onwards, however, it seemed to pass between the walls of the uterus and the tumour on the sides; at the same time a sensation was communicated to the hand as if weak adhesions were lacerated by the point of the sound. After destroying these apparent adhesions between the tumour and the interior of the uterus, the sound passed up a considerable distance. The os tincæ scarcely admitted the point of the index-finger; it was very firm and resisting; the finger could merely be pressed into it, not through it, and could not, consequently, be inserted so as to feel the tumour within. The papillary form of the cervix was entirely

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lost, but the sound showed that, at least, one inch of uterine tissue was traversed by it before getting within the interior.

June 26. The patient said the use of the sound was followed by pain similar to labour-pain, and she complained of considerable soreness along the sides of the tumour touched by the sound. Very little sanguineous discharge followed. The top of the tumour had become depressed for the distance of three fingers' breadth, and a swelling of the left leg, which had existed several days, had also diminished very much.

27*th*. The old soreness at the top of the tumour in the right hypochondrium was less than before the operation of gastrotomy, although it had been greater afterwards. It had decreased greatly, and the rest of the soreness following the use of the sound had disappeared.

At the time of using the sound, the patient had had the catamenia for several days.

This examination, taken in connection with the appearances of the parts exposed by the operation of gastrotomy, induced me to conclude that the tumour existed within the cavity of the uterus, attached extensively to its interior surface, and that it was possible to remove it through the natural passage. I advised her, however, not to undergo the operation at present, but to return to New York, resume her usual habits, regain her general health, and revisit Philadelphia the ensuing autumn or spring. This advice was given in the hope that by that time nature, aided by art, might produce a more favourable condition of the os tincæ. She reluctantly assented to return home, and was only detained until I could procure for her some of the fresh crop of ergot, then maturing.

July 11. The patient improved in health, occasionally walking out, though she took less exercise than she desired in consequence of an unsightly œdematous swelling of the left leg. She had a return of soreness in the right hypochondrium. I again introduced the sound, which appeared to pass around the posterior and sides of the tumour. No change had yet occurred in the os tincæ.

This examination was attended with less pain than the other, and there was no stain of blood on the finger.

16*th*. The patient went home to-day, provided with a quantity of fresh ergot. She was directed to take ten grains of powdered ergot every two hours, until uterine contractions were induced, or until one drachm was taken, and then to suspend its use for one week, my object being to dilate the os tincæ and cause the tumour to descend more into the pelvis.

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August 3. My patient writes: "I do not think my health has been as good since I left Philadelphia. I am very weak, and have a great deal of pain in my side, over the upper part of the tumour, where it has troubled me so much. I have taken ergot twice. The first time, it gave me a great deal of pain, and I had a high fever. It affected my head very much. That night I could not rest at all, and the next day I felt very badly. Yesterday, I took it again. In half an hour after I took the first powder the blood rushed to my head, my face seemed swollen, and there was difficulty in breathing. After taking the second powder my limbs trembled badly, and my flesh was hot and feverish."

In consequence of these unpleasant symptoms, I directed her to take only half the quantity of ergot.

20*th*. She again writes: "Lask week I took, according to your directions, half the quantity of ergot. The effect was the same as when I took the full dose. It affects my head more than any other part of my system. I am almost afraid to meddle with it until where you can see the effect produced. My health is quite good; but I am very weak, and bloat very badly. My appetite is good, or rather craving; but everything I eat produces a very oppressive sense of fulness, and an uncomfortable bloating of the stomach and abdomen."

I immediately ordered the discontinuance of the use of ergot.

October 11. The patient returned to Philadelphia in the enjoyment of tolerably good health, having recovered the tone of system she had lost during her previous confinement.

21*st*. I examined the case again. The abdomen was rather larger, the tumour being wider and higher, which, I think, was accounted for by the position of things in the pelvis; the uterus was elevated from the cavity of the pelvis; the os tincæ was thrown more forward, and was much more closed. In other respects, the patient was much the same as when she left Philadelphia.

22*d*. In company with Professor Grant, I passed through the os tincæ a probe-pointed, curved bistoury, and slightly nicked it in several directions. This was attended with very little pain, and followed by only a few drops of blood. I now ordered ten grains of powdered ergot to be taken every two hours until uterine pain was established. After taking two doses, the same distress was produced by it as described in her letters, without any effect upon the womb itself. An examination, per vaginam, the same evening, indicated no action on the os uteri; neither was there any tenderness of the parts. The ergot was, therefore, discontinued. On the 23d, I examined, per

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vaginam, again. There was no heat or soreness of the parts, and no prospect of overcoming the contracted condition of the os tincæ. I now informed the patient that, in consequence of the unpleasant symptoms produced by ergot, particularly as it was unaccompanied by its specific action upon the uterus, I could not depend upon its influence; and therefore it was doubtful whether the removal of the tumour could be accomplished. This information had a very depressing effect upon her, as it seemed to destroy all hope of getting rid of her disease. Next morning early I was sent for; she had had in the night a violent chill, which, lasting for about an hour, was followed by considerable reaction. This was soon succeeded by the development of erysipelatous inflammation along the track of the abdominal cicatrix, which spread rapidly in every direction, invading not only the skin, but also the serous and mucous tissues, as indicated by diarrhœa and vomiting, and an aphthous and inflamed condition of the lining membrane of the mouth and throat. The erysipelas continued, unchecked, with great violence, until the powers of life gave way, and the patient sunk under it on the morning of November 3.

On opening the cavity of the abdomen, several hours after death, it was found that the old cicatrix was strongly adherent, almost its whole extent, to the anterior face of the uterus; that a considerable amount of purulent serum had collected within the cavity of the peritoneum, and that loose adhesions existed between several points of this membrane, indicating *recent* inflammation. The tumour itself had very much the same appearance as at the time of the operation of gastrotomy, with the exception of the cystiform bodies, which had disappeared from its surface, save the one at the fundus, which had not altered in size, though it was more solid. The ovaries had not changed in appearance. The right one burst in attempting to remove the tumour, and a greenish-yellow fluid escaped. The firmness of the tumour had very much diminished; it was much softer than before.

The mass, with the attached cicatrix and ovaries, was removed and examined. It was laid open, on its posterior face, from the os tincæ to the fundus, so as nearly to bisect it. This disclosed a fibrous tumour in the interior of the uterus, sealed firmly to it at every point, except a space about three-quarters of an inch in diameter, communicating with the mouth of the womb. Upon passing a sound into this space, it went directly up to the right Fallopian tube, and no farther. This track was lined with mucous membrane, was about three-quarters of

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an inch wide, about two inches long, and was the only part of the interior of the womb supplied with mucous membrane; every other point, even around the left Fallopian tube, was closely adherent to the exterior of the tumour. The canal, through the cervix, from the vagina to the interior of the uterus, where it came against the tumour, was about an inch or an inch and a half in length, and was lined with healthy membrane, the notches made by the bistoury not being perceptible. The tumour itself had evidently degenerated. Large masses of it had softened down into a brain-like substance, while other parts retained the fibrous character.

Remarks.—The autopsy satisfactorily explained some points of particular interest in this case. While making the examination to decide upon the propriety of gastrotomy, the sound, taking the direction towards the right side, passed into the uterus only two and a half inches, which is the normal depth of the cavity. It must, therefore, have followed the track to the right Fallopian tube, which was in reality the only *cavity* in the uterus, and, as the development of the tumour and uterus took place principally above this point, and as every other part of their surfaces was adherent, the want of correspondence between the size of the uterus and the depth of its cavity is thus explained. The sound being one of the means of diagnosis in abdominal tumours, this case shows that, although it may enter no farther than the normal distance, this circumstance alone is not a correct indication of the condition of the uterus.

Again, after the patient had recovered from the operation of gastrotomy, the sound was employed a second time. It again passed without impediment to the distance of two and a half inches, and, upon urging it onwards gently, it appeared to overcome weak adhesions between the tumour and uterus, so that the sound finally entered a considerable distance. The sensation, communicated by the sound, of lacerating adhesions, no doubt was caused by the point of the instrument penetrating into the substance itself of the degenerated tissue of the tumour. This was confirmed by the fact that, in plunging the sound into the tumour, after its removal, precisely the same sensation was produced.

There is another interesting fact connected with the attack of erysipelas in this case. On the 15th of October I operated on an elderly lady from Chester, Delaware County, for fissure of the anus. She was constitutionally liable to erysipelas, and the operation was followed by a severe attack of it, but from which she recovered. On

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the 20th of October I operated on a middle-aged lady, from Chester County, for the removal of a large steatomatous tumour in the axilla. This was also followed by erysipelas, a first attack, and from which she also recovered. On the 22d of October I operated on the case here reported. Dr. Grant, who assisted me in the operation on the Delaware County patient, informed me that two of his patients were also seized with erysipelas the same week.

CASE V.—*Miss M. B., aged 36 years; tumour intra-uterine, and distended the uterus to the size of full pregnancy; os tincæ closed; cervix entire and dense; orifice very small; the whole tumour removed; supposed weight eight or nine pounds; recovered.*

Miss M. B., aged 36 years, of Lancaster County, Penn., consulted me on the 13th of November, 1849, for a large tumour in the abdomen. As she stood dressed she appeared as large as a female at the full period of gestation. The tumour was prominent, hard, unyielding, and extended upwards nearly three inches above the umbilicus and laterally to both the iliac bones. It was very slightly movable.

Examining per vaginam, I found the os tincæ closed, and thrown towards the left sacro-iliac junction. The cervix was of the usual size and regular in shape, but dense. The brim of the pelvis was occupied by the lower part of a resisting mass, as hard as that occupying the abdomen.

I had the following history from herself: She began to menstruate in her 16th year, and did so regularly and without pain up to the year 1841. In her 17th year she had two attacks of fever, and when 20 years old she suffered from cough, which continued two years, and which was said to be consumption. In 1841, while standing on a rocking-chair and reaching upward, she fell, and thinks she struck the lower part of the abdomen against the chair. She was so much hurt as to keep her bed for two weeks, during which time a lump formed in the vagina, which was opened by her physician, but nothing beside clotted blood discharged. From that period she had never been well, menstruation was always painful, menorrhagic in character and more frequent than before, and the catheter was demanded frequently for retention of urine. Subsequently to this injury she had an attack of pleurisy. The pain of menstruation now became almost insufferable, and continued during the whole menstrual period, and for about three days following, accompanied with swelling over the pubis. To relieve this intense suffering large doses of opium had been taken. She first noticed the tumour the last of December,

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1846. It was then as large as a hen's egg, hard, and immediately above the symphysis pubis. On the 8th of June, 1847, she was seized with very severe pain in the tumour, which had greatly increased in size. For three weeks she was exceedingly ill, suffered severely night and day, and the urine, after having been retained for thirty hours, had to be drawn off with the catheter. This attack was followed almost immediately by another equally severe, lasting seven weeks, and accompanied with unceasing and extreme pain. On the 8th of August a consultation was called, and blisters were ordered to be applied, every other day, over the tumour. This treatment was continued until twenty blisters had been used, which relieved the pain but partially. During this period, for seven weeks, she could not turn herself from one side to the other. Four weeks afterward she had an affection of the bowels, which was accompanied with a discharge of blood and matter resembling the scrapings of entrails. The discharges were very frequent, small, slimy, and bloody, and there was great tenesmus. After this there seemed to be a slight diminution of the tumour. She kept her bed until the 4th of September, after which she was better, and able to be about until May, 1848, except two weeks in the month of February. The tumour now did not appear to increase in size, and she remained free from severe pain until June, 1849, when she had another very severe attack, and swelled enormously. Her physician, Dr. Isaac Winters, and his son, Dr. Leamon Winters, both attended her until the 18th of August. She was so ill at this time that both remained with her one night, administering morphia every hour, and to such extent as to induce an attack of delirium tremens. Still, the suffering was not relieved. Her brother-in-law, who resided in Philadelphia, insisted upon bringing her here, to place her under the care of Dr. F. A. Martin, his own family physician. She was accordingly brought on the 18th of August, and Dr. Martin commenced attendance on the 20th, and continued his care until November; she believes that the pain was somewhat relieved, but the tumour continued to increase. During the attendance of Dr. M. she had five attacks of extreme pain, which lasted, at each several period, three or four days, and always came on with greatest severity after each menstrual flow had ceased, although, during the whole period of their flow, she suffered very much. Menstruation also was copious, lasting about eight days.

The above history was fully corroborated by Dr. Isaac Winters, of Hikletown, Lancaster County, in a letter dated March 13, 1850. The

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patient had been seen in consultation with him by Dr. F. A. Muhlenburg of Lancaster.

After examination, I gave the opinion of a probability of being able to remove the tumour; the patient decided to go home for a short time, and return again to Philadelphia, to place herself under my care.

December 13, 1849. The patient returned to Philadelphia. I visited her on the 15th and made a second examination. I discovered nothing differing from what the first observation exhibited. I now, however, employed the uterine sound. It passed through the os and cervix uteri with considerable difficulty and pain; and passed fully two inches in before it appeared to get beyond the fleshy depth of the cervix. It entered the interior of the uterus towards the left side to the distance of seven inches. It could not be moved round the interior; and, on every movement of the sound, the impulse seemed to be communicated to the tumour, as felt through the walls of the abdomen.

After the examination, I decided that there existed within the cavity of the uterus, a fibrous tumour which was developing itself in the fundus, and not distending the cervix, and determined, that if the specific action of ergot could be procured, I would make a section of the cervix, and cause the tumour to be expelled from the interior. Accordingly, I prescribed 10 grs. of ergot, to be taken every four hours, until uterine pains should be established. This was continued until the 19th, with the production of some uterine efforts, and a slight impression on the 19th upon the upper part of the cervix, as if it were about distending. Until this time, although an examination was made daily, no change could be recognized.

20*th*. To-day I introduced the Ricord Speculum, and by means of a long bistoury, passed through the speculum, I made several slight nicks into the os tincæ, which was followed by very little bleeding. Ergot was continued without interruption. On the 21st instant, I thought the cervix was still yielding, and on the 22d, I made several more nicks into the os, extending them up through the cervix.

27*th*. Ergot was still continued as first prescribed up to this date, producing a slight but by no means decided action of the uterus. To-day I cut the cervix much more extensively, and again on the 28th repeated the sections, very little bleeding having followed the operations. Ergot was still administered until the 31st, when I severed the cervix still more, and afterwards, in consequence of the ergot having apparently lost its effect, I ordered ten grs. every two hours. No

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impression as yet seemed to have been made upon the tumour, and very little upon the cervix.

January, 1850. I renewed the operations on the 5th, 7th, and 8th, respectively, making much more free sections of the os and cervix. I was now able to introduce the whole length of my index-finger through the cervix, and was just able to strike against the lower portion of the tumour with the extreme point of the finger. This dilatation of the cervix seemed to have been accomplished solely by the knife, without any aid from the ergot, which, up to the 8th, was continued every two hours.

I now for the first time suspected that the ergot which had been used was of bad quality, and having procured some of a recent crop, I ordered 10 grs. to be given every half hour for five hours in succession every morning, and then be suspended. This treatment was to commence on the 9th. I soon found that the specific action of the ergot was energetically established, and by the 12th, the patient was thrown into a state of complete ergotism, accompanied with great distress in the head. I again incised the cervix on the 12th, and also on the 13th, although the use of ergot had been suspended. To-day, the 13th, I found the cervix and os uteri yielding so much, that I could pass two fingers into the uterus, and I was able to reach the tumour at one inch distance, although it first required a finger's length. The ergot now produced dreadful pains, which continued without intermission. Severe as her sufferings were, the patient assured me they were nothing to compare with her previous agony.

In passing the knife into the uterus to-day (the 13th), I severed several of the attachments of the tumour, besides cutting into its substance. This was followed by very little hemorrhage. The substance of the tumour was so resisting that the knife grated through the tissue so loudly, that the nurse heard it. After the ergotism had subsided, I recommenced the same prescription of the ergot, less frequently, but often enough to keep the uterus in contractile action. On the 17th, I operated again and for the last time, destroying more of the attachments between the tumour and the uterus, and also disintegrating to a considerable extent the tumour itself. Ergot was continued occasionally after this until the 30th of January, and during this period the cervix gradually distended more and more, and the tumour sunk lower in the abdomen, so that, on the 30th, its lower part could be distinctly recognized through the os tincæ, undergoing a process of softening and disintegration. A discharge now began to flow from the vagina, and in a few days it amounted to an immense

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quantity, and very offensive. This was accompanied with irritative fever, and considerable oppression over the chest, with some difficulty of respiration. These symptoms, however, were soon relieved by a blister over the breast, on the 5th of February. By the 9th of February the tumour had sunk to a point two inches below the umbilicus, was much narrower and less prominent, had contracted in all its dimensions, was less resisting, could be felt through the os tincæ rapidly softening, and the fingers, after the examination, were tainted with a tenacious, offensive, putrid odour. Large quantities of ill-conditioned offensive matter were still discharged from the vagina. From this time the tumour rapidly diminished.

February 15. A copious watery discharge flowed all the day, irritating the vulva and thighs. The thin portion of the discharge soaked through the clothes, while a more dense matter was deposited upon them, containing flakes resembling pieces of wet tow. The tumour seemed to descend into the vagina and press against the neck of the bladder, so as to cause retention of urine for sixteen hours, after which the urine passed, and was again retained for sixteen hours. On the 17th, I was sent for to relieve the bladder. The patient was also greatly excited in consequence of the escape of a portion of the tumour, still attached, which she supposed was the womb coming out. I now removed a large portion of the tumour, which which was occupying the vagina, and which had lost its vitality, and resembled, as before, a mass of wet tow. On the 18th, I removed another mass of the tumour of the same character, and on the 19th, I surveyed the whole interior of the uterus with Bond's placental forceps, and could detect nothing remaining. The abdominal tumour had now entirely disappeared, and the fundus of the uterus could scarcely be felt by dipping deeply into the basin of the pelvis, over the top of the pubis.

The patient recovered rapidly, and returned home in about two weeks. I have seen her since, and heard of her frequently. She has regained her health perfectly, and engages in all kinds of household labour.

Remarks.—Ergot was continued in this case almost uninterruptedly for a period of seven or eight weeks, without producing any unpleasant effects upon the general health. It is not improbable that the continued tension of the muscular fibres of the uterus, induced by the ergot, may have had considerable agency in destroying the vitality of the tumour, as I can readily conceive that the circulation

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of an intra-uterine tumour, situated as this was, may be entirely suspended by maintaining an active and uninterrupted contraction of the uterus. It is in this way that ergot, when incautiously used during parturition, particularly in first labours, often destroys the life of the child.

Ergot, in this case, acted with great power upon a uterus not under parturient excitement. The resistance of the cervix was overcome by the free use of the knife, and, so soon as the os tincæ was expanded, the ergot caused the uterus to contract and expel its contents. This, therefore, was a species of labour artificially induced.

This patient was in the city quite recently, called to see me, and is in the enjoyment of excellent health. The functions of the uterus are perfect. This is the more remarkable, as her general health had been so much impaired as to indicate decidedly a malignant tendency.

CASE VI.—*Mrs. S. B. K., aged forty-two years, tumour intramural; was developed in the posterior wall of the cervix, expanding it into the form of a cyst; occupied the abdomen to the height of the umbilicus; patient bloodless from repeated floodings, and her life in imminent hazard from present hemorrhage; bleeding ceased immediately on operating; removed the whole mass, weighing nine or ten pounds, at once through the os externum. Death from anæmia.*

Mrs. S. B. K. consulted me May 29, 1850. She was of rather a full habit, and disposed to corpulency, but quite anæmic from great loss of blood. There was also a peculiar straw-coloured tinge upon the skin. A tumour could be felt in the abdomen, occupying the central part, extending up to the umbilicus, and could be traced dipping deeply into the pelvis. It also extended laterally, on both sides of the linea alba, to points midway between the mesian line and spinous processes of the ilii. It was oblong in shape, hard and smooth, and pretty firmly fixed in its position. At its upper part, and immediately to the left of the umbilicus, was a small but much more prominent tumour, which seemed to be superimposed upon the deeply seated mass. This portion was softer, semi-elastic and smooth—movable upon the large tumour, and attached to it. The patient did not complain of any soreness in the examination.

Per vaginam, the pelvis was found to be occupied by a large mass, which somewhat resembled to the touch the under service of a placenta when in the grasp of the os tincæ, but firmer. It evidently was covered by a smooth mucous surface, beneath which slight indenta-

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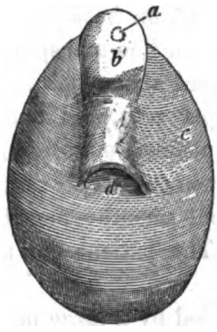
tions or sulci could be felt. The pelvic portion of the tumour did not occupy the recto-vaginal space, but was within the vagina. The vagina could be traced running up behind and on the sides of the tumour until the index-finger rode in a sulcus at its termination. The pelvic tumour seemed to consist of an expansion of the posterior part of the cervix uteri, and of the os tincæ. The tumour did not project from the cavity of the uterus, as no fold of the posterior lip could be felt grasping it; on the contrary, this lip of the os tincæ was lost in the tumour, and appeared to be an expanded covering for it. In front and far above the pubis and anterior to it, the index-finger could detect the front lip of the os tincæ stretched across the anterior face of the tumour, resembling a bow, its ends or commissures being gradually thinned down until they were lost in the expanded posterior lip. In consequence of its great height, it was very difficult to insert the point of the finger into the os tincæ. The sound entered the uterus to the distance of eight inches, keeping in front of the tumour, and its point could be distinctly felt at the umbilicus.

This examination was accompanied with but slight pain and bleeding, and was followed by no unpleasant symptoms.

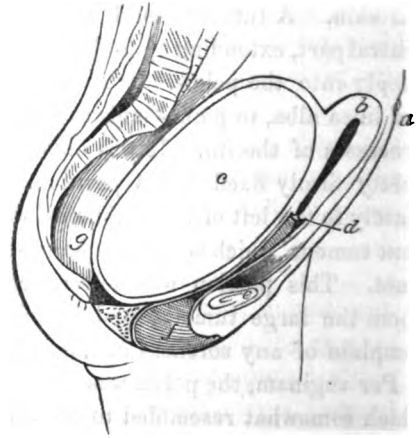
The diagnosis was, a fibrous tumour of the uterus, not in the cavity, but located in the posterior wall, expanding that wall into an envelop, and elongating the uterine cavity, and stretching laterally the anterior lip of the os tincæ. The following drawings, figures 3 and 4, represent the relative position of the parts.

Fig. 3.



a. Umbilicus. *b*. Fundus and body of Uterus. *c*. Tumour. *d*. Os Tincæ.

Fig. 4.



a. Umbilicus. *b*. Fundus and body of uterus. *c*. Tumour. *d*. Os Tincæ. *e*. Pubis. *f*. Vagina. *g*. Rectum.

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The patient gave me her history as follows: She was 42 years of age. Menstruation commenced at 12 years, at which time she was large and fat. She continued regular to the period of marriage. At 14 years of age she had intermittent fever for five months. Never had had any other disease, and has no hereditary disposition thereto. She was 24 years of age at marriage, and has had three children, the youngest being 8 years old at the time she came under my notice. She did not nurse her last child, in consequence of sore nipples, and six weeks after its birth she had abscesses in both breasts. After this parturition she again menstruated and remained regular until March, 1848. About ten days after a regular period she took a long walk to church, on Sunday morning, at the same time feeling uneasy in her back. On her return home she was taken with a slight bloody discharge from the uterus. This returned in the evening, and again next morning, increasing each time; and, while seated at dinner-table on Monday, a sudden copious gush took place, and again a larger quantity the same evening. The patient thinks the amount discharged at noon amounted to about one gallon. The hemorrhage continued badly until the succeeding Sunday, when she swooned and remained as dead for three hours. Reaction was not accompanied with consciousness, and she lay for about a week senseless. After a confinement of five weeks she gradually recovered. The menses again returned regularly, but were more and more copious until the first of July, 1849, when another flooding occurred, while in the act of rising in the morning. This soon ceased, after which she crossed the Delaware from Camden to the city. Very soon after landing, flooding again came on, and large masses of coagula were discharged. This confined her one week and prostrated her greatly. A pain in the back, lasting one day, always preceded these attacks. In August she menstruated again, and the first week in September she again flooded badly for a week. Menstruation was again regular until the middle of December, when hemorrhage came on again, but not so badly, lasting three days, and followed by a discharge sometimes red, at others white, and again watery. This discharge continued for fifteen weeks. During this period she had repeated hemorrhages at intervals of ten, fourteen, or twenty days, lasting for two or three days at a time. Her last attack was very sudden and copious. Previous to the floodings she became large and heavy, and had difficulty in passing urine. She was almost bloodless, skin œdematous or doughy, pulse 88, and exceedingly feeble.

Every means having been used to arrest hemorrhage without avail,

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and the patient rapidly sinking, the urgency of the symptoms called for prompt action. The patient herself was likewise very solicitous that an effort should be made to save her. I proposed the operation which I will presently give in detail. To prepare the uterus for the operation, and also with a view to arrest the sanguineous flow, I ordered an infusion of ergot, and about seven grains to be taken three times a day. Next day, the 30th, a slight uneasiness in the back was induced. The medicine was continued up to the 31st, when it was administered every four hours.

June 1.—About daylight the patient was taken with pretty severe pains in the back, which assumed the character of periodical uterine pain. As the ergot was now acting efficiently without arresting the discharge, and, as the vital powers were still giving way in spite of sustaining treatment, I took Prof. Grant with me at 8½ P. M., intending to operate should he unite in approving the measure.

After a careful examination, Dr. Grant coincided with me in regard to the urgency, the propriety, and plan of the operation. The patient having been placed upon the edge of the bed, I introduced a long bistoury into the cavity of the uterus, and turning its edge backward upon the tumour, cut through the posterior wall of the cervix and posterior lip of the os tincæ, down through the corresponding portion of the vagina, all of which formed the antero-inferior covering of the tumour. In doing this, I sunk the knife into the substance of the tumour, which grated beneath it. Afterwards, with the point of the index-finger engaged in the incision, I endeavoured to enucleate the lower or pelvic portion of the tumour, which I easily accomplished by breaking up the attachments between it and the coating first incised. The portion of the tumour thus laid bare, or uncoated, equalled the size of a large orange, and was quite smooth, as if covered with a smooth membrane. Here and there, were strong bands, sealing the envelop to the tumour; but most of them I separated with my finger, and others with the knife. Some of the strongest in front and to the right side I allowed to remain for a subsequent operation. Very little blood was lost; *indeed, from the moment the cut was made*, at which time there was a slight gush of *venous* blood, *not a drop more was discharged.*

Immediately before the operation I gave the patient a double dose of ergot, viz. fourteen grains, which brought on active labour-pains; afterwards, I introduced a sponge tampon, to provide against hemorrhage. Another dose of ergot was ordered at 7 o'clock P. M.

At 8½ P. M. I was sent for in consequence of the severe labour-

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pain which had continued from the time of the operation, and which had increased after the last dose of ergot to such a degree that, fifteen minutes afterwards, a teaspoonful of elixir of opium was administered to counteract its effects. I removed the tampon, found that there had been no bleeding, and that the mass had descended more into the pelvis. I separated additional portions of the tumour with the finger; but the bands, before referred to, were too strong to be broken up. Before leaving the patient, I gave another spoonful of elixir of opium, and ordered ergot to be suspended until morning.

2*d*. The patient passed rather an uncomfortable night; was free from pain, but laboured under the unpleasant effects of the anodyne. She was much debilitated; there was no bleeding, but, in the day, there was a whitish discharge from the vagina. Some more adhesions were broken up with the finger. She passed the day pretty comfortably.

3*d*. 3½ P. M.—Dr. Grant visited the patient with me. Her stomach had been very irritable, rejecting everything. Ergot had been given every two hours since five o'clock in the morning, until four doses had been taken; but this, and everything else, were rejected. She complained of pain in the back, and had difficulty in passing urine. Upon external examination of the abdomen, the tumour was found to have diminished very much; and, upon examination per vaginam, it was found to be so low down as to press upon the internal faces of the vulva. The finger passed readily around the tumour, between it and its envelop; but there were several strong adhesions in front, which I severed with Cooper's bistoury, and then broke up several others with the point of the finger.

There had been a very copious discharge of watery fluid from the vagina all day, and throwing off an incipient putrescent odour. The tumour also felt as if it had begun to degenerate.

10 P. M.—The patient had a chill at about 7 P. M., which was followed by fever. Respiration was slightly laboured, countenance good, complained of her back, and the discharge became more fetid.

4*th*. 9 A. M.—The patient passed rather an uncomfortable night; respiration laboured, and complained of soreness. Introduced catheter, and drew away about one gill of urine. The tumour filled up the pelvis.

4 P. M.—Saw the patient in company with Dr. Grant. It being feared that the chill, fever, and oppressed breathing arose from the irritation of the putrefying mass in the pelvis and abdomen, it was decided to remove as much of the tumour as possible. The patient

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lay upon her left side, with her hips at the edge of the bed, and her knees drawn up. Upon separating the vulva with the fingers, the tumour could be seen. It was much degenerated, and extremely offensive. Seizing the tumour by the two index-fingers, passed high up on the opposite sides, I drew upon it; and, aided by the crotchet, inserted at different points, I succeeded in bringing out the lower portion. Several bands on the anterior face, which could not be broken with the finger, were now severed with the bistoury; after which, the left hand was insinuated into the vagina, along the side of the tumour, and also into the cyst, and employed in separating the attachments. With the points of the fingers I was able to scoop off the tumour, or enucleate it from the walls of the cyst, aided by a crotchet in the other hand making the parts tense. Occasionally, I met bands so strong that I had to divide them with the bistoury. The manipulations with the hand were very similar to the operation of detaching an adherent placenta. Persevering in this way, I succeeded in turning out of the cyst, and also out of the vagina, the whole mass of the tumour. There was no distinct pedicle; but the tumour was attached to the expanded wall of the uterus, like the orange to its skin, and in like manner separated. The whole operation lasted about half an hour.

Previously to the operation, the abdomen was a good deal distended, particularly in the region of the tumour; percussion returned a resonant sound, indicating a collection of air within the interstices of the tumour, the evolution of the gas having occurred in consequence of putrefaction in the dead mass. This was made evident by the operation; for, so soon as I began to manipulate the tumour, gas escaped, filling the room with offensive odour, and the size of the abdomen as rapidly diminished. The tumour itself, after removal, still contained gas within its substance. Not a particle of blood escaped, nor indeed, has any since the first operation. The whole uterus descended into the pelvis, so that nothing of it could be felt above the pubis. The tumour filled a large wash-basin, and, in its degenerated state, weighed five pounds. Its estimated weight, before decay, was eight or ten pounds.

10 P. M.—The removal of the tumour did not appear to afford relief to the symptoms. Respiration became more and more laboured —the pulse was small, though not frequent; there were considerable restlessness and jactitation, and the patient was evidently sinking. Anodynes and stimulants did not relieve her, and she died at two o'clock A. M. of the 5th.

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5th. 8 P. M.—The body was examined by Dr. Grant. The chest and abdomen were opened, but there were no marks of disease present. Everything looked pale and white, perfectly blanched. Several ounces of yellowish serum were in the cavity of the peritoneum, and a considerable quantity of bloody serum was in the right pleura, less in the left, and some in the pericardium. The omentum was spread beautifully over the intestines, and as pale as the rest of the body. There was merely a perceptible pinkish hue upon the surface of some of the intestines. The uterine organs were entirely within the pelvis; the recto-vaginal cul-de-sac of the peritoneum was very deep. There seemed to be a partially abraded spot on the peritoneum, covering the promontory of the sacrum, and a corresponding one on the posterior surface of the cyst which covered the tumour. The uterus, rectum, bladder, and vagina were removed in a mass, and examined.

The parts removed, proved the diagnosis to be accurately correct. The posterior part of the cervix uteri had been the seat of the tumour; which, originating in its substance, had distended it into a complete cyst and filled the pelvis, projecting into the abdomen as far as the umbilicus, while the body of the uterus rested upon its fundus. The whole tumour had been enucleated; and the cyst on its inside was lined by a shreddy substance, showing that it had been everywhere adherent to the tumour.

The opinion of the medical gentlemen present, as expressed by a certificate, was—that the patient died from *anæmia.*

Remarks.—This operation, although it did not succeed in preventing a fatal result, I consider one of the most successful in its performance I have ever accomplished. The entire tumour, *en masse*, was turned out from its cyst in the posterior wall of the uterus, and delivered through the vagina without causing the least injury to the neighbouring viscera, as was subsequently proved by the *post-mortem* examination.

The important fact that hemorrhage, arising in consequence of the presence of a fibrous tumour, can always be arrested by cutting through the coating of the tumour, was most clearly illustrated in this case. The patient was perfectly anæmic from the direct loss of blood, which still continued to flow, but ceased immediately after a section of the tumour was made. Unfortunately, however, sanguification could not be established after the operation, in time to prevent the fatal prostration induced by the loss of blood previous to the incision.

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An earlier resort to surgical treatment most probably would have saved the patient. She had been under the care of a homœopath up to the period of the operation, and how far the do-nothing-treatment of this practice aided in the result, it is impossible to say. What means homœopathy possesses of controlling uterine hemorrhage, I do not know, but it appears to me that scientific medicine would have afforded more efficient aid, and prevented such a complete exhaustion of the vascular system as was exhibited here.

CASE VII.—*Miss A. B., aged 49 years; tumour intra-uterine and sealed to the interior surface of the uterus; extended to within one inch of the umbilicus; the cervix was lost in the tumour, and the os was firm and ring-like; removed one-third of the tumour; died suddenly from disease of the heart.*

October 1, 1850, I was requested to see Miss A. B., aged 49 years. She commenced menstrual life at 15 years of age, and had always been regular, except during the last year, when she went three times beyond the month. Menstruation had never been painful, nor the discharge clotted, but had always been profuse, and during the last few years had amounted to flooding. She was disposed to corpulency, and had an anæmic appearance.

The abdomen was not very prominent, but covered with a thick deposit of adipose tissue. A tumour was felt through this, deeply seated, extending up within an inch of the umbilicus, outwards, to within an inch of the left superior spinous process, and across the linea alba towards the right. It was hard, resisting, and oval-shaped, nearly as large as a child's head, but slightly movable, and free from pain on handling. On examining per vaginam, the os tincæ could be felt on the left side of the pelvis, but so high as to be difficult to reach. The tumour occupied the superior strait, and the cervix uteri seemed lost in the tumour, while the os tincæ was sufficiently open to admit the point of the index-finger, the lip being slightly thickened. The sound entered 6½ inches, and all movements of the tumour were readily communicated to the sound. No bleeding or pain accompanied or followed the examination.

The diagnosis was intra-uterine tumour of a fibrous character.

2*d*. I ordered the patient to take seven grains of ergot three times a day, which was continued at that rate until the 4th, when it was ordered six times a day. Two days after, she took ergot eight times a day, but no evidence of uterine contraction followed; no pain, fulness, nor tightness in the lower part of the abdomen.

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On the 7*th* I examined her again, and, although the ergot had failed to produce pain, yet the os tincæ was more open, and much lower in the vagina, so that it could be reached more readily by the finger. The point of the finger could now touch the lower end of the tumour, which seemed to occupy the posterior wall. Ergot was continued as before.

On the 10*th*, I discovered that the os tincæ was still becoming more and more relaxed and depressed. On the 12*th*, the ergot producing nausea, I suspended its use until the morning of the 14*th*, when I ordered her to take ten grains every half hour from 10 A. M. until 3 P. M., the time appointed for the first operation.

14*th*. I passed the long bistoury into the interior of the uterus, and nicked the os and cervix, so as to be able to pass my finger more readily into the uterus. Next day I administered ergot in the same way, and repeated the operation, opening the os tincæ to a greater extent.

16*th*. I ordered ergot to be taken three times a day, and the following day I operated again, cutting into the substance of the tumour. On the 19*th* I intended again to operate, but was prevented by the supervention of menstruation, although ergot had been taken every half hour. The moderate use of ergot was continued. The vaginal discharges had been increased since ergot was given. In the above operations there was very little pain, and but few drops of blood appeared.

24*th*. Operated again, and cut away the attachments between the tumour and the right side of the cervix, and also incised the lower end of the tumour in several places. 26*th* and 28*th*, again operated, dividing several adhesions, and cutting deeply into the tumour. A tumour appeared to occupy the right side of the pelvis, apparently distinct from the other. The size of the first tumour diminished, and its fundus fell nearly two inches below the umbilicus. The os tincæ was much wider open, and the discharge became more copious and offensive.

30*th*. Passed the sound into the uterus 5½ inches. The tumour in the right side of the pelvis was now discovered to be but a part of the first, and could be partially enucleated by the point of the finger, curved over the edge of the os tincæ, towards the right side. I incised the tumour again. November 1—operated again, and enucleated more of the tumour. The discharge of course increased after every operation.

November 4. Perceived a putrid smell as soon as I entered the

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room, which indicates softening and degeneration of the tumour. The watery discharge had greatly increased. Divided several strong bands on the left of the cervix, and enucleated more of the tumour with my index-finger.

6*th*, 8*th*, 11*th*, and 14*th*, performed repeated operations, using the crotchet, to aid in disintegrating the tumour. On the 18*th* removed a portion of the tumour. 20*th*. Suspended the use of ergot. Discharge and fetor increased, and tumour rapidly diminished. 24*th*. Slight fever, and menstruation. Ordered Seidlitz powder and spirits of nitre. 25*th*. Tumour diminished to one-third its original size.

28*th*. Operated without administering ergot—the operation consisting in nicking the internal os at several points. The operations were borne with but little manifestation of suffering. After using the knife, I was about introducing the crotchet, when the patient, in a strong voice, inquired: "What is thee going to do now, Doctor?" and before I commenced operating with the crotchet she called to a female friend to "bring the cologne, that she was fainting." As the feeling of syncope was not an unfrequent occurrence with her, I thought nothing of the request; but her limbs becoming suddenly relaxed, I looked up, and was much alarmed to find her eyes turned up, and a death-like expression of the countenance. The whole body at once collapsed. Her legs, which were supported on two chairs, fell aside; her arms were flaccid, her face turned blue, respiration had entirely ceased, and there was no pulsation at the wrist or heart. She was lying on her back, and I quickly turned her upon her side, dashed her with cold water, applied stimulants to the nostrils, but all to no purpose. So rapid and complete was this collapse, and so perfect was the cessation of all vital manifestation, that, before the expiration of one minute, I imagined her dead. I, however, called for a bucket of hot water, into which to plunge the feet, and for bellows for the purpose of creating artificial respiration, and continued the dashings of cold water on her face and breast. While arranging the nozzle of the bellows to the nostril there was a very slight gasp. As soon as this occurred, I blew strongly with the bellows upon the face and chest, and it was succeeded, in about fifteen seconds, by another gasp, and then by another and another, until a faint pulsation could be perceived first at the heart, and afterwards at the wrist. The respiration and circulation gradually improved, and, after they were pretty well established, we placed her comfortably in bed. Immediately after the return of breathing there was considerable frothing

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at the mouth. I now, with great difficulty, succeeded in getting down some Hoffman's anodyne, and covered her stomach and extremities with sinapisms. This was about 3½ P. M. In about an hour, or an hour and a half, she began to move herself, but previously to this not a muscle moved, except the respiratory muscles. All was languid, and flaccid. Now, however, she began to move more freely, soon becoming exceedingly restless, moving from one side of the bed to the other, but still continued unconscious, and remained more or less so, until 3 A. M. of the 29th, when she took some drink, and replied intelligently to my questions. She smiled; and, in reply to a question, said she had no pain anywhere, and felt comfortable. Her pulse at this time was pretty full, the skin and extremities were warm, and there was febrile excitement. There was also a peculiar contraction of the brows, but otherwise no alteration in her countenance.

29*th*. Had a large natural evacuation from the bowels—getting out of bed herself, and seemed quite strong—also passed urine. The febrile excitement lasted all day. Pulse in the evening 120–125. Had two more smaller fecal evacuations at evening. She could be easily roused, spoke and drank, and was much less restless than the previous night.

30*th*. Was very quiet through the day, very difficult to rouse, and after midday this was impossible. The pulse became more and more frequent, respiration shorter and more hurried, and, in the evening, distension of the abdomen commenced and rapidly increased. She died at 8½ P. M.

Twenty-four hours after death, Prof. Grant made an examination of the body. The abdomen was opened along the linea alba, the incision passing through a layer of subcutaneous fat, about one and a half inches thick. There was also a layer of fat beneath the muscles, between them and the fascia transversalis. The whole cavity of the abdomen, all the viscera, and peritoneum, were in the most remarkable state of health—not the least vestige of inflammation or disease. The lower portion of the cavity was occupied by the uterine tumour, which was quite irregular, and nodulated, having a projection passing over towards the right side. The peritoneal surface of the tumour was perfectly healthy in appearance. The uterine mass was removed from the cavity and examined. The os and cervix were quite healthy. The finger was passed into the interior, and the tumour was found broken up. There being no cause

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of death within the abdomen or pelvis, the chest was opened. The left lung was quite healthy; no pleural adhesions; the lower lobe was filled with dark-coloured blood, but pressure showed healthy crepitation. The right lung was adherent from old disease, but healthy. The cavity of the pleura contained two or three ounces of bloody serum. The pericardium was opened, and about half a pint of serum, highly coloured with blood, was found in its cavity. The heart was exceedingly soft and flabby, was flat, falling together like a wet rag. Its walls were thin, dilated, and fragile, tearing very readily. The ventricles, particularly the right one, had degenerated into fat. Indeed, the heart, as it lay in the pericardium, resembled a mass of fat. Within the left ventricle there was a large mass of white fibrin, or "heart-clot," about two or three inches long and one or two inches thick. In the right ventricle was another, about the same length, and half its width. These clots may have formed during the attack of profound syncope, and afterwards remained to obstruct the circulation. The coronary veins were in a high state of engorgement, large and full.

As Dr. Grant considered that the condition of the heart was amply sufficient to account for the symptoms and death, the dissection was not prosecuted farther. The certificate of death was written by Dr. Grant in the following words: "This certifies that A. B., aged 49 years, died last evening of disease of the heart (dilatation and fatty degeneration).

Remarks.—I congratulated myself that, in this case, no anæsthetic agent had been administered, as the symptoms would undoubtedly have been ascribed to its influence. But, fortunately for this valuable means of destroying sensibility, it was not employed, although the patient frequently desired it. The *post-mortem* examination shows that death could not be attributed to the operation. Previously to the last attempt upon the tumour, the patient's mind became considerably excited on a certain subject, and the mental agitation had not yet subsided. This, I think, had much to do in the production of these alarming symptoms. Had the syncope occurred in an upright position, so as to have deprived the brain more effectually of blood, death must have been instantaneous. The recumbent posture, however, secured to the encephalon sufficient vital stimulus to enable it to respond to the restorative agents employed, and thus enabled life to struggle on a few hours longer.

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CASE VIII.—*Miss H. B.*, 31 *years old; tumour intra-uterine, and sealed to the interior of the uterus; very prominent, and extended above the umbilicus; cervix entire and movable on the tumour; os tincæ closed; supposed weight seven or eight pounds. Recovered.*

October 16, 1850. I examined Miss H. B. for a very prominent abdominal tumour, and which I found occupying the lower part of the abdomen. It extended above the umbilicus, and resembled a pregnant uterus at six or seven months. It was very firm and unyielding, slightly movable, and had attached to its right side above, a movable tumour about as large as a walnut. In examining per vaginam, the hymen was found intact, the upper part of the pelvis filled with a hard tumour, and the os tincæ closed and thrown back. The cervix was of the usual size, and could be moved upon the tumour. The whole mass could be moved up and down by playing it between the hand on the abdomen and the finger in the vagina. The sound passed into the uterus seven inches, and its point could readily be felt through the walls of the abdomen high up on the right side, on a line with the umbilicus. There was very little pain, and no bleeding during this examination. Dr. Grant assisted me.

The first menstruation was at fifteen years of age; had always been regular, but not very free; without pain or clotting, and lasted about five days. It had been more copious since the appearance of the tumour, and slightly clotted. Occasionally, there had been slight leucorrhea. She first noticed the tumour, four years before consulting me, in the right iliac region. It was then about the size of a small orange, egg-shaped, hard, and not observed to be movable. She never had any pain in it, and it remained the same size until last spring. Last spring, she had an unusual weight and oppression in the lower part of the abdomen and pain in the back, which caused her to notice that the tumour had very much increased. Since then she was not sensible of its having gained in size. Sometimes there was a sharp stinging pain in it. Sometimes she had a desire to pass urine frequently, with occasional pain and difficulty. Habitual constipation, no difficulty in defecation. All other functions good. Pulse 76.

One year ago last winter she had an attack of erysipelas. She has frequently had symptoms of it since. With that exception she always enjoyed good health. She knew of no hereditary disease. Her brother, however, had tubercular phthisis.

Diagnosis.—Fibrous tumour within the cavity of the uterus.

November 5. The patient came to town to place herself under my

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surgical care. Ordered her to take seven grains of ergot three times a day. This was doubled on the 8*th*, and an examination per vaginam was made on the 11*th*. The ergot caused some pain in the right side, and some discharge. On the 16*th* she menstruated.

20*th*. Passed the long bistoury into the uterus and nicked the os and cervix, so as to allow the point of the index-finger to pass into the os tincæ. 22*d*. Operated again, and opened the os so as to admit the index-finger to the first joint. 24*th*. Again cut the os and cervix, and made a section into the tumour. 26*th*. Continued the sections, and with the point of the finger enucleated the whole lower part of the tumour. This was followed by an attack of quotidian intermittent, which was checked by quinia. 30*th*. Separated more of the tumour. Succeeded by a return of intermittent, which was again stopped by quinia.

December 7. Menstruation. 13*th*. Operated. The tumour was now one inch below the umbilicus and less prominent. 16*th*. Operated, and used a scoop, made for the purpose, to detach portions of the tumour from the walls of the uterus, and also a crotchet-like hook, with which I succeeded in destroying the right side of the tumour. I removed a portion of it, which was decidedly of a fibrous character. On the 19*th* I operated again—the tumour was much reduced in size, and there was a considerable fetid discharge. Ergot produced labour-pains. 24*th*. Broke up more of the tumour with the hook. 28*th*. Menstruation. 30*th*. Assisted by Dr. Grant, I operated again, incised the os tincæ more freely, and broke up the remainder of the tumour. The fetor of the discharge was excessive, and pervaded the whole house. This was the last operation required.

January 18, 1850. The tumour regularly diminished, and the fetid discharge continued. The patient rode out the first time. 31*st*. I examined the patient; the tumour had nearly disappeared from the abdomen.

February 3. With Bond's forceps I removed dead portions of tumour from the cavity of the uterus. This was followed by a diminished fetid discharge. Up to this period, and for a few days after, ergot was administered, and the offensive discharge continued. On the 13*th*, the patient returned home to Chester County. There being some soreness over the right iliac region, I ordered her to apply iodine to the skin.

18*th*. I received a letter from her brother, a physician, stating that his sister still continued to improve in strength since her arrival at home, although she had to keep the house three or four days after

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her arrival, owing to the inclement weather. He thought the soreness was abating, although very slowly. The discharge was fast abating. It was not more than one-third of the amount it was when I left her, but free from lumps or shreds of the tumour. He thought she was improving as fast as any person could wish.

March 8. The patient writes: "It is with pleasure that I inform you of the improvement of my health since you last heard from me, although the soreness in my side is still very troublesome. The abdomen still continues to swell [occasional flatulent distensions], and my back is very weak and at times painful, but withal my health and spirits are much better. There is still some discharge from the tumour, but not any shreds."

20*th*. The patient visited the city. I examined her in company with Dr. Harry of Chester County. There were still some hardness and soreness over the right iliac and pubic regions, but every other part of the abdomen was natural. The menses had been suspended since the 28th of December. Ordered the tincture of iodine and an infusion of polygala senega.

After spending several days in the city she returned home; but in being conveyed from the depot several miles across a rough country, she received several severe jars, which caused great pain. This was followed by a most violent attack, as indicated by the following letter from her brother. "March 30: Sister is suffering very much indeed from pain in her back and otherwise, that has been so far beyond our control. We are very anxious for you to come up in the first train this morning and see her, if it is in any way in your power; her sufferings are almost beyond endurance." I accordingly visited the patient, and found her suffering the most violent agony in the back, across the loins, accompanied with tympanitic distension of the abdomen. I ordered her to be freely cupped, and to take calomel and magnesia purgatives, and enemas.

31*st*. Her brother writes: "Hannah rested well last night without the aid of anodynes. Her medicine operated well, and reduced the size of her abdomen slightly, so that she does not complain of as much tenseness as she did yesterday. Her soreness still continues, but not quite as severe as it was. She still is unable to help herself to any degree. She has improved in every respect slightly, and I think, from present appearances, she will be able in a few days to leave her bed. She has not had any return of those pains since you left." A few days after, I received the following note from another brother: "I am requested by sister and Daniel [the Doctor] to in-

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form you that her health has very much improved since your visit last Sunday, although her back is yet very much affected. She has at times a beating in her back, resembling that in a gathering when ready for discharging. She has a reasonably good appetite. The swelling has very much subsided. Daniel has on one or two occasions taken some blood from her back by cupping."

May 31. The following is an extract from a letter by the patient herself: "After your kind but hasty visit, I passed four weeks of pretty severe suffering, but I have so far recovered that I have taken two short rides without much inconvenience, and on Sunday morning last I walked as far as brother's. This week I have had several slight attacks of difficulty of breathing, which have given me some uneasiness. The attacks were brought on by merely walking around the yard. The inflammation or soreness is not entirely removed from my back; it is still stiff and somewhat deformed. The swelling of the abdomen has entirely disappeared. The hardness appears to be decreasing and the discharge also."

November 28. The patient being in the city I examined her. There was no vestige of the tumour. Menstruation was regular. There was still a slight increase of the right side at every menstrual period. She was entirely well.

May 19, 1852. The patient called to see me. She was in the enjoyment of perfect health. Had become strong and hearty, and attended to the work of a farm-house. All the functions regular, and no sign of disease or tumour remained.

Remarks.—The most of this tumour disappeared in a very fetid discharge, very little of it having been removed in substance. After opening the os and cervix by the knife, and invading the tumour, the latter soon began to degenerate and diminish in size. The ergot manifested its specific action on the uterus before any attempt was made upon the os tincæ; and, after the os and cervix had yielded, its influence was decidedly increased.

Miss B. was in town a few days previously to this date [March 15, 1853], and called to see me. Her health is perfectly restored.

CASE IX.—*Mrs. E. B., 36 years old; tumour intra-mural; cervix uteri bent, against the tumour, at an acute angle; operative measures discontinued before the tumour entirely disappeared; recovered.*

November 16, 1850, I was requested by Dr. J. C. Lehman, of

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Philadelphia, to see Mrs. E. B., a patient under his care. She was a coloured woman, 36 years old. Menstruation commenced at the age of 16, and continued regularly afterward, usually lasting three or four days. It was never painful before marriage. She married at 17 years of age; after which, menstruation became very painful, more profuse, and lasted one week; so it continued for about four years, when she was relieved by medicine. The pains, at times, were equal to those of parturition. She had one child at the age of 18 years. There was no particular difficulty at the birth, except in the delivery of the placenta, which was adherent to the left side. It was three hours before it could be detached. The left side had remained sore from that time. During pregnancy, she had a slight show, regularly, every month. She soon became pregnant again, and had to wean the first child when six months old. When three months advanced she miscarried, and also twice afterward; the last illness was accompanied with great flooding. During these periods of pregnancy she had always a slight monthly show. Until she was twenty-four years old she was very thin, and her waist was extremely slender.

In 1838, she grew "big all over," even to the ends of the fingers, so that she could not pick up a pin. This enlargement after a time disappeared; but she remained stout afterward. Her first husband having died, she married again in 1843. Soon after, the abdomen began to enlarge, and she imagined she felt a movement within. Consequently, she supposed herself to be pregnant. Several physicians whom she consulted also considered her pregnant, until she passed beyond her time. By pursuing a treatment of warm baths and frictions, both the swelling and the movements disappeared, and she continued well in every respect for two or three years. In the fall of 1849, she first noticed the uterine tumour, and consulted Dr. Lehman.

16*th*. I first saw the patient in consultation with Dr. Lehman, and examined her. She was a large, corpulent woman. The abdomen was very fat, so that the exploration, through its parietes, could not be made satisfactorily. An examination, per vaginam, discovered the cervix uteri to be elevated to a point opposite the right acetabulum. The cervix was turned up against the tumour, at a very acute angle, and the os tincæ was directed toward the symphysis pubis. The os was small and closed, the cervix remarkably soft and flabby, and not, in the least, enlarged or expanded. Above the superior strait of the pelvis an irregular tumour could be felt, to the

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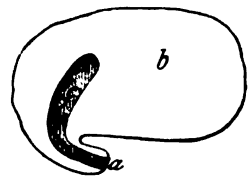
left of the cervix, stretching across the pelvis, while other projections could be perceived in other directions. It was hard, and somewhat uneven, and receded from the cervix very abruptly. The sound entered with difficulty, passed up about one inch, and was then arrested by the angle in the cervix. By continuing the manipulation for some time at this point, I succeeded in passing the instrument, and it passed in about four inches. The walls of the abdomen were too thick to allow its being felt distinctly above the pubis; but the motion of its end, in the cavity, was by no means free. Pressure over the pubis communicated motion to the sound. It was impossible, therefore, to decide upon the form of this tumour, or to indicate precisely its location; but the diagnosis was intra-mural fibrous tumour. The relation of the cervix with the tumour, as ascertained by the vaginal examination, may be thus represented (Fig. 5):—

Fig. 5.



a. Cervix uteri. *b*. Tumour. *c*. Cavity of uterus.

February 4, 1851. After having placed the patient for several days under the influence of ergot, I commenced operating, by passing a long bistoury into the os tincæ as far as the angle, and scoring it in several directions. The tissue of the cervix, however, was so flabby and yielding that it gave way under the edge of the knife, and very little impression was made on it. I repeated the operations on the 6*th*, 8*th*, and 10*th* of February, having administered ergot during the intervals, which caused considerable pain, but produced no manifest impression upon the cervix. It was only after these several operations, that I was now merely able to insert into the os tincæ the point of the index-finger. On the 13*th*, I again used the bistoury; but very unsatisfactorily, in consequence of the very flabby tissue of the cervix. To overcome this difficulty, I had manufactured a small bistourie caché, so that I could introduce it into the uterus like a sound. The following drawing (Fig. 6) represents this instrument, at half its size. I have used it repeatedly for enlarging the cervical canal.

On the 18*th*, I used this instrument by passing it well into the uterus, then opening it wide, and, while kept open, withdrawing it so

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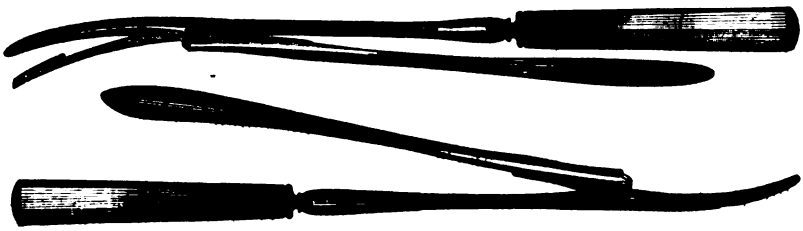
that it cut its way out. It was found to answer an admirable purpose; as, the expanded instrument, by stretching the cervix, kept the

Fig. 6.



tissue tense, and thus enabled the knife to cut easily. On the 20*th*, I repeated the latter operation, and succeeded in opening the cervix, so as to admit the index-finger to the os internum. After another section, on the 22*d*, I was able to pass the finger entirely in and hook it over the acute angle formed by the cervix. The menses now appearing, the operations were suspended until the 28*th*, when I again used the concealed bistoury, in association with Dr. Bruce, of Pittsburg, who had just returned from a professional tour in Europe.

March 3*d*, 4*th*, and 8*th*, the long common bistoury was employed in cutting into the substance of the tumour itself, for the purpose of breaking it up and causing it to degenerate and discharge. This was followed by several other attempts, at intervals of four days, until the 25*th*, after which all operative measures were discontinued, in the hope that the tumour would disappear. Besides, the mass of the tumour was so far beyond reach, and the corpulency of the patient so great as to render it impossible to manipulate over the pubis, that none of the operations were full and satisfactory. The tumour, however, was evidently diminishing, and as the operations could not be prosecuted with entire safety under the circumstances the patient was transferred to the care of her physician, Dr. Lehman.

February 25, 1853. I called to see Eliza. I had not seen her since attending her in consultation with Dr. Lehman, in March, 1851. She told me that in the spring of 1851 she passed through a very severe attack of illness; that after the operation the enlargement entirely left her, and that she was quite well the whole summer following. Before the operation she had always suffered the most dreadful pains, but afterward she remained entirely free from suffering. She had menstruated regularly ever since, with the exception of two months last summer. At this time she had a sudden

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fall, when she flooded a great deal. Her abdomen, at this time, is large; much of it is adipose deposit, while there appears to be also some dropsical effusion. She is still quite corpulent. I examined her again *per vaginam*, and can discover the remains of the tumour, though it has diminished considerably in size. Her general health is pretty good.

Remarks.—The great difficulty in the management of the above case arose from the acute angle in the cervix, and its extreme flexibility, and, also, from the impossibility of surveying the size and location of the tumour over the pubic region. Had the patient been a spare woman, with the parietes of the abdomen thin, so that the whole tumour could have been grasped and fixed in the hand above the pubis, I should not have hesitated to incise freely, with the bistoury, that portion of the tumour which stretched across the pelvis; and should have endeavoured afterward to extricate it from its bed without making any effort to remove it through the os tincæ. To attempt this, however, under the peculiar circumstances of the case, was considered unsafe; and hence the operation was conducted through the os and cervix uteri with only partial success.

CASE X.—*Miss E. K., aged 35 years; tumour intra-mural, very prominent above the pubis, extends upwards within two and a half inches of the umbilicus; cervix folded up against the tumour; tumour as large as a child's head; removed in detached portions; apparent convalescence; death from peritonitis; disease malignant.*

September 9, 1851. Miss E. K., of Carlisle, Pennsylvania, aged 35 years, consulted me on account of a uterine tumour. She gave the following history of herself: She began to menstruate between 12 and 13 years of age, and has continued regular since. The menses last three or four days, and are neither profuse nor clotted. She has not been troubled with leucorrhœa. One year ago last spring, she was seized with strangury, passing very little urine at a time, and very often. About the same time she felt an uneasiness in the abdomen while walking; she was hindered in her gait in the same way as if treading on an uneven surface. She noticed the tumour first early last fall; there were three of them, each about as large as a hickory-nut, hard, very slightly movable, and inclining to the left side. They were separate from each other, yet aggregated in the form of a triangle. Up to this time there had been no change in the menstruation. January last she was seized with a severe

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chill, which lasted three days, but was not followed by fever, although there were heat and great soreness in the abdomen. Until this period the tumours had not seemed to enlarge nor to become sore. After this they grew rapidly, the lowest one the most. They had never put her to any very great inconvenience at her monthly periods, or at any other time, until after this chill; since, the tumour at times had become very painful, the suffering being not confined to the menstrual periods. Her physician ordered her blue pill, which benefited her for a short time, but still, the tumour continued to grow. This was in March. In April, at her menstrual period, she was seized with a fainting fit; clots of blood were discharged with great pain, and were as large as the finger. This was accompanied with coldness of the surface. Her physician now applied an ointment of iodide of potassium externally, and gave her iodine internally, but without benefit. She continued to enlarge. Since this attack the menstrual fluid has always been clotted. Several intervals of menstruation, this summer, have been prolonged to five and six weeks, and every period now is painful for two days. She is habitually costive. There is an apparent obstruction in the act of defecation. She had a severe attack of strangury in May, which was accompanied by an inability to raise the left leg. Since the tumour became painful she has emaciated. Her general health has heretofore been good, and she never has had an attack of serious disease.

On examination I found a tumour, about the size of the fœtal head, presenting very prominently above the pubis. It was very hard and resisting, and extended from the pubis to a line two and a half inches from the umbilicus. There were two smaller tumours, each about as large as a walnut. The one on the right side was above the large tumour, extended to the navel, and to the linea alba; the other was about one and a half inches below the navel, and one inch to the left of the linea alba. There was some mobility in the small tumours, very little in the large one. The pelvis was filled with a solid mass. The os tincæ was sufficiently open to admit the index-finger up to the second joint, and the tumour could be felt occupying the left wall of the uterus. The cervix uteri seemed doubled up or folded against the tumour. The sound passed in four inches on the right of the tumour, and could easily be felt through the walls of the abdomen on the right side. Very little pain and no hemorrhage followed this examination. There was also an elastic swelling in the right groin, which had been there since last May. It returned a flat sound on percus-

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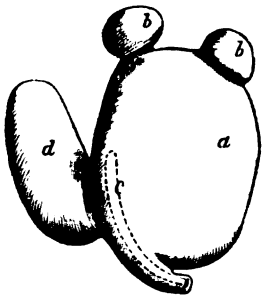
sion. The following diagram, Fig. 7, represents the relation of the parts:—

Fig 7.



a. Solid Tumour. *b.* The two small Tumours. *c.* The Uterus. *d.* Elastic Tumour.

The diagnosis was that a fibrous tumour was developed in the left wall of the uterus. An operation for its removal was desired.

10*th.* Ordered seven grains of ergot every two hours. After the examination she suffered pains of a bearing-down character, which were considerably increased soon after the administration of the ergot.

11*th.* Ergot was omitted during the night and commenced again this morning, and was still acting well. The patient slept none after her arrival in the city.

12*th.* The action of the ergot having been quite sufficient, I operated by passing the long bistoury about two inches into the cavity of the uterus, turning the edge toward the left side upon the tumour, and dividing its covering. This was readily done. Afterward, by means of the index-finger, I enucleated a large portion of the lower part of the tumour without any difficulty. Very little blood was lost. Three hours afterwards I examined the abdomen—the fundus of the large tumour had descended four inches below the umbilicus, the small right tumour about two inches, and the left one was on a level with the right. The large tumour was also much less prominent, and was very much lower in the pelvis. The os tincæ was dilating. The cut portion of the envelop was grasping the lower portion of the tumour like an os tincæ. The tumour was soft and elastic. Considerable bearing-down pain, and slight oozing of blood, followed the operation. Ergot was omitted and an anodyne administered.

13*th.* Passed a good night, free from pain. Ordered ergot again, and about half an hour afterward, with Dr. Grant, examined the patient and found the tumour already softening. I extended the

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opening into the cyst. The os tincæ was dilating. While Dr. G. supported the tumour externally, I was able to detach more of it by means of the finger. Administered an anæsthetic during this operation. Ordered ergot every two hours.

The same evening, with Dr. Grant, I again operated; the patient being under the influence of the chloroform mixture. I removed, in pieces, a large quantity of the tumour, perhaps one-fourth or one-third; at least, all the lower detached portions. It was still softening, although there was no smell of decomposition. Afterward, the patient took an anodyne, and had a comfortable night.

15*th*. Yesterday omitted the ergot and gave a laxative, which operated on the bowels. This morning ergot was given every half hour until eleven o'clock, with the effect of producing contractions of the uterus: the patient was placed under the influence of an anæsthetic, and another attempt made upon the tumour. Double the quantity of the tumour was detached in pieces; evident marks of putrefaction now existed. The tumour was reduced so as to be at least two-thirds less than its original size. It could be easily grasped by the hand. It was also softer.

17*th*. Dr. Grant removed a small portion of the tumour during my absence from the city.

19*th*. Removed the remainder of the tumour with very little difficulty; the uterus sunk below the pubis: there still remained the two small tumours and the deep-seated hardness between them, and, there was much more apparent, an elastic enlargement in the right iliac region.

After this the patient continued to improve, and was able to sit up on the 25*th* and the 26*th*, and was quite cheerful. After passing a comfortable night she awoke, at 3 A. M. of the 27*th*, with great pain and distress in the abdomen. I saw her at 8 A. M., with all the symptoms of violent and extensive peritonitis. In spite of the most active treatment she sunk under it, and died at 1 o'clock A. M. of the 28th.

28*th*, 2½ P. M.—Dr. Grant made a *post-mortem* examination. There was extensive peritonitis, the intestines were adherent and coated with lymph, and there was a large collection of purulent serum. The small tumours were projections of encephaloid matter from the uterus—the whole tissue of the uterus appeared to be converted into medullary matter, there being no muscular fibre perceptible. The right ovary was enlarged, and consisted of three cysts, each as large as a large orange, and filled with a clear fluid. The

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remains of the tumour removed were observable. The liver was healthy. The disease evidently was highly malignant.

Remarks.—It will be observed that, so soon as the covering of the tumour was severed, the mass that presented was soft and elastic, and soft portions of tumour continued to descend for several days without throwing off any fetor, showing that the degeneration of the mass had commenced, before the operation, as a vital process. The tumour had already taken on a cancerous action; and this was manifested by severe pain therein, by emaciation, and the whole physiognomy of the patient. It is not improbable, therefore, that in this highly malignant case, no other than a fatal termination would have occurred under any circumstances; but I think there is no doubt that death was hastened by a cause which had no relation to the operation. The last interference with the tumour was on the 19th September, after which the patient improved so as to be up on the 25th and 26th, and in good spirits. On the 27th, she was seized suddenly with marked symptoms of peritonitis, which caused her death. For the last few days the weather was wet, cold, and uncomfortable—her room was very damp and chilly, without fire, the windows raised to free the room of unpleasant odour, and the patient not sufficiently protected by clothing. All this, operating on a system previously prostrated, was sufficient to produce the inflammation which caused her death, and most likely was the true cause of it.

CASE XI.—*Mrs. E. W., 47 years old; tumour intra-mural; the whole anterior wall nodulated from the fundus to the os tincæ; patient perfectly anemic; incised the whole length of the uterus; recovered.*

In the latter part of September, 1851, Dr. Harry, of Chester County, called at my house to consult upon the case of Mrs. E. W., who had been greatly prostrated by repeated uterine hemorrhages, and in whose case he had exhausted all remedies. From the history of the case I suspected the presence of a tumour, and requested him to make an examination and write the result. The following is an extract from his letter: "I have just examined Mrs. E. W., and find the os uteri open to about the size of a shilling, and very dilatable, not possessing any contractile power; and within about an inch of the os a tough elastic kind of tumour occupying the whole of the uterus. I could neither pass my finger through it nor around it, so firm did its attachments appear to be. She still continues to waste, and is con-

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siderably exhausted, but not more so than I have seen her at other times. I stated to her and her husband the situation I believed her to be in, and told them I did not know of any remedy but the removal of the tumour. She and her husband wished you to be sent for immediately. I hope you can make it convenient to come up in the morning train."

September 30, 1851. I accordingly went to see Mrs. E. W., of Chester County, in consultation with Dr. Harry. The patient had been very much reduced by uterine hemorrhage, which was still going on. On my visit I received the following history: She is 47 years of age. At 14 she first menstruated, and without trouble or pain. The intervals were regularly three weeks, and the flow one week, free from clots, and not followed by leucorrhœa. She was married in the spring of 1827, the fall previous to which she took cold, and then had a painful menstruation. Eight weeks after marriage, she suffered dreadfully during menstruation. Since marriage she occasionally has had painful and clotted menstruation. She sometimes suffers, for three hours, the greatest agony, resembling labour-pains, of which she was relieved, once or twice, by taking the muriated tincture of iron. Last February, five years ago, at a regular menstrual period, in the course of one night, she had a copious loss of fresh blood, which produced extreme debility. For several successive periods the same thing occurred, with the same results; after which, the menstrual fluid began to clot, part of the time being fluid and part clotted. After some time the interval diminished to two weeks, sometimes to one, and finally she was hardly ever free from a sanguineous discharge. Fifteen months since she had a copious hemorrhage; another, six months afterward, and another three months after that, besides a continual discharge, varying in colour, sometimes white, at others red, and again as dark as tar. When the discharge was white, there was a great deal of watery fluid, resembling clear water, which gradually became darker and darker, and finally clotted. At times it was accompanied with a very offensive odour, though this had not been the case latterly. One year ago she had an attack of strangury, burning and itching in the bladder, and was forced to urinate very often. Formerly her bowels were constipated, now they are regular. Mostly, there is a feeling of obstruction during defecation. She has never been pregnant. Last June, three years ago, she had a severe attack of pain across the hips and lower part of the abdomen, which occurred directly after an attack of flooding. She has had two other attacks since, also after flooding, but not so

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severe. Last winter a year, she had a very distressing attack of neuralgia in the epigastrium, which lasted six weeks. Two years ago, in going down stairs, she fell and injured her back so as greatly to disable her.

She is now very much blanched from the loss of blood; and her pulse, which is 92, is very feeble. Her appetite is good. The skin has a peculiar straw-yellow colour.

I examined Mrs. W. and found that the anterior wall of the uterus was hypertrophied and nodulated, extending from the fundus to the os tincæ. It had not the usual fibrous feel, but yet there was considerable hardness in the enlarged wall. Viewed with the speculum, the anterior lip seemed to bulge into a rounded mass, as large as a small walnut, while the posterior lip was stretched around it. The sound passed into the uterus about one inch easily, then it was arrested in its progress, but after considerable difficulty and pain, it passed through an apparent stricture with a slip, and entered about three inches.

Every means having been employed by Dr. Harry, without effect, to arrest the hemorrhage, I proposed to lay open the hypertrophied portion of the uterus, with the double object of arresting hemorrhage and destroying the disease. The proposition having been accepted, I passed the long bistoury three inches into the uterus, and turning its edge against the anterior hypertrophied wall, made an incision along its whole length, sinking the knife deeply into the substance of the uterus. Immediately the nodules diminished, and the mass became softer. The hemorrhage at once ceased, and no bleeding followed. On withdrawing my finger, there adhered to it a grayish kind of matter. I could still feel another nodule to the left of the incision, but not larger than the smallest filbert, which I also incised. The following diagrams [Figs. 8 and 9] represent the condition of the parts previous to the operation.

Several days afterward I received the following letter from Dr. Harry: "The night after the operation, I was sent for to use the catheter, and drew from her about a pint of water; since which time she has remained rather comfortable, with the exception of neuralgic pains at different times and in different parts. She still continues to waste a little, but not so much as before the operation, and the discharge is becoming fetid. I examined the os uteri and found it in about the same condition as when you left. Her general appearance is better, and she told me to say to you that she felt *much* better."

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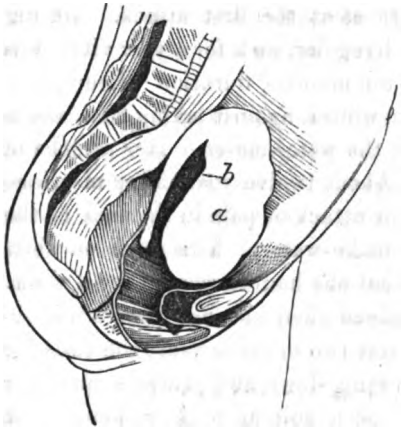
September, 1852. I have seen Mrs. W.; she has improved very much in health, and has had no return of hemorrhage.

Fig. 8.



a. Hypertrophied wall. *b.* Cavity of uterus.

Fig. 9.

a. Tumour in anterior lip. *b.* Posterior lip stretched around it. *c.* Os tincæ.

Remarks.—This case illustrates a mode of practice in prostrating hemorrhages arising from the presence of fibrous tumours, which I wish to impress upon the attention of the profession. Here all the usual remedies for arresting uterine hemorrhage had failed, but so soon as the hypertrophied wall was incised it ceased. The general health and constitution also had become greatly impaired, the cancerous diathesis seemed to have been developed, and even the tumours had softened so as to stain the finger with a grayish encephaloid matter; yet the operation was followed by restoration to health.

CASE XII.—*Mrs. E. A. M., 42 years old; tumour extra-uterine or pelvic; the uterus and bladder raised into the abdomen; gastrotomy; non-removal of the tumour; recovery from the operation; subsequent operations per vias naturales; tumour diminished in size; recovered.*

Mrs. E. A. M., aged 42 years, consulted me for an abdominal tumour, November 28, 1851. The menstrual flow commenced at 11 years of age, and occurred without pain or any other unpleasant symptoms. Her sister and her sister's child also menstruated at 11 years of age. For three or four months succeeding this the menses were irregular, occurring every week or two, the discharge having a natural appearance. She then became regular every four weeks. At 18 years of age she was married. A few months following, after

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undergoing very great fatigue, she was seized with great pain and pressure in the womb, and was confined to her bed for one week. This attack was accompanied with much irritation of the bladder. For a considerable time after, the slightest cold renewed the inflammation, but it never was as severe as at the first attack. During this critical state of health she was irregular, and, for a year after, was subject, at times, to fainting fits of ten minutes' duration Ever since, she has had to observe great care, in winter, against taking cold, and is particularly liable to cold when at the water-closet. Her attacks of cold fall always upon the womb. About twelve years after marriage she suffered intensely from a violent attack of pain in the neck of the bladder, with a constant desire to make water. This came on while travelling, and became so severe that she had to remain for a week. The urine was excessively hot. Since then she has always felt obstruction in urination, and for the last two or three years the calls are frequent. She has always had bearing-down and pressure since her first attack of pain, and has not been able to walk without great fatigue. The bearing-down pain at times has been so severe that her face would become purple. Two years since she suspected there was an enlargement of the large bowel, feeling as if there was a sinew stretched across her in front, and also across the lower part of her back. She had also suffered for years a pain in the xyphoid cartilage. Bowels are always regular. Never has had leucorrhœa.

About five years ago her friends noticed an enlargement of the abdomen, and she first felt something like a "lump" over the pubis, in the centre, about two years ago. It was uncomfortable on pressure and hard, about one and a half inches in diameter. It continued to increase in tenderness, but she was in a measure relieved by a liniment of laudanum, olive oil, and hartshorn. The last few weeks she had to enlarge her dresses in consequence of the distress at the epigastrium. She was forced to take off her stays one year ago to relieve herself from pressure about the pubis. Coitus has always been very painful; it was dreadful soon after marriage. It was least painful just previously to menstruation. After the first severe attack, a lady recommended her to wear a gold globe pessary, which in the course of half an hour gave her great relief from the pressure. She wore it perhaps a year; after that coitus was not so painful.

About two months since her physician, after making an examination, decided the existence of a tumour, and recommended her to consult me.

December 5, 1851, I examined the patient. When lying on her

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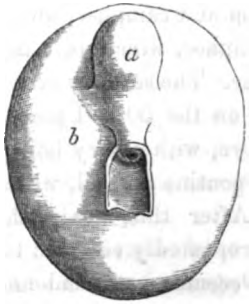
back, there appeared to be a general fulness of the abdomen, uniform in shape, and well supplied with adipose tissue. A tumour could be indistinctly felt occupying the whole lower part of the abdomen, below the umbilicus. It was evidently very deeply seated, and hard to distinguish, but could be felt most distinctly over each groin, by deep pressure. There was a very tender spot about two inches below the umbilicus.

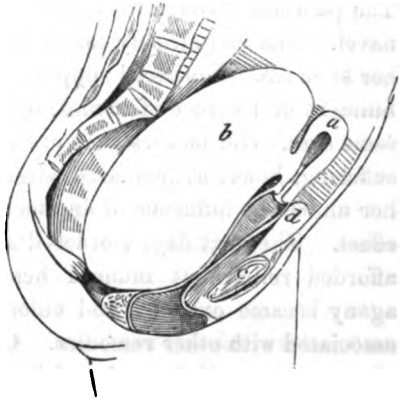
On examining per vaginam, the pelvis was found to be occupied, almost wholly, by a resisting tumour, having a firmly nodulated or granulated feel. It seemed to lay behind the vagina, pressed against the perineum, and filled up the whole cavity. No appearance of the uterus could be discovered in the pelvis; but by placing the patient on the left side, with the knees drawn up, and pushing up the index-finger, to the utmost extent, behind the pubis, the os tincæ could be merely reached—it occupied a position about one and a half inches above, and anterior to, the symphysis pubis. The os seemed entire and detached from the tumour. With considerable difficulty the uterine sound was introduced and carried up into the interior of the organ four inches; and its point could be easily felt one and a half inches below the umbilicus, seeming to be quite superficial. The operation gave some pain, but there was no bleeding. The sound introduced into the bladder, showed that this organ also was elevated into the cavity of the abdomen. There was no examination per rectum, in consequence of an attack of hemorrhoids. The following diagrams, figures 10 and 11, represent the relative position of the parts.

Fig. 10.



a. Uterus. b. Tumour.

Fig. 11.



a. Uterus. b. Tumour. c. Pubis. d. Bladder.

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9th. Examined the patient again, in company with Dr. Grant. The sound was introduced; and, while in the uterus, Dr. G. manipulated the tumour; but, in consequence of the peculiar position of the uterus, no motion could be communicated to the tumour without necessarily giving a similar motion to the uterus. The sound, however, did not appear to move to an extent corresponding to the motions of the tumour, but synchronously with it. A sound passed into the bladder, did not incline to either side. Per rectum, the tumour was felt anterior to the bowel, which was pressed by it back close against the sacrum. The tumour occupied the whole space between the rectum and the vagina, and filled the pelvis. Upon placing the patient upon her elbows and knees, I attempted to press the tumour from the cavity of the pelvis; and, although I supposed I raised it about one inch, I could not dislodge it from its bed.

The same evening Dr. Rumsey, her former physician, met us, and stated that, when he made examination two months ago, he could distinctly feel the tumour extending two or three inches above the umbilicus, and that it was readily movable from side to side; that the shape of the abdomen has since altered; that, instead of being quite prominent, it has become wider below, and flatter; and, that he thinks the tumour may have tilted over, which now causes her the great distress she is suffering.

The diagnosis in the above case is quite obscure. The tumour is fibrous—most probably extra-uterine; but, as we could not decide, we have agreed, if the patient should insist upon an operation, to open the abdomen, and be governed accordingly.

I was first called to see Mrs. M. November 28, and found her suffering great agony, which had existed for two or three days. The pain was through the abdomen; more particularly, just below the navel. She had already taken morphia for relief; but it distressed her stomach. I ordered suppositories of opium and camphor, and a liniment of tincture of arnica, opium, and camphor, which gave her some ease. Her menses were present at the time. The severity of the suffering, however, increased afterward; and, on the 30th, I placed her under the influence of an anæsthetic mixture, with a very happy effect. The next day, I ordered a dose of turpentine and oil, which afforded relief, but induced hemorrhoids. After this, again the agony became extreme, and chloroform was repeatedly resorted to, associated with other remedies. On the 8th December, a belladonna plaster was applied to the abdomen, and, on the 9th, the following compound, made into a pill, was ordered three times a day:—

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℞. Pilul. hydrarg.;
Extract. hyoscyam. ää grs. ij.;
Rad. ipecac. pulv. gr. ½.

10*th*. Had a comfortable night, and was tolerably easy all day. Pain in the epigastrium and the left side, which had been very distressing, now gone. There was pain in the right side. Pulse 76. The tongue clean. Before this, the tongue had been covered with a very white and harsh fur, with red tip and sides. This fur had cleaned off, leaving a chapped and bright-red tongue, but the redness had diminished. Bowels had been moved several times.

18*th*. There were slight symptoms of ptyalism. Pills suspended. Patient did very well after last report, but complained of great soreness just below the umbilicus, corresponding with the location of the uterus. Much of the tympanitis and distress of abdomen had disappeared. Tongue clean; appetite improved; countenance better. It was necessary occasionally to use chloroform, to relieve pain. Bowels regular. Menstruation.

20*th*. The operation of gastrotomy was performed. The tumour was not removable. It occupied the posterior and inferior part of the cavity of the abdomen. It had a hard, unyielding, fibrous feel. Its upper surface was covered by peritoneum, which was soon reflected off to the walls of the abdomen—showing evidently, that it had originated in the cavity of the pelvis, and, as it increased, elevated the peritoneum above it. The patient recovered rapidly from the operation.

March 13, 1852. The patient was much more comfortable after the operation of gastrotomy, but regretted that she still had to endure the presence of the tumour. Having ascertained, at the time the abdomen was open, the exact location of the tumour, I proposed making an effort to destroy it through the vagina. Accordingly, assisted by Prof. Grant, I made an incision through the posterior wall of the vagina, until I came upon the tumour, into which I sunk the knife to the depth of at least three-fourths of an inch. The opening admitted the point of the index-finger, and it was filled up by means of a sponge tent. There were very little bleeding and very little pain, and the operation was followed by no unpleasant symptoms.

15*th*. Removed the tent, which caused some pain, but very little disturbance of the system. The bowels and bladder have acted well, and without pain, since the operation.

16*th*. Operated again—enlarged the opening in the wall of the

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vagina, so as to admit readily the index-finger. Afterwards, with the finger, I enucleated the lower portion of the tumour from the wall of the vagina, for about two inches in circumference, all round. Very little bleeding followed.

18*th*. I examined the patient, and observed that the lower portion of the tumour was throwing off a somewhat offensive discharge, so as to contaminate the finger with its odour.

20*th*. Called to operate again, but the patient had been menstruating since yesterday, and in consequence, the operation was deferred. On examination, however, I discovered that the tumour was beginning to soften, and the finger came away much more fetid. The patient also complained of the offensiveness of the discharge.

23*d*. The patient has been complaining of fulness and pain in the anterior and inferior part of the abdomen, and some oppression in breathing. Last night, however, a large gush of very offensive fluid occurred from the vagina, which relieved her. No doubt, the fluid had been retained in a purse, between the posterior wall of the vagina and the tumour, and the distension caused by it produced the distress.

24*th*. Another copious and very offensive discharge occurred again this morning. I enlarged the incision through the vagina upwards and downwards, and separated much more of the tumour, which had softened down greatly; the odour was extremely offensive, showing a rapid degeneration.

April 14. Up to this date the discharge from the tumour was very copious and offensive, occasionally stained with blood. At the same time the size of the abdomen diminished. To-day I operated again, extending the incision through the walls of the vagina towards the rectum nearly an inch more, and, having detached more of the tumour, I turned the edge of the knife forward, sunk the beak into the tumour, and cut through its substance, carrying the knife forwards towards the pubis, and divided the tumour in an antero-posterior direction. On passing the index-finger into the interior of this cut, I found the tumour quite softened and cellular, and a dark-coloured blood issued from it rapidly. This blood was mixed with other discharge, had an offensive odour, and was partly coagulated. From four to six ounces having come away rapidly, I introduced a sponge tampon, which immediately arrested the discharge.

After this the discharge continued more or less for several weeks; the tumour softened and very much diminished in size; the general health greatly improved; the patient began to exercise out of doors; and in the latter part of June, the discharge having ceased, and all

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operative measures having been carried as far as it was deemed safe, she went to the country for the summer.

September, the patient returned to Philadelphia; having enjoyed excellent health during her absence. The abdomen is still large, but in all other respects she enjoys good health.

Remarks.—The above tumour being extra-uterine, there was nothing to be gained by the administration of ergot, and consequently it was omitted in the treatment of this case. The separation of the lower portion of the tumour from its envelop, and the admission of air into its texture, started the process of eremacausis within it, which extended to a large portion of the mass. The patient at this date [March 15, 1853] is in the enjoyment of good health.

CASE XIII.—*Mrs. W. G., 62 years old; tumour intra-uterine; attached to the cervix; tumour removed; recovered from the operation; apprehension of cancerous degeneration.*

September 25, 1852. I examined Mrs. W. G. of Baltimore, a very large and corpulent lady. The uterus is enlarged, but movable, its mouth thrown back towards the rectum, and rather difficult to reach. The cervix was expanded; and, upon inserting the point of the index-finger into the os tincæ, which was sufficiently open to barely admit it, the anterior lip seemed to have been much thinned down, and the posterior one thickened. But the latter condition, viz. the thickening of the posterior lip, was only apparent; for, upon examining it more accurately, this thickened portion proved to be the lower part of an intra-uterine tumour, over which the posterior lip was tightly stretched, and to which the internal face of the cervix was attached; or rather the tumour seemed to have its pedicle arising from this point. The os tincæ was very much attenuated, open about half an inch in diameter, and stretched over the tumour, which, however, was entirely within the cavity, and seemed to be retained there in consequence of its attachment to the cervix. The sound entered the uterus four inches, and gave no pain. Very little bleeding accompanied the examination.

The following history of the case was written out by the patient's daughter, and is given in her own words: "Mother says, as near as she can recollect, she began to menstruate between the age of fourteen and fifteen, and was regular for about one year, when she was afflicted with a white swelling, of which she suffered for about five months. Her monthly sickness did not return then for about one

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year. She then continued regular. She was married at the age of twenty-eight. She is the mother of three children. The first was born in her 29th year, the next in her 32d, the last in her 34th. The afterbirth was attached to the last for about twelve hours. Her constitution was always good, her usual weight being about 275 lbs. She thinks change of life began at the age of fifty, from which she suffered little or no inconvenience, excepting an unpleasant itching in the groin and an occasional show. Of the latter I do not know that she was ever clear longer than three months at a time. She continued in that way until the sixth month 1851, in her 61st year of age, when she was taken with hemorrhage of the womb, which we thought was brought on by over-exertion. After a short time she became better, but had more or less discharge every day, affecting her very little, as she walked or rode out at pleasure. In the fifth month, the spring following [1852], she had another attack of hemorrhage, worse than ever, with periodical pain in her hips and back, which we checked by the Thompsonian treatment. She then partially recovered, the discharge being of a brownish colour, which continued until the middle of the eighth month following, when she was again taken with the hemorrhage, amounting to flooding, and we feared it would prove serious. We again resorted to the above-mentioned remedies, and flooding in some measure ceased, but in about two weeks she was taken with pain in the hips and back, which were apparently severe labour-pains. We then became more alarmed, and proposed to her to go to Philadelphia, and see Dr. Atlee. She consented, thinking it the last resort, as she was then suffering almost indescribable pain. We left Baltimore on the morning of the 25th of ninth month, and arrived safely in Philadelphia. She was favoured to get along without much suffering."

26*th*. I ordered 3j of powdered ergot to be scalded by nine tablespoonfuls of boiling water, and one spoonful to be administered three times a day. On the 27th, the ergot acted efficiently, and the patient had active labour-pains. An examination proved that the pain was uterine.

28*th*. The dose of ergot was omitted at evening, as the action of the uterus still continued; and, as it was desirable for the patient to have sleep, morphia was administered, but with very little alleviation of the pain until towards morning, when she got a few hours' rest. After a dose of ergot had been taken, which produced intense pain, I introduced the long bistoury, and nicked the anterior edge of the os tincæ in several places. After the incisions, I could sensibly per-

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ceive it yielding. No blood followed. In the evening I examined the patient, and found the tumour engaging in the os tincæ, and a slightly fetid odour was thrown off. The os was still more expanded.

29*th*. The uterine pains continued most of the night, the patient getting no sleep until towards morning. A dose of ergot was again administered in the morning, and again I nicked the os tincæ. In the afternoon my brother, Dr. J. L. Atlee, of Lancaster, visited the patient with me. The os tincæ was opened to the size of a quarter of a dollar. The expulsive pains continued.

30*th*. By means of the long bistoury I separated a large portion of the tumour from the left side of the cervix. This was done by keeping the edge of the knife inclined towards the tumour while its back was pressing against the interior of the cervix, the knife being guided by the point of the left index-finger. Very little bleeding occurred.

October 2. The tumour was descending, and I discovered attachments around the right side and in front. I detached all within reach in the same way as those on the left side. That portion of the tumour previously separated was evidently softening, and a decided fetor was perceptible. In consequence of the use of ergot she had suffered severe pain most of the time until 12 o'clock at night, when, by means of an opiate, this seemed to subside, and she slept well until morning, which refreshed her much.

3*d*. The discharge from the vagina became so very offensive as to require injections of a solution of the hypochlorite of lime. The pains again became intense, and were relieved by an opiate at night.

4*th*. By means of the finger and knife I separated the lower portion of the tumour from the walls of the uterus. It was softening more and more, and could now be readily broken down. It was getting still more offensive, and the discharge was increasing in quantity. Ergot continued. After the operation to-day, she had a slight chill, followed by some fever, and in the evening an attack of cholera morbus, which was soon checked.

6*th*. After administering a double dose of ergot, I operated again, and removed several pieces of the tumour by means of Bond's placental forceps. Two of these pieces were as large as walnuts, and very much degenerated, while another portion, removed, possessed evidences of vitality. It was estimated that about four ounces by weight were removed.

8*th*. After giving another double dose of ergot, I again removed

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about four ounces of the tumour, and broke up more of it within the uterine cavity, and separated its attachments to the walls of the uterus. The fetid discharge was copious, and its offensiveness almost intolerable. Ordered ergot to be taken every hour until active labour was induced. This occurred about 12 o'clock M., and she suffered until next morning.

10*th*. The whole cervix was now free from tumour, the os tincæ seemed to be contracting, and in the interior of the uterus I could feel the remains of the degenerated mass. The discharge was still more and more fetid. There was now no more red or flesh-like discharge.

13*th*. Ergot has been continued. The os is still closing more, and begins to grasp the finger. The shreddy remains of the tumour can be felt lining the walls of the uterus, and the offensive discharge continues.

16*th*. The patient being a very large and corpulent woman, with an abdomen loaded with fat, it was impossible to recognize the tumour through the parietes above the pubis, and consequently it could only be surveyed through the vagina. As all the tumour within reach seemed to have disappeared, I permitted the patient to return home.

22*d*. I received a letter of this date from her daughter, saying: "We left Philadelphia in the morning train for Baltimore; had quite a pleasant ride home; mother stood the journey beyond our expectations, for I do not know that she was any worse after we arrived than when we started. Her situation is much the same as it was when we left. We began giving her the ergot the next day, and have continued it, with the exception of one day. She has suffered severe pain every night since, with the exception of the one when she omitted the ergot. The discharge has varied a little; it is not so dark, more of a watery colour and character, and not quite so profuse. Her appetite has increased some since her return, and her general health is as well as we could expect, and her spirits still keep good."

30*th*. She writes: "Mother has had a different discharge from anything that she has had since the first week she was in Philadelphia. It looks like clotted blood, about a tablespoonful. It appears to be mixed a little with those little strings, and does not smell like the other, but quite as unpleasant to me. Her general health appears to be quite well.

November 14. Dr. Weeks, of Philadelphia, a friend of the patient, called at my house, and stated that he had been at Baltimore last

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week, and had examined Mrs. G. He found the os tincæ nearly closed, and thrown back, and difficult to enter. About three ounces of fetid discharge followed.

22*d*. Her daughter writes: "After the examination by Dr. Weeks, mother appeared much better; the discharge was more profuse, and of a thicker character. It appeared that the passage had in some measure become stopped. About three evenings ago she had quite a flow of blood, which did not last long, the discharge amounting to, I suppose, about two ounces. She has rested most of the night for the last two or three, but so soon as she rises out of bed, she has the return of pain, which sometimes lasts all day, but is not so severe as it has been. The discharge is still of a watery character, and resembles the washings of meat as much as anything else I can compare it to, sometimes more offensive than at others."

December 7. At the desire of the patient, I visited Baltimore and examined her again. The os tincæ, instead of being thinned, was now thickened and indurated; its orifice rather easily admitted the index-finger; no tumour on the interior could be felt. There was still a constant offensive discharge of a brownish colour. Her general health was tolerably good. From the condition of the os tincæ, I suspected the existence of scirrho-cancerous degeneration.

18*th*. Her husband wrote as follows: "There is not much change in my wife since thee was here, until the last few days, and that has been quite a flow of blood, say from eight to ten ounces within the last twenty-four hours. Since the bleeding commenced, the discharge has not been so great, nor nearly so offensive."

This is the last account I have had of the patient.

Remarks.—The greatly enlarged abdomen of this patient by adipose tissue, rendered it impossible to know anything about the development of the tumours upwards. I am inclined to think, however, that they projected from the interior of the cervix, and that they originally were scirrho-cancerous. After their removal the same disease developed itself at the points of attachment, and in the os tincæ. The final result of the case is not yet determined.

CASE XIV.—*Mrs. S. G., about 49 years of age; tumour intramural, having been developed in the anterior wall of the uterus, and expanding that wall into a cyst inclosing it; the whole tumour removed; supposed weight seven or eight pounds; recovered.*

Early in March, 1853, Mrs. S. G. came from Montgomery County,

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Penna., to consult me for an abdominal tumour. After a slight examination, she returned home, with the intention of making arrangements to come again to Philadelphia, to place herself under my care.

March 22, 1853. The patient having presented herself, I made a careful examination of the case, in company with Dr. T. G. Drysdale. She was rather a large woman, but having a relaxed abdomen, not much covered with fat, she was easily examined. The abdomen was occupied with a firm, solid tumour, the size of an adult's head. It extended about one inch above the umbilicus, within the same distance of the right superior spinous process, not quite so near to the left superior spinous process, and quite prominent anteriorly. It is not very movable, nor very sensitive on handling.

The os tincæ was thrown backwards, but could be reached without much difficulty. The point of the finger readily entered it; and, by the exercise of some force, could be passed through a pretty thick and rigid os, by which it was firmly grasped. Its thickest part was anterior. After passing the finger through the os tincæ and cervix, up to the second joint, a tumour could be felt occupying the anterior wall of the uterus, and bulging towards its cavity; it seemed to be continued from the thickened part of the cervix. While the finger was thus inserted, and the other hand placed upon the abdomen, so as to grasp the tumour, the uterus and tumour could both be played between them in every direction, proving that they were intimately united. The sound entered 6½ inches, taking a direction backwards, behind the tumour, and its end was felt above the umbilicus, deeply in, back of the tumour, and rather above it. The sound, while in this position, could be played between the two hands. Every motion of the tumour was communicated to the sound, and *vice versa* every motion of the sound was given to the tumour. The tumour could plainly be felt, occupying a position between the sound internally and the hand placed over the abdomen; and it evidently was inclosed in the anterior wall of the uterus. The following diagrams (Figs. 12 and 13) represent it:—

The foregoing examination was accompanied with slight bleeding, and the stretching of the os tincæ by the index-finger caused some pain.

The history of the patient follows: She was about 49 years old, began to menstruate at 14 years of age, and was always regular afterward. The menstrual flow invariably commenced with pain, and lasted six or seven days, but was very little in quantity. When 18 years of age she married, had six children, and her labours were

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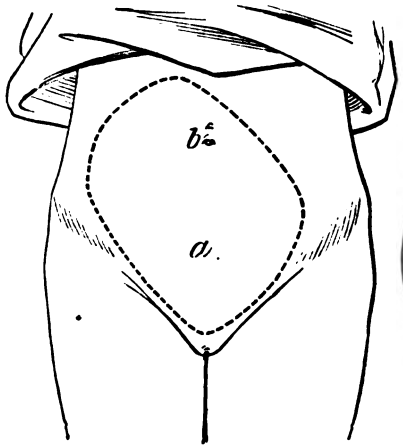
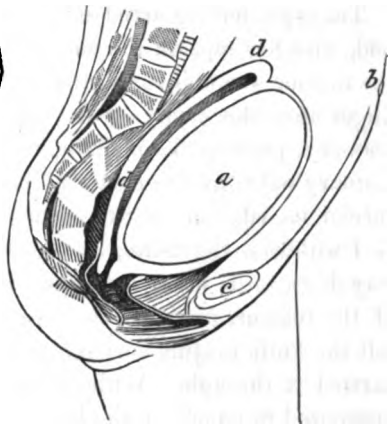
usually difficult. She nursed all her children until they were two years of age. Her last child was born sixteen years ago. Between

Fig. 12.



a. Tumour. b. Umbilicus.

Fig. 13.



a. Tumour. b. Umbilicus. c. Pubis. dd. Uterus.

the births she had two miscarriages, at the period of two months gestation, accompanied with considerable flooding. Her menstruation had been suspended during the intervals of childbearing, but after weaning her last child she again became regular; and, although she suffered pain, it was less so than before. About two and a half years ago, the menses assumed a clotted character. This was about one year before she noticed the tumour. The clots were as large as the fist, and accompanied every period, lasting three or four days; and she was so debilitated by the discharge that she was forced to keep in bed all the time. She had had great difficulty in passing urine; sometimes it came only drop by drop, at others in a stream, then would stop all at once. There was no difficulty in defecation. She still has her monthly turns, but often floods greatly. She menstruated last week. Last fall a year she first discovered the tumour. It was then as large as her two fists; her physician examined it, and treated it for six months with iron and iodine. It was sore to the touch, was not movable to any great extent, although it seemed to fall from side to side, in turning her body.

In anticipation of an operation, I now ordered her to take seven grains of powdered ergot every two hours, until I should see her.

23*d*, 3 o'clock P.M. Dr. Drysdale and myself called again. The ergot had produced slight sickness, some distress of the head, and

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pain and tightness in the back, loins, and lower part of the abdomen, and there was rather more tenderness of the tumour. She had taken seven or eight doses of ergot, and its specific action on the uterine fibre was manifest.

The ergot having acted sufficiently, I placed the patient across the bed, with her hips on the edge and her feet on two chairs, and while the tumour was well supported by Dr. Drysdale, passed my left index-finger into the cavity of the uterus, upon which I glided a long bistoury, pushing the blade up its whole length. After getting the bistoury well into the cavity of the uterus, and as high as possible, I turned its edge against the tumour and cut boldly into its substance as I withdrew the knife, laying open its uterine envelop the whole way down to its lower extremity, and sinking the blade into the body of the tumour, from half to three-fourths of an inch. Dr. Drysdale felt the knife grating through the fibrous texture of the tumour as I carried it through. With the point of the index-finger I now endeavoured to enucleate the lower portion of the tumour from its cyst, which I succeeded in doing, to the extent of two or three inches in diameter. At one point, a thick band obstructed this part of the operation, which I severed with the bistoury. There was not so much bleeding during this operation as accompanied the examination previously;—only a mere stain on the finger introduced. The patient complained very little of the operation. Ergot was continued, as before.

24*th*. The patient had been sickened by the ergot, and it had also increased the distress in the head. It likewise caused pain and tightness in the back and lower part of the abdomen. The tongue was somewhat furred, and there was slight febrile excitement. She had taken no ergot to-day; ordered it not to be resumed.

The os tincæ was thrown more forward, and was more easily entered; the tumour had descended, and could be touched with the index-finger half an inch from the os. The lower portion of the tumour appeared to be softening, and it threw off a slightly fetid odour. I again succeeded in shelling off more of the covering of the tumour with my finger.

Upon examining the patient, while lying on her back, the tumour was found to have descended about one and a half inches below its height yesterday. It occupied a more central position, and seemed to be narrower. She had a reddish serous discharge from the time of the operation.

25*th*. The tumour had diminished in size in all directions, and was

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softer over its anterior face. The cervix distended rapidly, and the tumour was pressing against the os. The finger discovered the tumour immediately on entering the os tincæ, and could be passed in all around the mass. It was quite clear that the lower portion of the tumour was escaping from its uterine cyst, and was rapidly degenerating and throwing off an unpleasant odour; the finger was stained by a dirty-looking discharge.

26*th*. The os tincæ was opening more and more, having expanded to the size of a shilling, and was thinning down at its edge. The tumour presented a much softer feel, was rapidly degenerating, and throwing off a more offensive discharge of a dirty colour. She had taken no ergot since the 23d; but a combination of blue mass, opium, and ipecacuanha. There was slight febrile excitement, and the tongue was coated with a white slimy fur. Ordered ergot three times a day.

28*th*. The ergot had produced decided uterine pains. The os tincæ was opened to the size of a quarter of a dollar, and dilatable. The size of the tumour was greatly diminished. It was softening more and more, and the discharge increased in quantity and offensiveness. The mercurial compound was suspended yesterday.

29*th*. Labour-pains pretty strong. Os tincæ open to the size of a dollar. Tumour still becoming smaller; discharge quite copious, and very offensive. Ordered an anodyne at evening.

April 3. Having been much indisposed, I had not been able to see my patient for several days until this day. The tumour in the mean time had diminished so much that, to the eye, nothing abnormal was observable in the abdomen. The top of the tumour could be felt at least two or two and a half inches below the umbilicus; the whole tumour was narrowed, and also much flattened, or, rather, much less prominent. The extremely fetid discharge continued. The doses of ergot had been continued with anodynes at night. Labour-pains were always induced by the ergot.

4*th*. In company with Dr. Drysdale, I attempted the removal of a portion of the tumour. The os tincæ was now fully dilated, and occupied by a large mass of the degenerated tumour, and above this mass could be felt a much more solid and resisting portion, upon which the disorganized and flabby mass was movable, and to which it seemed attached. Dr. Drysdale now placed both hands upon the lower part of the abdomen and supported the tumour firmly, while I, by means of Bond's placental forceps, the crotchet-hook, and the index-finger, succeeded in removing a large quantity of the dead tumour, which had evidently been originally fibrous, and still ex-

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hibited some of its characteristics. This brought me in direct contact with the more solid portion above. I now endeavoured to break up its structure, which I succeeded in doing, partially by the crotchet, but much more effectually by the point of the index-finger, the nail having been sharpened for that purpose. She bore the operation well.

On examining the abdomen afterwards, it was very apparent that there was a great diminution of the tumour.

7th. Assisted by Dr. Drysdale, I again succeeded in removing a large mass of the tumour, in the same way as before. The remains of the mass could be felt occupying a very limited space immediately over the pubis, its largest bulk being to the left of the linea alba. The mass on the right of the linea alba had nearly disappeared. The discharge was still copious, thick, of a dirty colour, and extremely fetid. No bleeding accompanied the operation, or followed it.

11th. Very little of the tumour could now be felt over the pubis. The discharge continued the same in character, but less copious. Per vaginam, I could detect the remains of the tumour within the os tincæ. The whole seemed softened and degenerated. With the point of the finger I disintegrated it, and removed several portions with Bond's forceps. Up to this time the patient had taken two doses of ergot daily, which always acted efficiently, and an anodyne at night.

20th. The patient had been getting along very well. The ergot and anodyne were continued, as before, up to this date; and, as no fetor was now observed in the discharge, they were discontinued. The fundus of the uterus was below the level of the pubis, yet it could still be felt by pressing the finger in behind the symphysis. The health of the patient was improving, and I allowed her to sit up.

May 1. Since the ergot had been omitted there had been a very rapid improvement of the health. The patient had been sitting up and walking through her room daily. Her appetite was good, she indulged in good substantial food, and she was making preparations to return home. On examining her, I found that the debris of the tumour had been thrown off by the uterus, and was occupying the cavity of the vagina as a condensed disorganized mass, from which it was readily removed. The whole tumour had now disappeared. Its original weight is supposed to have been about eight pounds.

Remarks.—This case, as well as some others, illustrates the fact

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that ergot will induce contraction of the uterine fibre *ab origine*, not requiring, as is supposed, to be preceded by parturient efforts. It would seem that one condition necessary to the specific action of ergot is, an elongation or greater development of the muscular fibre from any cause. This condition may either be partial or general, and its cause may be located either in the parenchyma of the muscular wall, or in the cavity of the uterus. Under either of those circumstances, the tendency of the action of ergot is to restore the elongated fibre to its original normal condition. Ergot, therefore, becomes a powerful agent in the treatment of fibrous tumours of the uterus, as the foregoing cases prove. In intra-mural tumours, or those entirely incased, I make an artificial os tincæ with the bistoury; the expanded fibre, stimulated by ergot, contracts, and the tumour is expelled on the same principle that a polypus or an ovum is expelled by the uterus.

This case also proves clearly that a section into the substance of these tumours is soon followed by their degeneration. Whether this is caused by destroying the integrity of their envelop, or through the admission of atmospheric air, it is difficult to decide. Perhaps both causes operate. Certain it is, however, that these tumours are so imperfectly organized, that eremacausis soon follows the above treatment, and may be developed throughout the whole mass.

Another fact illustrated by this case is the very important and practical one that we never need hesitate cutting boldly into those masses through fear of hemorrhage. Before the operation this patient had frequent floodings, and even during the examination she had pretty free bleedings; but from the moment of dividing the walls of the envelop there was no more tendency to bleed. This is my universal experience.

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PRIZE ESSAY.

THE CELL:

ITS

PHYSIOLOGY, PATHOLOGY, AND PHILOSOPHY;

AS DEDUCED FROM

ORIGINAL INVESTIGATIONS.

TO WHICH IS ADDED

ITS HISTORY AND CRITICISM.

BY WALDO J. BURNETT, M.D.,
OF BOSTON.

Natura in minimis maxima est.

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INTRODUCTORY NOTE.

THIS Essay was written in the latter part of 1851, and the many advances the science of structure has since made, has thrown so much light on some obscure points, that the parts relating to these ought almost to be rewritten, to be up with the times.

I have sought to, at least in part, make good this deficiency, by inserting as much new matter as well could be, while the sheets were going through the press.

BOSTON, Sept. 1853.

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PREFACE.

PHYSIOLOGICAL Science has such extended relations, and occupies so wide a field, that any one of its departments alone, furnishes us with material sufficient for the study of our whole lives. Each one of its specialities now covers more ground than did the entire fabric as understood by the earlier writers. This is so true that any comprehensive treatise on Physiology written at the present time would be encyclopædial in size; and, if the labour of a single individual, would necessarily be quite imperfect.

These views being correct, the plan of the present work, as indicated by its title, may seem altogether too comprehensive, if not presuming. But a consideration of the whole matter has led me to think that no other plan could be pursued that would enable me so well to advance those ideas and doctrines which experimental inquiry has shown me to be true.

Our most rational Pathology must now be considered as based upon our clearest and most correct ideas of physiological action. In fact, Pathology cannot, like Physiology, be regarded as a distinct science; for it is but the erring condition of the latter—its actions modified and governed by the same laws, but acting upon different premises.

These two branches, then, are efficient mutual aids. There can be no correct understanding of pathological phenomena without a previous knowledge of physiology; and, on the other hand, no one can carefully study the former without having his ideas of the latter made more clear and comprehensive.

It was for these reasons that I did not take for my subject, simply the physiology of cells, which would alone have been sufficiently extensive.

It may not be amiss for me to state that all the statements, facts, illustrations, &c. contained in the essay are my own, when not other-

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wise indicated; moreover, most of them were prepared and made specially for this work.

It has been a source of regret that so few of the more recent German works have been accessible to me in the preparation of this subject. During the past few years especially, the number of microscopical observers in Germany has been very great, and there are a corresponding number of works being published, of the highest and most trustworthy character. It would indeed be well if American students would divert somewhat their attention from Paris in this direction.

Most noted among these is the masterly work of Kölliker (*Mikroscop. Anatomie, &c.*), of which I unfortunately was able to possess only a small part until after the completion of this essay.

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INTRODUCTORY CHAPTER.

HISTORY AND CRITICISM OF CELL-DOCTRINES.

A SURVEY of the rise and progress of any of the inductive sciences fully shows that signal discoveries are never due to the labours of a single individual. For there existed previously, minds that had rudely caught at the same idea, or imperfectly pursued the same line of inquiry; but in which, either from want of requisite data, or more generally from the absence of a peculiar mental capacity, the subject did not reach its full maturity. Subsequently, however, that individual becomes the discoverer, who is so gifted that he can apprehend that peculiar colligation of facts, which always must and always does precede the discovery of an important scientific law. This view of the relation of the Discoverer to the discovery, detracts no merit from the former, for the work, however nearly finished it might have been, would not have been completed, had not the discoverer set the seal of his capacity upon it.

The history of the inductive sciences is full of examples bearing directly upon this point. In physics, such was true of the law of gravitation, as brought out in a perfect form by Newton. Every one had witnessed the falling of bodies towards each other or the earth; but it needed the mind of Newton to perceive in it the expression of a law as wide as nature herself.

Without alluding to other instances, I may say that this view is fully sustained by the historical relations of the subject under consideration. As we shall soon see, the existence of Cells, as component parts of the organisms, had long been known to anatomists and physiologists, but this fact found, for the first time, in the mind of Thos. Schwann, the requisite capacity to perceive in it the fundamental expression of organization, and that, therefore, it must have an application as wide as the range of organized beings.

This *principle of discovery*, as it may be called, the gift of a fortunate few, appears to sustain relations to physical facts, either singly or collectively, other than those with which we are now con-

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versant. So that I can entertain no doubt but that the truth of the whole law was impressed upon the mind of Schwann just as forcibly at the hour of its suggestion, as it now is, after its widely extended verification.

The task of tracing out historically those steps of progress in physiology, which necessarily preceded the advent of the modern cell-doctrines, cannot be otherwise than both pleasant and profitable. There is a difficulty, however, in reducing it to the limits of a single chapter, without making that chapter include several others which treat of particular cells; and, therefore, to avoid repetition, if the following account is incomplete, the remaining portion must be looked for in the following pages, devoted to particular points.

The recognition of the fact that vesicles constitute a component part of the animal economy, did not, of course, occur until after the application of magnifying powers to anatomical structures. When this was made, is a point too obscure to be settled. But it appears to me that the application of such powers, in an intelligent way, cannot be traced farther back than the time of Malpighi and Leeuwenhoek, who seem to have observed wonderful things through their rudest of glasses. Malpighi* recognized the blood-corpuscles as little globules, and Leeuwenhoek† has given quite a good description of them, even to some peculiarities of their form. The labours of both these anatomists, in other departments, were something to the same effect; they made out tolerably well some structures and caught glimpses of others, but did not advance to the point of conceiving that there was any unity in organization, or that the vesicle was anything but *one* of its almost ever-varied forms.

The observations of Hewson,‡ made nearly one hundred years

* Malpighi, Opera Posthuma, London, 1686, p. 92.

† Leeuwenhoek, Opera omnia seu Arcana Naturæ detecta, Leyden, 1687, tom. ii. p. 421.

‡ Hewson, Experimental Inquiries, London, 1774–77. *Vide* also Sydenham Society edition of his works.

Remarks.—Owing to the much greater ease of study, the structure of the vegetable tissue, *as cellular*, appears to have been made out at an early date. It seems to have been first pointed out by Rob. Hooke (Micrographia, London, 1667). But it received a considerable advance from Malpighi, who sent his great work (Anatome Plantarum) to the Royal Society of London, in the year 1670. His observations appear to have been very accurate, and not only did he maintain the cellular structure of plants, but also declared that it was composed of separate cells, which he designated "Utriculi."

From this time until the year 1831, the *cell* (I should say rather *vesicular*) structure of plants was pretty clearly understood by botanists. See Mirbel, Traite d'Anat. et de Physiol. Veg., Paris, 1802. Also, the earlier works of Hugo Von Mohl.

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after, although remarkable for their time, and containing many tolerably well-elucidated points, involved no histological relations, and in this respect did not perhaps contribute much to the advance of definite ideas of fundamental structure. In fact, during the 17th and 18th centuries, the microscope, rude as it was, existed in the hands of but few, and was, for the most part, used to gratify a youthful curiosity, instead of being applied to the elucidation of any intricate subject.

The labours of Haller should not, in this connection, pass unnoticed; for, although he may not have elucidated many points by the microscope, yet the doctrines which he advanced, based upon a most profound erudition and an extensive experience, all tended to assign an individuality to organization; and with such a unity of purpose, there would necessarily supervene a seeking for a unity of structure.

It does not appear that any advance was made as to the points under consideration until the time of Treviranus,* who, in 1816, published his account of the tissues, dividing them into three kinds, viz. *amorphous*, *fibrous*, and *globular*.

This announcement is somewhat remarkable for its correctness; but I think it must be regarded as premature, the experience of the time not justifying so broad a generalization.

The work of Heusinger,† which appeared soon after, may justly be considered as marking an episode in the history of animal tissues. For in it we find views expressed, which, although not founded upon careful and extended investigations, fully show the marked tendency of studies of this kind. This tendency here was in the organized, as it had already been in the unorganized world; viz., to seek for the idea of the *whole* in a unity of elements.

Heusinger even attempted to explain how fibres and tubes are formed from elementary spherical globules, by the linear arrangements of the latter.‡

But, remarkable as this may appear, his ideas of these relations if I may thus express myself, seem to have been anatomical, and not physiological; for he does not appear to have conceived of the true physiological signification of these globules.

Passing over a brief period, we find that, in 1833, Robert Brown§

* Treviranus; Vermischte Schriften, Göttingen, 1816.

† Heusinger; System der Histologie, Eisenach, 1822–24.

‡ Heusinger; *loc. citat.* i. p. 112.

§ Brown; Transact. Linnean Soc., London, 1833.

Although this was published in 1833, there are good reasons for supposing that the discovery should be dated 1831.

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discovered the nucleus of the vegetable cell, pointed out its constancy, but does not appear to have recognized its physiological relations with the cell itself. Nevertheless, I regard it as a necessary and important step in the gradual development of those doctrines which ultimately were to assume a more perfect form.

At about this time a new ardour for the study of minute anatomy seems to have arisen with many German and French physiologists. This was due, in part, to the very admirable class of scientific men just then appearing, and in part, also, to the better class of instruments becoming available.*

The names of Schultz, Purkinje, Valentin, Wagner, Müller, and others, cannot be too honourably mentioned for their histological labours at this period.

The investigations of Purkinje, in connection with Deutsch,† upon cartilage corpuscles and the process of ossification, served to point out the relations which simple cells might sustain to the more compound tissues.

In 1835, there appeared the remarkable work of Valentin,‡ upon the histology and development of tissues. I think there can be but little doubt that here we find the first traces of cell-doctrines, or the recognition of cells (I will not say nucleated cells), as the bases of all organized forms; and which, subsequently, were so brought out by Schwann, that he may be truly said to have made them justly his own. Valentin witnessed the formation of pigment-cells around pre-existing nuclei; he also instituted the comparison between cartilage cells and those forming vegetable cellular tissue, and pointed out other important relations.§ In fact, I think it may be said of Valentin, that he perceived the true physiological relations of cells as far as he well could without apprehending the grand fact that the nucleated cell is the fundamental expression of organic forms. It was by the appreciation of this grand fact that Schwann has set his seal upon all previous labours—for this was the *vis vitæ* of the whole matter, and, however strongly Valentin may urge his claims,‖ I cannot regard them in any other light.

In the following year Schultz¶ published a work on the circulation

* I refer here to the instruments of Frauenhofer, Shiek, and Chevalier.

† Deutsch; De penitiori ossium structura observationes, Breslau, 1834.

‡ Valentin; Handbuch der Entwickelungsgeschichte des Menchen, Berlin, 1835.

§ Valentin; *loc. citat.*, pp. 194, 209, 287.

‖ See Wagner's Lehrbuch der Physiologie, Leipzig, 1839.

¶ Schultz; Das System der Circulation in seiner Entwickelung durch die Thierreihe, Stuttgard, 1836.

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of blood, including the histological condition of its elements. He appears to have comprehended the cell relations of the blood particles, and describes their formation around a pre-existing nucleus. He noticed also many other important points; but his results were not appreciated at the time, and no general signification of these phenomena was entertained.

Immediately succeeding this, there appeared many new works on microscopic subjects. The microscope had ceased, with scientific men, to be an instrument of curiosity, and was now used as an auxiliary for the elucidation of the higher phenomena of life.

Numerous works were published—among the more prominent of which may be mentioned, those of Henle,* Vogel,† Valentin,‡ Donné,§ Boehm,‖ and many others, whose articles are scattered through the various journals of the day.

This brings us up to the period when Schwann commenced his investigations upon animal tissues, and with which those of Schleiden upon vegetable structures were opportunely contemporary.

The above-mentioned work of Henle fully demonstrated that animal tissues may be nourished without vessels—in fact, having a mode of life like that of vegetables. This, as Schwann himself observes, was a very important advance, as it brought the physiological relations of the two kingdoms to the same point. He showed that the *Rete Malpighi* consisted of *nucleated cells*, which grew from smaller ones; thus establishing the fact of the individual power of cells for their own life and increase.

It was about this time that Valentin,¶ when describing the nucleus of the epidermal cells, remarks, that they remind him of the nucleus that occurs in the cells of vegetable tissues.

Still, all these comparisons must be regarded only in the light of *analogy* instead of *homology;* perhaps in the same way that the wings of a butterfly would be compared with those of a bird;—no fundamental unity was perceived.

* Henle; Symbolæ ad anatomium villorum intestinalium imprimis eorum epithelii et vasorum lacteorum, Berlin, 1837.

† Vogel; Physiologisch-pathologische untersuchungen ueber Eiter, Erlangen, 1838.

‡ Valentin; Repertorium, Bds. i. and ii.

§ Donné; Recherches microscopiques sur la nature des mucus et de la matière des écoulements, Paris, 1837.

‖ Boehm; Die kranke Darmchleimhaut in der asiatischer Cholera mikroskopisch untersuchte, Berlin, 1838.

¶ Valentin; Nov. Act., N. C. xviii. pt. i. p. 96.

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In 1838, the excellent researches of Schleiden* upon phytogenesis were published. In it, Schleiden pointed out in a clear and definite way the formation of cells in vegetable structures, according to a single and uniform method; and by him, the law of unity of development of vegetable cells was believed and advocated.

At this time, while Schwann was at work upon the animal tissues, Schleiden communicated to him the results of his investigations, and Schwann, aware of what had been done as to animal-cell formation by Valentin and Schultz, seized upon the grand idea of *unity of plan* in the appearance of the elementary forms of organized structures. He gave an individuality, a vitality, to the anatomical details then collected. He caught the first glimpses of a truth, which, although it then needed a verification, was soon to be recognized as the grandest in physiological science.

The correct apprehension, then, of this law of fundamental development, as applicable to *all* organic structures, belongs to Schwann.† The steps of progress preceding it we have just reviewed. It is evident that the same course of inquiry had been imperfectly pursued before, or even that, previously, the same idea had been rudely grasped. In fact, according to the very constitution of science, it could not have been otherwise; for its history has taught us that induction is its broad highway, and it would be equivalent to conferring divinity upon a man to state that he made a grand discovery in science which had no relations whatever with the labours of others who had preceded him.

Such appears to me to be, in brief, the correct view of the origin and complete development of the cell-doctrine. I have based it upon all the historical evidence within my reach. It now remains to take a survey of the subject since that time; or, rather, since it attained its individuality of expression.

If it is true that all in physiology appertaining to animal cells, up to the time of Schwann, was truly made his own, by the interpretation he passed upon them; the same is nearly equally true of the labours in this department, made during the three or four years succeeding the publication of his work. For, those who took up this

* Schleiden; Beiträge zur Phytogenesis. Müller's Archiv, 1838, pt. ii. It appears, however, that these investigations were made some time before this date, for Schwann mentions that Schleiden communicated them to him in October 1837.—*Vide* Mikroskop. Untersuch., (preface.)

† Schwann; Mikroskopische Untersuchungen ueber die Uebereinstimmung in der Structur und dem Wachsthum der Thiere and Pflanzen, Berlin, 1839.

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line of investigation, and who, in one sense, deserved the name of original observers (when they recorded their own observations, preferred to quote the original language of Schwann). This is a fact in the history of science well worthy of remembrance, and especially by those who seek the honours due to the correct study of natural phenomena.

In the verification of this grand idea, the wide range over which Schwann extended his labours, has truly made subsequent observers feel that that field had been almost entirely preoccupied. It has also impressed upon them the belief that so much labour could only thus be so successfully accomplished, except under that scientific conviction of fundamental unities in nature; a *teleological* instead of a *physical* view of the relations of natural phenomena; which has always been the guiding spirit to great success in the comprehension of nature.*

But, in thus expressing myself, I would not claim too much for Schwann. Signal advances in any science require the successive and accumulated labours of many worthy men. In the present case, if, on the one hand, subsequent and more extended researches showed that the generalizations, naturally adopted by Schwann, were too broad, yet, on the other hand, this field has since been extended in a hitherto unknown direction.

The doctrine of Schwann is, that all animal tissues originate from cells, and that these cells are always formed around a pre-existing nucleus. As to each of these prominent points, differences of opinion have arisen with subsequent observers.

That all tissues have *not* the necessary pre-existence of cells, was advanced first by Henle,† as to some of the so-called fibrous tissues, and has since been verified as to structures known as *fibrillated*. In other words, a tissue may be formed by the aggregation of granules in a certain way, and without the intervention of true nucleated cells. As to this point there can now be entertained no doubt, for, since the time of Henle, it has and may be constantly observed.

But in regard to the other point, the formation of cells otherwise than *around* a pre-existing nucleus, the opinions have been many and

* I should mention, however, that, judging from a portion of Schwann's writings, he took the *physical* instead of the *teleological* view of organization. See *loc. cit.* Syd. Soc. Ed. p. 187.

† Henle; Traite d'Anatomie générale. Trad. d'Allemand, par A. J. Jourdain, 2 vol. Paris, 1843, tom. i. p. 374.

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various. Among the first who expressed their dissent, was Reichert,* who, in his embryological studies, sought in vain for the constant presence of the nucleus in the vitelline cells.

The discovery of Ascherson,† as to the phenomena sequent upon the conference of oil and albumen, was of some importance in relation to this point. For, according to it, cell-membranes might be formed around a drop or minute particle of oil, and without any nucleus. But this doctrine did not have much weight at the time, and we shall see, hereafter, that a different interpretation should be put upon these phenomena than had been supposed.

The phenomena of the multiplication of cells by segmentation, presented new and irreconcilable difficulties. The subject did not seem at all clear upon any explanation; moreover, at this time, Henle‡ had observed appearances connected with cells, which, although he was inclined to adopt generally the view of Schwann, led him to express his dissent of its universality of application.

At this time, the writings of Hugo Von Mohl§ and Nägeli,‖ as to the formation of vegetable cells, supported the doctrines of Schleiden and Schwann; and, considering their authority, tended, no doubt, to confirm the old view. The immediately subsequent writings, however, of physiologists of both the vegetable and the animal kingdom, were of a different character. In 1843, Karsten¶ published his dissertation on the cell, in which he dissented entirely from the "standard view," believing that cells originate without a pre-existing nucleus, and by the expansion of amorphous granules of organic matter.

In 1844, Kölliker,** a most trustworthy observer, after a consideration of the whole matter, expresses his belief in the want of unity of mode of cell formation; and declares that, if there is a *single method* which is invariable, it yet remains to be discovered. This was also the opinion of most of the excellent observers of the time. In fact, the method of Schwann seemed to be admitted as that of the exception rather than of the rule. Although such discrepancies in science are unpleasant at the time, yet, by them, science makes its most signal advances. In the present case, it was now evident that there

* Reichert; Das Entwickelungsleben im Wirbelthierreich, Berlin, 1840, pp. 6, 93.
† Ascherson; Müller's Archiv, 1840, p. 49.
‡ Henle; Lehre von den Gewebe, Leipzig, 1841, p. 154.
§ Mohl; Vermischte Schriften, p. 84.
‖ Nägeli; Ueber Entwickelung des Pollens, Zurich, 1842.
¶ Karsten; De Cella vitale Dissertatio, Berlin, 1843.
** Kölliker; Entwickelungsgeschichte der Cephalopoden, Zurich, 1844.

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was much to learn about a point which had been long regarded as quite definitely settled.

I shall not here dwell upon the details of these different views, for they are not easily understood without the aid of figures. But it appears pretty evident, and has since been so verified, that they were based upon correct observations. These differences seemed to increase in exact ratio with those of the observers.

In 1846, Mr. Paget,* in his excellent *Report on the Progress of Anatomy and Physiology*, expressed his doubts of the sufficiency of the then existing theories to explain many phenomena observed in the study of morbid growths. He had seen large growing tumours, in which there was *not* a nucleated cell, being only a collection of fibres; on the other hand, he had met with those, being only a mere collection of cells, and never apparently passing beyond that stage. This led him to the opinion that, in morbid growths at least, the cell is sometimes to be regarded as the *terminal* and not the *transitional* form. How far these views may be regarded as correct will be discussed on a future page.

Corresponding views, that soon followed from a great number of observers and writers, need not detain our analysis here.† Moreover, their writings are scattered through a mass of literature; and, even were any criticism desirable, it could be made with difficulty.

Since 1846, up to the present day (1851), the clearest and probably the most correct views of the origin and nature of cells, whatever may have been *the* mode, have been derived from the careful study of the genesis and structure of morbid growths; which, because they are morbid, are infra-formations, yet are connected with healthy tissue, and therefore may be justly considered as most fit for these inquiries.

* Paget. Report on the Progress of Anatomy and Physiology. July No. British and Foreign Medical Review, 1846.

† Among the writings that have appeared within the past few years, the following may be mentioned as worthy of reference:—

Reichert; Müller's Archiv, 1844–53.—The relations of cells, and the progress of the science, are here thoroughly discussed.

Kölliker; Schleiden's and Nägeli's Zeitschrift, fur Wiss. Botanik, 1845, heft ii.—This is a lucid exposition of the origin and nature of the nucleated cell.

Reichert; Müller's Archiv, heft ii. and iii. 1846.—Meckel; Müller's Archiv, heft i. 1846.—Courts; Gaz. Med. de Paris, 18 Avril, 1846.—Harting; Botanische Zeitung, 1846, p. 46.—Hugo Von Mohl; Botanische Zeitung, 1846, p. 387.—Nägeli; Schleiden's and Nägeli's Zeitschrift, fur Wiss. Botanik, heft iii. and iv. 1846; see also the translation by the Royal Society.

Gairdner; Rep. Brit. Assoc. Ad. Sc. 1850.—For other references, see the various English, French, and German Physiological text-books of the last four or five years.

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PART I.

PHYSIOLOGY OF CELLS.

CHAPTER I.

GENERAL RELATIONS OF CELLS TO ORGANIZATION.

WHEN a fact in nature has once been fully recognized, we are apt to seek for it too broad an application and generalization. This may be due to a fundamental law of human reason—an instinctive tendency to unity in our knowledge of all natural phenomena; it is a kind of intuitive prevision, which is really founded on induction in part, but of the use of which in science we cannot be too careful. Schwann was led to make this application in regard to the histology of all animal tissues; and, although subsequent experience has shown that it is not true as to the exact manner in which it was enforced—that is, the nucleated cell as the necessary basis of all tissues—yet it holds true when considered as to the real philosophy of cell life. As we have mentioned in the history and criticism, there is a class of structures the formation of which does *not* involve nucleated cells. Such are fibrillated and granular tissues, both as healthy and diseased products. They are formed by the aggregation and conference in a regular way, of the primordial granules of organizable liquids. All the other tissues, whatever their function or aspect, are formed through the agency of nucleated cells. In fact, these cells appear to be a physical state, through which, in order to acquire functional organization, these tissues must pass. In many of the compound tissues of the higher animals, the phases of these transformations are not easily traced, for they are transient, and generally not well defined. But, in some of the lower forms of animal life, and especially those which are aquatic, this is not the case, for here the adult forms are in one sense the persistent representatives of the embryonic forms

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of the higher types. On these accounts, our clearest views of the nature and function of tissues, have, in almost every case, been due to studies upon inferior animals. In the following pages, the advantages thence accruing will be easily perceived. Upon a comprehensive and correct appreciation of the relations of these various phases of cell-transformation, is based all our real fundamental knowledge of physiology.

There is one point concerning all these cell-conditions and transformations, which is now pretty well determined. It is that, in those cases in which the individuality of the cell is swallowed up in the substance of a tissue, the tissues thus formed are subservient to the functions of *organic* life. On the other hand, in those cases in which the individuality of the cell is preserved, so that a single cell is a true histological representative of the whole tissue, the tissues thus composed perform the higher functions of the economy, such as secretion and the elimination of certain forces. The idea here intended to be conveyed will be perceived, if I mention the schlerous and muscular tissues as examples of the former, and the nervous and epithelial tissues as examples of the latter case. This statement is, I believe, quite true with all except the lowest forms of animal life.

The relations of cell-studies to organization as modifying our ideas of the nature, *de ipso*, of function when applicable to animal tissues, require here a brief mention. Our ideas of the peculiarity of the function of any tissue are based upon our experience with it as such. Thus, we have been so accustomed to associate animal motion with the muscular tissue, that the former is not easily conceived possible without the presence of the latter. And, again, we have been so wont to link sensation with the nervous system, that the presence of the former has been deemed impossible without that of the latter. But cell-studies, in the lowest forms of animal life, show that these views are incorrect; for with them we do have *motion* without *muscle*, and *sensation* without *nervous* tissue; and in each of these cases, as we shall more fully show hereafter, the results are due to the agency of *simple* cells. In these facts, which have been carefully made out, is embodied a great deal of physiological truth; for we are thereby taught that function cannot be regarded as the result of this or that material form, but that this form exists as such in virtue of the function being performed perfectly. In other words, it is the acceptance of the *teleological* instead of the *physical* view of the relations of physiology to anatomy; a result, which I cannot conceive could have been

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obtained, except by a study of these ulterior forms of organized matter. And, while it is the legitimate offspring of such studies, it is at the same time the almost prophetic guide to many of our safest conclusions.

The general relations of cells to organization, as illustrated in the reproductive processes, cannot here be considered without a more extended view of these last than would be compatible with this part of my work. The elimination of a new and distinct being, is, properly, the only true individualizing process of the economy; for, if I may thus express myself, it is the potentiality of life made manifest in a chosen material form. This form is a *nucleated cell*, which, as it is the form in which and by which life dawned upon the inorganic world, so also does it seem the most appropriate form by which all the mutual physiological relations with that world can be best sustained.

CHAPTER II.

CELL-GENESIS IN GENERAL.

A KNOWLEDGE of the anatomical relations of an organized form, has always preceded the correct appreciation of its physiological conditions. This is so, perhaps, because the former are more easily made out, and when well understood scarcely admit of an hypothesis.

In cell-studies, as we have already seen, this has been quite true. Many botanists and anatomists, before the time of Schleiden and Schwann, understood very well the anatomical relations of cells. The same is true, also, of cell-genesis; for, as has been stated, both Valentin and Schultz had accurately observed how it occurred. The first description of cell-formation, as clearly understood from special study, appeared, as is well known, in the paper of Schleiden, published in 1838. The view here expressed was founded upon studies made upon vegetable structure; it coincides exactly, however, with the view of Schwann, based upon investigations of animal tissues. The brief but comprehensive description of cell-formation given in their works has been stereotyped in the minds of all subsequent physiologists. The well-formed cell consists of a close membrane or sac, more or less transparent, and filled with liquid giving it a glo-

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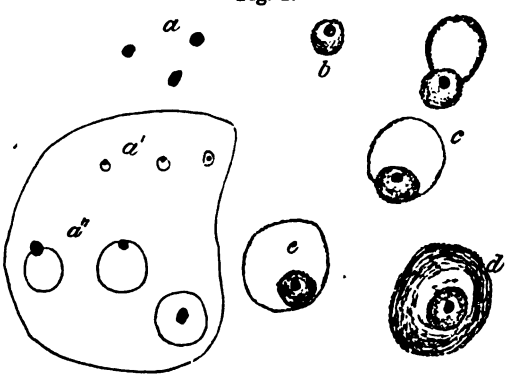
bular aspect. In this contained liquid is situated a small solid (?) body, either floating free or attached to the walls of the cell: this is the *nucleus*. This nucleus *may be* also hollow like the original cell-membrane; and, like it, contains a liquid in which is a smaller solid (?) body called the *nucleolus*. The cell consists, then, of three parts, and is called *nucleolated*. But we will take the simple *nucleated* cell, consisting of only the first two; here the nucleus is the *germ* of the whole, and therefore must exist before the cell-membrane itself. Moreover, the nucleus is supposed to differ also as to its chemical composition from the remaining parts of the cell. According to Schleiden and Schwann, therefore, the nucleated cell is regarded as consisting of three distinct parts, viz.: cell-membrane, cell-liquid, and nucleus. The nucleus is the starting-point. These nuclei, or cytoblasts, as they have been called, already existing in a formative plasma, the following is the mode by which the complete cell is formed. I take it from Schleiden, as originally published in 1838:—

"So soon as the cytoblasts have attained their full size, a delicate transparent vesicle rises upon their surface. This is the young cell, which, at first, represents a very flat segment of a sphere, the plane side of which is formed by the cytoblast, and the convex side by the young cell, which is placed upon it somewhat like a watch-glass upon a watch. . . . . The vesicle gradually expands and becomes more consistent, and, with the exception of the cytoblast, which always forms a portion of it, the wall now consists of gelatine. The

Fig. 1.



The mode of cell-formation, according to Schleiden and Schwann.* *a*. Cytoblasts or nuclei. *b*. Same, more highly magnified, showing nucleolus. *a'*. Formation of nuclei. *a''*. More highly magnified. *c*. Formation of cell-membrane from nucleus (*b*). *d*. Cell entirely formed, nucleolated, and nucleus in centre, free.

* *Vide* Sydenh. Soc. edit. of their Researches.

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entire cell then increases beyond the margin of the cytoblast, and quickly becomes so large that the latter, at last, merely appears as a small body inclosed in one of its side-walls."

I have thus quoted this description at length, that the peculiarity of this mode of formation might be clearly seen.

It is at once evident, that, if we admit the view here advanced to be correct, the existence of cells always presupposes the existence of nuclei; the former never being formed without the latter existing previously.

In the history of this subject, I have alluded to some of the views dissimilar to the preceding one, which have been urged by observers since the time of Schwann.

It appears, however, that they have been less inclined to put forward particular views of their own than to assert that they have been unable to witness the phenomena described by Schwann. And some observers,* whose experience in these matters is not inconsiderable, have declared that they have never witnessed the formation of cells in the way above mentioned.

My own experience, although not quite so exclusive, has not been much different. I have, in watching the formation of blood-cells, and that of some pathological corpuscles, seen all the phenomena pointed out by Schwann. But I have failed to recognize their universality, or even commonness. On the other hand, the whole tendency of my experience generally, and the results I have obtained from quite an extended series of investigations made especially with this object in view, have pretty clearly shown me, that, in animal tissues at least, the usual mode of cell-formation is based upon quite a different plan—involving views of the relations of the different parts of the cell to each other which I think pretty well borne out by the results of recent investigations, made by myself and others.

I propose to describe this mode somewhat in detail, not only because I think it new, and humbly claim for it an originality, but because of its commonness.†

We will take the example of the production of new cells in the

* Paget; Report on the Progress of Anatomy and Physiology.—Brit. and For. Med. Review, July, 1846.

Also, Gairdner; Report of the British Association for the Advancement of Science, 1850.

† This new mode of cell-formation I presented in a paper to the American Association for the Advancement of Science, Cambridge meeting, 1849.—See the Proceedings, p. 261.

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adult tissue. An organizable hyaline plasma having been effused by the nutrient vessels; in this plasma, minute dark points or utricles begin to appear. These constitute the first material manifestations of organization, and, strictly speaking, are the *germs* of future cells. They are minute sacs and not solid particles; a point I have very satisfactorily determined by the aid of the highest and best microscopic power. They appear to be formed by the conference of a minute particle of oil with albumen, uniting under the influence of vitality, according to the so-called Ascherson mode of cell-membrane formation. These minute utricles give the blastematous liquid a dotted aspect; moreover, being hollow sacs with albuminous walls, they are capable of endosmotic and exosmotic action. By endosmosis they increase, and become clearly visible closed sacs, which increase and grow apparently by simple expansion. Having obtained a certain size, the process of the formation of a nucleus begins to take place. This is accomplished in the following manner: The closed sac above described is filled with liquid; this is clear at first, but soon a cloudiness throughout appears. This nebulous appearance commences to recede from the periphery, where there appears a light space, the central portion of the liquid, however, being all the more dark and opaque. This condensation towards the centre going on, it terminates by the appearance in the centre of a minute vesicle or body, which is the nucleus. It is then a *nucleated cell*, capable of all the functions of such particles. After such processes have occurred, it may happen that this vesicular nucleus expands, as did the original cell-wall, and then, the same changes taking place, a body is formed within it; and in this way there is formed a nucleated cell in a closed cell membrane, constituting the true *nucleolated cell.*

But in normally formed tissues, these last-mentioned changes are far from being common; the original cell-membrane rupturing and passing away before the nucleolus is formed.

In brief, the mode of cell-genesis here described consists in the following points: 1st. The appearance of a primordial utricle in a formative blastema. 2d. Its expansion into a clearly seen vesicle. 3d. The partial condensation of its liquid contents towards the centre, giving rise to a new utricle, constituting the nucleus—the whole forming the complete nucleated cell.

It will immediately be perceived that, according to this mode, the cell proper necessarily exists before the nucleus, which is a kind of daughter-vesicle formed within it, and is a secondary product—but not at all dissimilar, either genetically or histologically, from the

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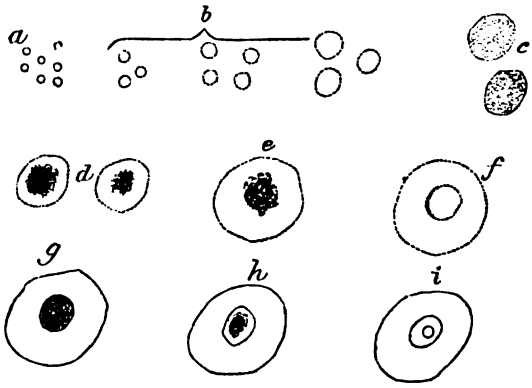
cell itself. According to this mode, also, there may be cell-sacs without nuclei, the formation of the latter having never, from some imperfection, taken place; then again there may be many nuclei in a single cell-sac.

Fig. 2.



Cell-formation, as I have observed it.—*a*. The so-called granules, which are really utricles, being formed by the union of oil and albumen. *b*. The same, increased in size by simple expansion. *c*. Still larger, and liquid contents cloudy. *d*. Condensation of cloudiness towards centre, leaving periphery clear. *e*. The same, farther progressed. *f*. Condensation finished, ending in a clear vesicle—the nucleus—the whole being the *nucleated cell*. *g*. Cell with liquid contents cloudy, as in *c*. *h*. The same, still farther progressed, as in *d*. *i*. Condensation finished, ending in a clear vesicle—the nucleolus—the whole being the *nucleolated cell*.

This mode of cell-genesis, which may be regarded as a peculiar one, has been based upon the results of a series of investigations made upon the origin and development of epithelial tissues; and I may also add that all or nearly all of my studies of the intimate character of tissues, both normal and pathological, have tended not only to support, but confirm this view. My investigations upon the vegetable tissues have not been extended, and if from what little I have seen I should be entitled to an opinion, I should say that this mode holds equally valid here, and the phases of development of some of the Cryptogamia appear to have a very good solution with this method of genesis.

The difference between the view of Schwann and Schleiden, and this one just enunciated, is this: The former regarded the nucleus as a primary formation, and as histologically and chemically different from the cell itself which is formed *around* it; while, according to my own view, on the other hand, the nucleus is a secondary product, formed *in* the cell-vesicle, and with which it is in every way homologous.

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I have found this mode of cell-formation to be quite general, and serving to explain phenomena hitherto obscure. In pathological products of either a heteromorphous or homeomorphous nature, it may be clearly observed. In fact, in many of these instances, I have been unable to perceive how any other view could be applicable at all.

In the normal textures, and especially those of the lower animals, in which the tissues remain persistent on their cell-stages, the same line of procedure is followed. The phases of genesis which are due to a difference of the cells themselves, cannot be well understood unless we take up their formation, each specially, as it occurs in the animal kingdom.

CHAPTER III.

CELL-GENESIS IN PARTICULAR.

I. The Ovum.

As is now well known, the ovum is, histologically speaking, only an epithelial cell, although we should not forget that, physiologically, it is something more. Now, as the mode of cell-genesis just put forth was based upon studies of epithelial cells, the inference should be valid, that ova are formed in exactly the same way. The results obtained by Prof. Agassiz, as to the formation of ova in the *Ascidian Mollusca*, are in complete accordance with this view.* To these I may add my own results, incidentally obtained over quite an extended range of the animal kingdom, from the genus Tubullaria of the Radiata, to some of the higher forms of the Vertebrata.

A detail of all these observations would be quite lengthy, and in this place unnecessary, however interesting they might be when written out as a special subject. I will here state the results I have obtained with the class of Birds, and they may be taken as a very good exponent of the others, made in the other classes of the animal kingdom.

At the approach of the period of procreation, there appear in the

* Proceed. Amer. Assoc. for the Advancement of Sc. second meeting. Cambridge, 1849, p. 157.

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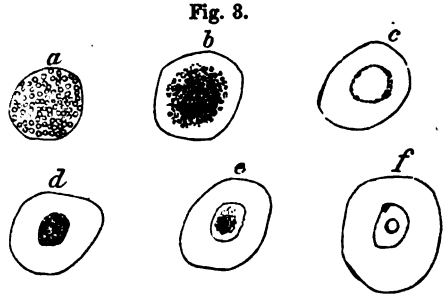
ovarian stroma, numbers of minute vesicles, or utricles, of a globular shape, and too small to admit of a very definite description. These increase apparently by simple expansion, and when just visible to the unaided eye, are filled with fine granules or utricles. At the next stage of development, a partial replacement of these utricles by a vesicle is perceived; and although I have been unable to watch the phases of change, yet I cannot but believe that it occurs by the same processes as those belonging to the formation of the nucleus, as we have just pointed out. This vesicle or nucleus, at first very small, increases quite rapidly, and in it is formed another vesicle or nucleus, constituting the third of the series. You have then the ovum, which is a great compound cell, the liquid of which is filled with granules constituting the *vitellus**—the nucleus constituting the *germinal vesicle*—and in this last the nucleolus, or *germinative dot*.

Fig. 8.



Formation of ovum as observable in birds. *a*. First appearance of ovum; as a sac filled with granules. *b*. The same, with a condensation towards centre taking place. *c*. Condensation finished, a clear vesicle appearing. *d, e, f*. The same changes repeated, ending in ovum (*f*), consisting of *vitellus*, *germinative vesicle*, and *germinative dot*.

This whole series, however, is not formed until the ovum, as a whole, has reached considerable size, or nearly that of the adult, at least with birds. This *germinative dot* may, as a vesicle, expand, and have within it several nuclei, as is not unfrequently seen.

Such are the phenomena as occurring in the class of birds. But with some of the other classes of animals, there occur peculiarities which will serve to illustrate the real nature of the genesis. On

* By a kind of condensation of peripheral layer of these vitelline granules, a membrane may be formed, constituting the so-called *vitelline sac*, which is pretty constant in the higher animals, but which is equally absent in many of the lower ones. This subject of a vitelline membrane, however, is one on which all are far from being agreed. I will only add that, among the Mammalia, it is admitted by Valentin, Wagner, Wharton Jones, and some others, while the opposite is the opinion of Baer and Bischoff.

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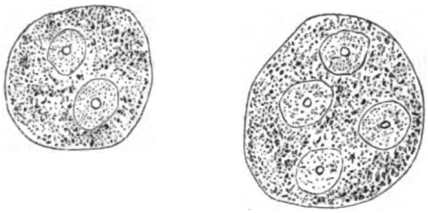
this account, I will here allude to some of them. In many of the *articulata*, and especially in the classes *insecta* and *arachnida*, the ova not unfrequently contain several nuclei or germinative vesicles. This was first pointed out by Herold.* But I have frequently noticed it in a series of investigations upon the embryology of spiders, made a year or two since, and in which I have carefully figured this peculiarity under its many forms. These facts go to show that the relations of the germinative vesicle to the ovum, are the same as those existing between the nucleus and the cell—that is, it is a secondary formation; and, as occurs often in cells, there may be a plural production. Moreover, it would appear from this fact that, however important the germinative vesicle may be in the evolution of the new being, it does not hold a single or unital relation to the ovum as a whole, as would necessarily be true were the Schwann mode of formation here admitted.† The vitelline substance is, as is well

Fig. 4.



Ova of common Spider (*Agelena nævia*), showing plurality of germinative vesicles (nuclei). (Magnified 50 diameters.)

known, composed of cells; and these appear to be formed, according to both the direct observations of Prof. Agassiz and myself, in exactly the same way—by the expansion of primordial utricles, in which cell-contents are developed. In stating these views of the genesis of the ovum, I am well aware that I am taking grounds quite dissimilar to those advocated by many of the physiologists of the day. The researches of Wagner, Valentin, and others, all advocate exactly the inverse method of production, that is, that the germinative dot exists as the primary part, upon which the germinative vesicle and vitellus are built up. Such also is the view which Schwann

* Herold, De Aranearum generatione, Marburg, 1824.

† Recent embryological studies, and especially some upon insects, have led me to think that altogether too much significance has been placed upon the agency of the germinative vesicle in the primitive changes of the ovum. In the Aphides, I have observed the egg-buds develop exactly as though they had been true eggs, and yet they have no trace whatever of a germinative vesicle—being nothing but an oval mass of nucleated cells.

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very naturally has taken; for, as is true of myself in relation to the cell theory I have advanced, he sought the same class of phenomena in the genesis of the ovum, which is really only a cell, as in the cell itself.

Farther researches in this direction must remove these differences; and although dissimilar opinions must always be expected, yet there may be equally looked for a greater uniformity of results, from the modern improved means of observation.*

* Owing to the importance of the subject, to look at it in an historical point of view (and which also might serve to make clear some of the obscure points), will not be a useless task. The anatomical relations of the ovum were known long before cell-studies, as such, were commenced. But the recognition of its particular parts as essential for certain functions does not date back many years. The germinal vesicle was recognised by Purkinje, in 1825, and called after that by his name. In 1827, Baer discovered the ovum of the Mammalia, which, however, he identified with the germinal vesicle of birds as pointed out by Purkinje. This point, however, was pretty clearly settled a few years after, by Coste, Valentin, and Wharton Jones. In 1835, Wagner pointed out the germinative dot and its relations. And it may justly be questioned if embryology could have been understandingly studied, that is, as to all the functions of the ovum, until these points were made out.

For investigations as to the *formation* of the ovum, considered as consisting of the three above-named parts, we must refer to those of Krause, Valentin, Wagner, Martin Barry, Wharton Jones, and Bischoff and Baer. They seem by no means to have arrived at uniform results. According to Wagner, Krause, Martin Barry, and Baer, the germinative dot exists first, and the remaining portion of the ovum is formed according to the Schwann mode of cell-formation. This, however, is not the opinion of Valentin and Bischoff. The latter believes that the germinal vesicle exists primarily, *in* which is formed the germinative dot, and *around* which, by an aggregation of granules on its periphery (and *not* according to Schwann's method) is afterwards formed the vitellus. In other words, the formation of the ovum is *sui generis*, and having no analogue in the production of any other tissues.

Of late, and since the publication of these writings of those who may be justly called the fathers of the science, the whole subject has been studied in a very thorough manner. I cannot pretend here to even allude to the various opinions put forth, and a history of the whole matter would be a volume of itself. It is evident that at least three distinct opinions beside my own exist as to the mode of ovum genesis: 1st. That of Wagner and others, which is according to the Schwann mode of cell-formation. 2d. That of Bischoff, which is, that the germinal vesicle exists first, the other parts being formed within and around it, as before mentioned. 3d. That of Bergmann, who thinks that the ovum as a sac is first formed, and subsequently the germinal membrane and its contents are formed in it. This is at least enough to fully show the unsettled state of the subject. As the views of Bergmann resemble my own, as above stated, more closely than any other, I will, for a moment, refer to them. His observations were upon the ova of the frog and the salamander. According to him, the vitellus at the commencement of its development consists of granules. These first arrange themselves into large groups, then divide into smaller ones, around which vesicles are formed by means of a viscid substance; you have these numerous cells filled with granules; and the whole contents are formed in a like manner.

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I cannot, with some, and especially Bischoff, regard the ovum as having a special formation, because it is an ovum. On the other hand, I naturally look to its genesis as a true exponent of those elementary processes of organization occurring in the tissues, of all of which it is the real parent.

The genesis of the *sperm cell* is exactly like that of the ovum; this is a point concerning which I have devoted considerable time, much more than to that of the ovum. Moreover, the ease of study of its phases of development is much greater. I have seen no evidence of its formation according to the Schwann mode of cell-formation, but it follows exactly in the line of that of common epithelia. As is now very satisfactorily determined, the ovum and sperm-cell are the true analogues of each other; their similarity of genesis would justly have been inferred from this fact alone, and hereafter, when treating of spermatic particles as special formations *in* cells, the other relations of the subject will be discussed. But, without forestalling those remarks, I will only add that, the evidence of this analogy being conclusive, the histological signification of the ovum as *a cell* is placed beyond all doubt. And as *a cell* we are entitled to seek for it, *à priori*, the same kind of genesis that belongs to cells in general, a point which I have just sought to establish.

II. Chyle and Blood-cells.

After those of the ovum and sperm-cell, the genetic relations of these cells next demand our consideration, for they are the first free and independent cells formed in the embryo, and are those by whose nutritive agency the others, which are united into continuous tissues, subsequently acquire their development. I am well aware that, in entering this field, I am taking hold of one of the most intricate and interesting of physiological subjects, and on which account, therefore, there are not only various but widely dissimilar opinions. My own observations, made for the purpose, entitle me, perhaps, to an opinion which is different from any I have seen, but it cannot be satisfactorily illustrated without that brief historical retrospect of the whole matter which will enable us to learn its phases of progress; a labour which, as to many other points also, cannot fail to be instructive.

The existence of these peculiar bodies (blood-cells) in the blood, was first noticed by Leeuwenhoek,* in 1674. It was sufficient for

* Leeuwenhoek; Opera omnia, &c. Leyden, 1687, ii. p. 421.

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him to recognize them as corpuscles, and as having a constant presence in that liquid. For nearly the succeeding hundred years, although their presence was well known to many anatomists, but little advance was made in a knowledge of their nature. In 1776, Della Torre* described them as little rings, having a dark centre, and he has figured their piling or clustering together which occurs during coagulation. But the first researches which are entitled to the name of *complete* were made by Hewson,† whose works were published at about the same time. As he appears to have had better means of investigation than the others who preceded him, his ideas of many of the peculiarities of these bodies are quite clear. We cannot wonder that he should have described the human blood-corpuscles as having a dark body or nucleus in their interior, when many observers of the present day persist in the same view. Owing to the publication of the results of Hewson, many new and curious investigations were made upon this subject, and that, too, over quite a range of the animal kingdom. I pass over here a review of those many writers who immediately followed this physiologist, and who have carefully noted the anatomical peculiarities of these cells as they occur in different animals (for these are secondary points to the subject of their genesis), to the writers to whom we will now direct the attention. Any investigations of this kind cannot be said to have been made until about the time of the appearance of the cell-doctrines of Schleiden and Schwann. As we have already seen in the history of cell-doctrines, Schultz‡ was the first who prosecuted these studies with success. He speaks of the blood-corpuscles of the chick as being formed *around* the yelk-globules, the latter existing as nuclei, from which arise vesicles, according to the Schwann method of cell-formation, although at that time Schultz probably had no idea of the signification of these processes.

* Della Torre; Nuove Osservazione Microscopische, Naples, 1776, p. 82.

† Hewson; Experimental Inquiries, &c. London, 1774–77, part iii. Or the republication of his works by the Sydenham Society. Edited by Gulliver, p. 220, *et seq.* Hewson was the first, I think, who pointed out the fact that the blood-cells of embryos and adults are different. Thus, he says, that those of the embryos of the viper and the chick are not flat and elliptical, but globular, and larger than those of the adult. This was an important fact to be learned.

‡ Schultz; Das System der Circulation, &c. Stuttgard, 1836. In a chronological point of view, the observations of Baumgaertner (Beobachtungen ueber die Nerven und das Blut, &c., Freyburg, 1839) should be mentioned first. He describes the formation of the blood-cells in the tadpole, by the aggregation of numerous granules around a central nucleus; these liquefy and form a membrane, and thus the corpuscle is formed (p. 40). We shall refer to this point on a subsequent page.

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In the embryology of Valentin,* published about the same time, a different opinion is maintained, from observations made since upon the same animal. Valentin regarded them as particular formations from the contents of the vitelline globules, which last, he affirms, are twice the size of the blood-cells. This formation takes place in the first liquor of the blood, which, he says, is completely transparent.

At this time, the constant presence of white or uncoloured corpuscles in the blood of vertebrates did not escape attention, and after Vogel† had pointed out their peculiarities, there naturally followed many speculations as to their nature and use. That they were the younger forms of the red cells, was first put forth by Wagner and Valentin,‡ a doctrine which, as we shall soon perceive, is advocated at the present time. This view appeared to derive weight from a second series of investigations made by Schultz,§ who affirmed that the coloured capsule was principally formed during the process of respiration.

In the next year Schwann's|| observations appeared, and although he expressed no opinion based upon his own researches, yet, from the evident cell-character of the particle, he naturally looked for its genesis according to the usual cell mode, and in support of which he falls back upon the results of Schultz.

Reichert,¶ in the year following, entertains the view that the blood-cells are special formations in the embryo, but out of the material furnished by the vitelline cells, and therefore resemble the primary cells of the organs generally. This opinion is also that of Bischoff,** at least with the oviparous vertebrates; but with the Mammalia, Bischoff regards the blood-cells as metamorphosed vitelline globules.

As to the formation of the blood-cells in the adult animal—the point now under discussion, Reichert†† entertains the singular opi-

* Valentin; Handbuch der Entwickelungsgeschichte des Menschen, Berlin, 1835, p. 296. At this time, Valentin's views do not appear to have been clear and explicit, although subsequently he may have made them so.

† Vogel; Physiologische pathol. Untersuch, &c. Erlangen, 1838.

‡ Valentin; Repertorium, 1837, p. 71.

§ Schultz; Ueber die gehemmte Auflosung und Auschirdung der Verbrauchten Blutbläschen. Hufeland's Journal, Apr. 1838.

|| Schwann; Mikroskop. Untersuch. &c. Berlin, 1839, p. 75.

¶ Reichert; Das Entwickelungsleben, &c. Berlin, 1840, p. 243.

** Bischoff; Traite du développement de l'homme, &c. Trad. d'Allemand, par Jourdain, Paris, 1843, p. 282.

†† Reichert. This is what Simon says is Reichert's opinion, as he learned by a private letter. *Vide* Simon's Chemistry of Man, Syd. ed. vol. i. p. 122.

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nion that they are the immediate production of the liver, and that this is the function of this organ.

Henle,* who has paid some attention to the subject, entertains the view that the coloured cells are formed from the lymph-corpuscles; and he regards the lymphatic glands as the chief, though not exclusive seat of the changes thus to ensue.† The views of Remak‡ deserve here an especial mention, not only from their plausibility, but because their real nature is fully illustrated by phenomena recently observed. His opinion is that they were formed in large parent vesicles, which entirely disappeared after the formation had taken place. These large vesicles he found in the lowest stratum of blood drawn from some of the higher animals, which had been allowed to stand for a time. His experiments were quite numerous, but they wanted completeness; moreover, his observations have not been verified, and there are good reasons for believing that the phenomena here observed were of an abnormal and not a healthy character.§

* Henle; Anat. Générale, &c. Trad. de l'Allemand, par Jourdain, Paris, 1843, t. i. p. 491. Also, as given by Simon from private letter; Chemistry of Man, vol. i. p. 121.

† Henle; as given by Simon: Chemistry of Man. Syd. ed. vol. i. p. 121. *Vide* also Traité d'Anat. Générale, &c. Paris, 1843, t. ii. p. 108.

‡ Remak; Medicinische Zeitung, July 7, 1841.

§ The phenomena here noticed by Remak, have been met with only as connected with pathological products, or under abnormal conditions. They may be seen often in extravasated blood, and appear to consist only of a group of blood-cells, around which has been thrown a plasmatic membrane, inclosing the whole as in a sac. This may be well observed in the blood of the elephant. Thus formed, contents of a very heterogeneous character may be brought together. See Kölliker; Zeitschrift für Rat. Med. bd. iv. p. 89. Also, an article in Edinburgh Monthly Journal of Medical Science, Sept. 1851.

This point has considerable interest from other connections. It is well known that in the spleen of the higher and lower vertebrates, and especially the latter, there are often found peculiar bodies which sometimes appear as sacs filled with blood-corpuscles in different stages of dissolution. With these and the like data, Kölliker has advanced an unique view of the function of the spleen, viz., that it is the organ for the dissolution and destruction of the affected blood-corpuscles. The support of this view he has undertaken at considerable length; see art. Spleen, in the Cyclop. Anat. and Phys. and his Mikrosk. Anat. &c. Bd. ii. Milz.

This view seems to have met with anything but the approval of other observers. Some have even denied that these saccular bodies in the spleen existed at all, while others regard them, when present, as quite accidental.

For myself, after having given several weeks' examination to the subject, and especially in conjunction with that of the function of the spleen, I must also dissent from Kölliker's views. My observations extended over great numbers of each class of the vertebrata, and, although I have very frequently found the bodies in question, yet

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A short time after this, the subject was taken up and treated in the most thorough manner by Mr. Wharton Jones,* and, considering the wide range over which his observations were carried, and the apparent care with which they were performed, they are certainly the most complete of any we as yet possess; moreover, they are now regarded as of authority on this subject. I shall therefore take them up rather in the manner of a discussion. Jones has undoubtedly laboured to establish a unity of mode of formation of these bodies, not only among the Vertebrata, but even among the Invertebrata. I trust, in making a brief *résumé* of his results, I shall not obscure this already sufficiently obscure subject.

Mr. Jones regards the chyle-corpuscle as the *basis* of the true blood throughout the Vertebrata. It is the starting-point from which all coloured cells must take their origin. In all the vertebrates *below* Mammalia (birds, reptiles, and fishes, in which the coloured corpuscle is distinctly nucleated), this formation takes place in the following manner: The chyle-corpuscle is at first a *coarsely granular, nucleated* cell; it begins to increase, and its surface becomes *finely granular*. From this stage, it passes on to having a smooth, clear membrane, and a well-defined nucleus, but is still colourless and globular. The next stage is that in which it assumes its colour and shape, and is then the perfectly formed coloured nucleated cell of these three classes. In Mammalia, however, a farther development takes place, for this cell-membrane bursts, and the *nucleus* of this cell escapes, constituting the true blood-cell; therefore, the red blood-cell of the Mammalia is not the analogue of the red blood-cell of the three other classes of the vertebrates, but of the *nucleus* of the blood-cell of these last. In other words, the blood-cell of Mammalia corresponds to the *products* of the blood-cells of birds, reptiles, and fishes; and, on this account, is regarded a higher formation.

In support of the view of the nature of the *nucleated blood-cell*, met with in the classes below Mammalia, he describes the phases of formation as he saw them in the blood of frogs. For these details, I must refer the reader to his own account. As to his view of the

from what I have seen of them, from the elephant to the eel, I can regard them only as accidental formations, or at least as bodies which have no special function.

For some farther reference to the subject, see Handfield Jones, Lond. Med. Gaz. Jan. 1847, pp. 140, 142; Ecker, Zeitsch. f. ration. Med. vi. p. 264; Virchow, Arch. f. path. Anat. u. klinische Med. i. pp. 452, 483; Funke, Ueber das Milzvenenblut, in Henle und Pfeufer's Zeitsch. i. 1851; Remak, Müller's Arch. 1852, Feb.; and Wharton Jones, Brit. and For. Med.-Chir. Rev. January, 1853.

* Wharton Jones; Philos. Transact. 1846, p. 63, *et seq.*

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nature of the blood-cell of the Mammalia corresponding to the *escaped nucleus* of the developed lymph-corpuscle, he remarks for its confirmation that, throughout Mammalia, there is an almost exact correspondence as to size between the nucleus of the *coloured nucleated cell* and the true blood-cell. This *coloured nucleated cell* (corresponding to the *true blood-cell* of birds, reptiles, and fishes) he has never seen in man, but thinks he has observed it in the horse and elephant.

In the embryo of Mammalia, he finds appearances agreeing exactly with the above view of the whole matter. Thus, in an embryo-ox of one and a quarter inches long, he found three kinds of cells in the blood, viz.: coloured and uncoloured nucleated cells, and the true blood-cells; thus having in the same liquid the elements of mammalian blood and that of vertebrates below it.

These last-mentioned phenomena, as observed in the embryo, coincide pretty closely with those observed by Kölliker,* who thinks that the embryonic blood-corpuscles are metamorphosed vitelline cells; but, in regard to the formation of the blood of adult vertebrates, he advocates the view that the coloured cells are the chyle-corpuscles directly transformed. On this point, therefore, he differs widely from Mr. Jones. I have mentioned this fact to show that the observing of the appearances in the embryo, which Mr. Jones has described, does not oblige one to adopt his view of the genesis of blood-cells in the adult animal.

These opinions of Jones I have not been able to reconcile with the appearances I have met, in studying this subject. And, in describing my own observations, I shall point out the grounds of disbelief in the view that, in the three lower classes of the Vertebrata, the coloured cell is the chyle-corpuscle developed and enlarged. But, in the Mammalia, a still wider discrepancy exists, and there are two points for which I have not been able to find the least ground for their reconciliation. The first of these is, the absence of the so-called *colourless nucleated cell* in the blood of adults, which should be found in great numbers; because, according to Mr. Jones, they are the parents of the true blood-cell.† The second of these is, the uniformity as to size of the chyle-corpuscle throughout the Mammalia; while, as is well known, there exists a very wide variation of size with the true blood-cell. In some, the chyle-corpuscle

* Kölliker, Henle und Pfeufer's Zeitschrift, iv. 1845–46.

† Mr. Jones speaks of finding the *colourless nucleated* cell only in the elephant and horse, among the Mammalia. I have often, and quite carefully, examined the blood of the elephant, but have failed to observe the nucleated cell here alluded to by Mr. Jones.

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and the blood-cell being about equal, as in man and the quadrumana; while in others, the former is three or four times the size of the latter, as in the musk-deer and goat. It is true that, in nearly all, the blood-cell is smaller than the chyle-corpuscle. But, according to the statements of Mr. Gulliver,* this is not true with some of the larger pachyderms, for instance, the elephant, in which the blood-cell is much the larger. Moreover, the size and general aspect of the true blood-cell hold certain invariable relations with the relative position of the animal as determined by a classification based upon other data. This fact, taken in connection with the great uniformity of size of the chyle-corpuscles, would lead us justly to infer, that, while these last possess only the common relations of cells, floating in a plasma, the true blood-cells have connection with the whole animal as a distinct and specific being. If this inference is correct, it at once shows how we are to regard the mutual relations of these two kinds of cells as existing in mammalian blood.

There is one other point to which allusion should be made. It is the difficulty of reconciling the oval shape of the blood-cells in the camel tribe, with the view that they are the *nuclei* of larger and colourless cells which are globular; for in the blood of all the oviparous vertebrates the nucleus has the shape of the blood-cell itself, which is oval; moreover, in all our cell-studies, it is the law that the nucleus and cell agree, at least in this particular. Mr. Jones refers to this point, and thinks the matter made clear, when he affirms that, in the *Paca*, the blood-cells become globular after the addition of water. He perhaps forgot that this same phenomenon occurs when water is added to the blood of any bird, reptile, or fish, and therefore this explanation is insufficient.†

Since the memoir of Mr. Jones, and up to the present time, there have appeared numerous detached papers on the subject, but most of them have wanted completeness.‡

* Gulliver, Proceed. Zoolog. Soc. London, No. clii.

I should mention, however, that my own observation does not agree with that of Gulliver; for in elephant's blood, which I have examined, I have found the red corpuscle to be 1-4500 of an inch in diameter, and the white only 1-2000.

† I have recently had an opportunity to examine the blood of the common lama (*Auchenia paca*, Illiger). The corpuscles were quite oval, being 1-8000 of an inch in length and 1-6000 in diameter. It is true that, like those of a frog, they became of a more spherical form by the addition of water, but they did not, by any means, lose their oval shape.

‡ I have not thought it proper to burden the general text with even an allusion to these many and varied writings. But, in an historical point of view, some of them

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I do not think that the views of Jones have been acceded to by most physiologists. Among these, may be notably mentioned Dr. Carpenter, who has advocated the distinct nature of the chyle-corpuscles, both as to function and distinction, believing that the blood-cells are special formations.* Mr. Hassall† entertains a similar opinion. But after all, the subject is in quite an unsettled state; and this is so because the opinions, justly deserving that name, of most observers have been formed more from negative than from positive data.

Whatever view we may take of the origin of the blood-cells and their relations to the chyle-corpuscles, this much is certain, that the materials of their formation in the adult are derived from the chyle. Therefore, in considering the histological relations of the latter, we shall be most likely to get at correct views of the genesis of the former. This I propose to do first: and whatever views we may thus obtain of what is properly the *reproduction* of these cells in the

are worthy of a brief notice in a note like this. I will go back to a few years preceding Mr. Jones's paper. In 1842, Mr. McLeod published his observations upon the development of the blood-cell in the chick, and which appear to be different from those either of Schultz or Valentin, before mentioned. According to him, in blood taken from the heart of a chick of three days, are found "a number of small granules floating in the field; these enlarge and become clearer in the centre, and when they have gained twice their original size, the central clear part becomes dull, and in a short time it is seen to be distinctly granular. The enlargement of these bodies, with the granular appearance of their centre, seems *not* to depend on the aggregation of granules round a central one, but on a property which they have *in* themselves of enlarging and presenting that figure. During all this time they are quite spherical. In the *second stage*, the central portion gradually becomes less opaque, and ceases to appear granular. It then has the appearance of a slightly flattened round cell, with a nucleus in the centre. The cell is disk-like, rather concave, but the nucleus convex. In the third stage, this concavity disappears, and the whole becomes slightly convex." (*Vide* London and Edinb. Month. Journal, Sept. 1842.) It will be perceived that the mode of genesis, here described, corresponds quite closely with that which I think belongs to cells generally.

Donné advanced the view that the spleen is the great elaborator of the coloured blood-cells. (*De l'origine des globules du sang.* Séance de l'Acad. 7 Mars, 1842.) Kramer has watched the development of the embryonic blood-cells in the frog. He says they are first *embryonic cells*, and then pass through a series of changes, ending in the perfectly formed corpuscles. In the adult, he is of the opinion that the chyle-corpuscle is transformed into the blood-cell by a clearing up and colouring process. (*Vide* Müller's Archiv, 1848, p. 63.)

Mr. Paget found the blood of a fœtus of one month to consist of *nucleated* cells, some of which had two nuclei, and were ovoidal. (*Vide* London Med. Gaz. Feb. 2, 1849.)

* Carpenter, Principles of Human Physiology, &c. Amer. ed. 1853, p. 169, *et seq.*

† Hassall, the Microscopic Anatomy of the Human Body, published in Fascic. London, 1847–51.

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adult, they can well be subsequently considered in connection with what we shall learn of what is properly their *primitive genesis* in the embryo.

If we take the chyle, as it is just forming, from the smaller chyle-vessels, we find it composed of a blastema, having a universally dotted or minutely granular aspect; or, in other words, composed of a hyaline liquid, in which float innumerable minute particles. These last constitute its only solid particles, and give the chyle its milky aspect. If we examine that taken from larger vessels, and those nearer the thoracic duct, we find, among these granules, small cells or vesicles. These are quite small, and appear to be simply vesicles, having only a hyaline liquid for their contents. There appears to be little doubt that they have not arisen except by the expansion of these primordial granules or utricles—according to the first processes of the method of cell-genesis I have already pointed out. Their simple, vesicular, hyaline aspect shows that they could not be formed *around* a primary nucleus, according to the method of Schwann. Once thus started, they go on increasing by dilatation; their liquid contents become granular, and some of the granules adhering to the inner surface of the sac, give the whole that well-known finely granular or nebulous aspect. Afterwards, the free granules in the interior partially centralize themselves, giving rise to the granulated nucleus. Such is the nebulous, granular-nucleated cell of the chyle, as met with in the thoracic duct. These characteristics become more and more pronounced, as the chyle approaches its influx into the general circulation. Thus far the subject is pretty clear and well understood. The chyle-corpuscles pass into the general current of the circulation, and, in so doing, it is evident that they become subservient, either directly or indirectly, to the formation of the coloured cells. This opinion, aside from any observations, could be justly inferred from their decreased number—being apparently replaced by the true blood-cell. Were this not so, we might justly ask, "What becomes of all the chyle-corpuscles?" This subserviency, according to my own observations, is *direct* in the oviparous vertebrates, but *indirect* in the Mammalia.

In birds, reptiles, and fishes, the chyle-corpuscle is the nucleus *around* which is formed the true blood-cell. I will here describe the phases of this development as seen in reptiles, the Anourous Batrachians. In the blood of these animals, after they have been well fed, two kinds of corpuscles are seen, and in nearly equal number. The first are small spherical or slightly oval corpuscles, having a dis-

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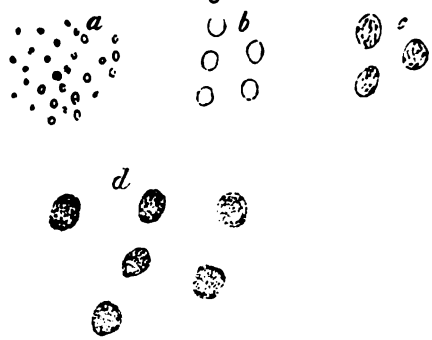
tinctly marked, granular, punctiform aspect. These are the chyle-corpuscles. The second are the well-known oval, nucleated, lenticular

Fig. 5.



Formation of chyle-corpuscles, taken from lymphatics of a goat. *a*. The first solid particles occurring in the lymph-liquid, and giving it a pellucid aspect—these are utricles. *b*. The same, increased in size by expansion. *c*. Still larger, with a few granules in the contained liquid. *d*. Contained liquid crowded with granules, the whole forming the chyle-corpuscle. (Magnified 250 diameters.)

bodies, the coloured cells being about three times the size of the first. The blood, thus composed, is to be examined without water, and even then the appearances of a formative process will not be seen at first. Many of the granular corpuscles appear only as such, but with a good light and a clear defining power, a delicate and almost transparent membrane will be perceived encompassing some of these corpuscles; at first, it is seen lying quite close upon it, but by the addition of a little water it is made quite distinct. With a few, this newly-formed membrane may be seen standing off, quite distant on all sides, from the granular nucleus. But it is so delicate that, after expansion to a considerable extent, it is nearly always ruptured by the pressure between two plates of glass. Until it has reached to about half the size of the adult cell, it is spherical and colourless. But after this, several notable changes begin to take place. The nucleus, hitherto *coarsely* granular, assumes a *finely* punctiform aspect, and finally the granules almost entirely disappear; it then has quite an even and smooth appearance. After this, both it and the cell-membrane, by which it is surrounded, begin to assume that oval and lenticular shape seen in the adult form. The contained liquid has a faint reddish hue, and so it may be said that they are partly coloured.

These changes, thus begun, continue, and end in the perfectly developed form. In some instances, however, the routine is a little different; the "clearing up" of the granular chyle-corpuscle takes

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place simultaneously with the formation of the new membrane around it, so that you have one *clear cell* contained within another. Then again, these chyle-corpuscles may assume a slightly oval shape before the enveloping membrane is formed; thus seeming to show that the idea of their future development is persistent in them, before any steps for its ulterior manifestation have been taken.

Aside from the evidence of this view of the matter, by direct observation of these changes, it is also supported by the fact that there is a close agreement as to size between the chyle-corpuscles and the nuclei of true blood-cells. Not only does this hold true of the blood of reptiles, but also of that of birds and fishes. Moreover, as far as my

Fig. 6.

Formation of blood in oviparous vertebrates.

(*Salamandra glutinosa.*) *a.* The Chyle-corpuscles. *b.* The same surrounded by a delicate membrane. *c.* The chyle-corpuscle become clear by a dissolution of the granules, the whole appearing as one vesicle within another. *d.* The membrane observed at *b.* increased, and having an oval shape. *e.* The well-formed corpuscle, having an oval nucleus partially clear—the whole coloured. These figures illustrate what I have observed with birds, and very many other reptiles. (Magnified 250 diameters.)

own observation goes, the chyle-corpuscles are smaller than the true blood-cells, throughout these three classes*—thus making it explicable that the latter should, in every instance, be formed *around* the former. In the cartilaginous fishes, in which the blood-cells are

* I have found this true of the smallest birds I have examined—and in these, above all other oviparous vertebrates, a deviation from the common rule would be most looked for. I do not know how it may be in the humming-bird; but Dr. Davy says, that here their long diameter is 1-2666, and their shortest 1-4000 of an inch, but he does not mention the size of the chyle-corpuscles. (*Vide* Proceed. Zool. Soc. in Ann. Nat. Hist. July, 1846.)

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quite large, much more so than in the osseous ones; and in all the reptiles, these relations are easily seen and satisfactorily made out. In birds, however, they are less clear; but in no instance have I met with appearances not reconcilable with—in fact, which did not demand—the above view.

The nucleated blood-cell belongs to the oviparous vertebrates alone; and I know of no instance in which it has been met with in the blood of adult Mammalia, at least, as a distinct and uniform body.

In the blood of Mammalia, a different condition of phenomena is met with. The perfectly formed blood-cells have *no* nucleus, but consist, each, of a lenticular, generally round vesicle, in which may occasionally be seen a few minute granules.

Generally, the white or chyle-corpuscles are the larger; and, as in the lower classes, they possess no regularly formed nucleus as a single body, but have in their centre, to correspond to it, two, three, four, or even more granules, which are of a quite uniform size and appearance.

The want of all correlation of size between these chyle-corpuscles and the true blood-cells or vesicles, appears to me to forbid the opinion that the former, as such, are directly concerned in the genesis of the latter; either being produced *in* them, according to the view of Mr. Jones, or *around* them, as is the opinion of Schultz.

My own opinion is, that the blood-vesicles of Mammalia are formed *around* the granules which constitute the compound nucleus of the chyle-cell, and this takes place in the same way as that of the blood-cell in oviparous vertebrates around the chyle-corpuscle itself; the nucleus afterwards becoming dissolved, and, in nearly every instance, entirely passing away.

In procuring the blood from living mammals for examination, we generally take it from some of the terminal capillaries, and such, therefore, as has traversed the whole line of the circulation. On this account, the phases of the genesis are not well marked, and seldom ever seen; the whole field appears, at first, occupied with only two kinds of bodies, the vesicles and the white corpuscles; and, in many instances, these changes have been so completed, that nothing else can be detected after the most careful search; but, generally, a little attention will show that a third form exists. This is a small *spherical* cell, slightly coloured, and in which may be often seen a few minute granules.

These cells are few in number, and vary in size from that of one-

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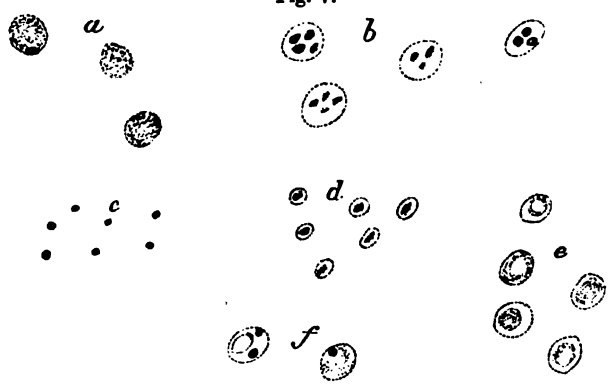
third to that of two-thirds or more of the size of the adult vesicle. These I have regarded as the newly forming blood-vesicles, the nucleus of which appears to disappear at a very early period; and, unless attention was directed to the point, they would not, on that account, be distinguished from the adult form, even although only two-thirds its size.

Fig. 7.



Formation of mammalian vertebrate blood.

(Man.) *a*. Chyle-corpuscles. *b*. The same treated with acid, showing nuclei. *c*. The escaped nuclei free. *d*. The nuclei with a delicate colourless membrane. *e*. Well-formed blood-corpuscles—having a dark or light centre according to focus. *f*. Blood-corpuscles with granules—the remains of old nuclei. I take here the instance of man, because his blood is always accessible; but the same phenomena can be witnessed with other higher animals. (Magnified 350 diameters.)

The fact that these phases of formation do not appear boldly distinct in our observations, is due, I think, firstly, to the rapidity of the genesis, and, secondly, to the fact just mentioned, that the specimens observed are generally from the terminal capillaries, by the time they have reached which, owing to the rapidity of formation, these changes are entirely or nearly completed.

In a liquid like the blood—the nutritive one of all the economy, and which is full of a formative plasma constantly in motion, and which has its vitality repeatedly renewed by respiration, the escaped nuclei of the chyle-corpuscles rapidly gather about them, each a plasmatic membrane, which, from the constant motion, is as rapidly expanded and developed.

Aside from the support which I think this view has from observed phenomena, it is the only one, in my opinion, that does not meet with overruling objections. I will here allude for a moment to some of these last, which are constantly occurring. The view of Wharton Jones is, that the blood-cells are the escaped nuclei, perfectly de-

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veloped, of the chyle-corpuscle; while that of Schultz is, that they are formed *around* the latter. Now, we will take the blood of man and of a goat; for, in them, the chyle-corpuscles are of about the same size, and larger than the blood-cell. It is, therefore, at once evident that the view of Schultz cannot be here maintained.

On the other hand, the view of Jones meets with equally valid objections. For the chyle-corpuscles being of the same size in these two animals, their nuclei (if it is admitted that they each have a single one, distinct) are also equal, and, being metamorphosed, should produce the same sized vesicle; or, at least, these vesicles should not be *smaller* than the nuclei. But this is not true—for, in the goat, the blood-vesicles are three and a half times smaller than the chyle-corpuscle; but, in man, they are only a third smaller.

But these difficulties do not present themselves, if we take the ground that the genesis takes place around the granules, which the chyle-cells contain; for, in every instance, they are smaller than the blood-vesicles, and may therefore well serve as a *punctum saliens* of the development of the latter.

The whole subject, however, will receive additional confirmation, when, in a succeeding chapter, we take up the functions of these two kinds of cells.

In the genesis of organic particles, which, throughout a wide range of animal life, have exactly the same function, we very naturally seek a unity of mode of expression. In the present case, this feeling is gratified by the manner in which we have just viewed the whole subject. It is this: the blood-vesicle in the Vertebrata is the product of the chyle-corpuscle, either as a whole or in part. *As a whole*, with the oviparous classes, in which it gives the formation a persistent nucleated character; *as a part*, in Mammalia, and in which, since it was simply a point of departure, and not an individual form as in the former case, it soon passes away, leaving the part as a simple vesicle.

It therefore may be stated that whereas, in the lower classes, the chyle-corpuscle is *directly* appropriated—in the higher, a step in advance has been taken, and the *elaborated products* of this corpuscle are used in the same way.

The formation of the blood-cells in the embryo necessarily involves changes considerably different from those of what is properly termed their *reproduction* in the adult. In the embryo, their first formation undoubtedly occurs *out of* the vitelline cells, and as such they obey the general laws of cell-genesis, and are nucleated; but it may

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be properly asked if these are *true* blood-cells? In the mammalian embryo, their reproduction takes place through the medium of the blood of the mother; but in the oviparous classes, it is out of the material laid up in the vitellus.

Now, their reproduction in the adult is out of a material elaborated from the food, which does not exist in the embryo.

These formations, thus necessarily different, cannot, I think, mutually elucidate each other's nature. Moreover, we do not yet fully understand many of the relations of embryonic life; for there are organs which then, and then only, have a functional activity. Such are, for instance, the vascular sanguineous glands; and recent investigations into their character tend to show that it is their part to elaborate a *cell-product* rich in albumen and protein, which is turned into the general circulation.* These cells are nucleated epithelial cells, which, by their association in the blood with coloured particles, might become partly coloured. Therefore, it appears to me that the fact of finding corpuscles of different appearances in the blood-liquid of mammalian embryos, is of no importance to the subject of blood-reproduction in the adult animal, in any point of view. In fact, I think it would be somewhat surprising that, in parts so newly formed, and that too out of nucleated cells, there should not then be found, in the vessels, cells of different aspects.

I have given some attention to these phenomena as occurring in very young embryos, and it may be well that I record here what was observed, and especially as it is confirmatory of the general tenor of the views advocated on the preceding pages.

My investigations have been with both the oviparous and the mammalian vertebrates.

We will look to them in their order.

1. *Oviparous Vertebrates.*—In the chick, the blood or nutritive corpuscles first appear in that portion of the germinal membrane soon to become the *area vasculosa;* this is at about the twenty-seventh hour of incubation. It is not, however, until nearly a day after this that the blood-cells can be well and satisfactorily distinguished. They then appear as cells or vesicles, having a variety of sizes and shapes, both nucleated and non-nucleated, and coloured in proportion to their size. They originate in the vitalized material (oil and albumen) of the vitellus, appearing first as small vesicles, without a nucleus, perfectly

* See a very elaborate Memoir by Professor Ecker, of Halle. Annal. des Sc. Nat. tom. viii. p. 103.

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globular, and faintly coloured. These vesicles increase by simple expansion, and their red colour becomes more and more pronounced as their size becomes larger. After having attained about half their full size, a nucleus appears in their centre, apparently from a kind of condensation of the contents, exactly as I have described the nuclear formation of cells in general, on a preceding page; that is, the central portion becomes opaque, which opacity disappears in proportion to the evolution of a distinct nuclear body. Most of the adult cells, therefore, have a nucleus, but generally it is small, and rarely central; in fact, seeming of no service.

When these changes have occurred, you have nucleated cells; but they have not their primitive spherical form, and are quite irregular. I have thought that a part of this irregularity was due to their compression in the newly formed vessels, while their imperfect lenticular character may be due to the fact of their being blood-cells; and this last shape has been gradually assumed as they have been developing.

Thus, it would appear that, when first formed, they originate in the nutritive blastema of the vitalized vitellus; commencing as vesicles, at first colourless, but which afterward become coloured as soon as a circulation is established, and then nucleated.

Now if, after the circulation is well established, say at the end of the eighth or ninth day, the blood be examined, it will be found composed of the same corpuscles, but which have a more regular shape and definite appearance; still, the absence of a *typical uniformity*, as is in the adult, will strike the observer. The whole condition seems to be conventional or provisional. The cells are generally distinctly nucleated; but the nucleus is small and round, and apparently sustains an indifferent relation to the whole as a simple cell. Their aspect is quite epithelial, and I am sure that, were it not for their colour, they would easily be mistaken for epithelial cells.

Fig. 8.

First formation of blood in oviparous vertebrates, from *area vasculosa* of chick of thirty hours' incubation. *a*. Vesicles appearing in vitellus; almost colourless. *b*. The same with a nucleus, and coloured. *c*. The corpuscles fully formed and coloured, having irregular shapes. Average size 1-3000th inch. (Magnif. 350 diam.)

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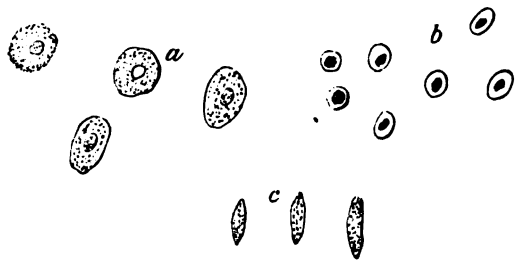
This, in fact, would show that they are, as we otherwise well know, simple vitelline products; for, as the ovum, vitellus and all, is, as we have seen, but a great compound epithelial cell, we should expect that the cells elaborated from its contents would take on an epithelial instead of any other type.

Fig. 9.



Blood of embryo chick of seven days' incubation. *a*. Blood-corpuscles coloured, nucleated, and oval-lenticular in shape. *b*. The same, forming, almost colourless and perfectly spherical. *c*. The same as *a*, seen laterally.

In regard to their constant reproduction in the embryo chick, or in fact in any of the oviparous vertebrates, I think it is merely by a repetition of the same processes as those of their formation. Kölliker,* Rees,† and others, are of the opinion that their reproduction is by division or segmentation, each corpuscle producing two by dividing in the middle. I have seen the class of phenomena to which these observers allude, and especially the hour-glass appearance of the globules. But, after considerable observation, I have been unable to satisfy myself that they are reproduced or multiplied in this way. In the young chick, I have frequently seen these peculiar corpuscles in the field, in the different stages of this change. In some, the shape was simply pyriform; in others, still more constricted, so that a small globule appeared connected with a large one; and then, again, the real hour-glass form. But what inclined me to the opinion that this change is not a normal or common one, is the fact, that, in those of a pyriform shape, the nucleus remained unchanged; and also that, in those of the hour-glass form, the nucleus was in one of the parts alone; whereas, in true fissuration, the nucleus is divided as well as the cell. I am therefore disposed to regard these phenomena as I do the crenulation of blood-corpuscles in general, that is, due to endosmose and exosmose.

* Kölliker; Henle und Pfeufer's Zeitsch. iv. 1845–6.

† Rees, quoted in Ranking's Abstract, vol. i. p. 251. Amer. edit.

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Fig. 10.

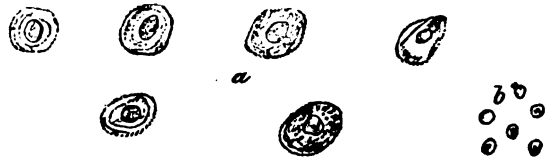
The hour-glass, fissurating appearance of blood-globules in embryo-chick of seven days. *a*. The globules apparently partially divided. *b*. The same unequally so, the nucleus on one side. *c*. The globules before these phenomena occur.

2. *Mammalian Vertebrates.*—In order to see the formation of the blood in these, it is necessary, of course, to take the embryo at a period so early that no vascular connection has taken place with the mother; and as good a time as any is when the bloodvessels are ramifying over the amnion, and just before they have formed any connection with the placental tufts. The blood then found belongs to the embryo alone, and has been elaborated by its own forces. Such was the case with the fœtal goats I examined. In a fœtus three-fourths of an inch in length, and fourteen days old, that is, after conception, the following phenomena were observed.

The blood had only *one* kind of cells. These were nucleated, spherical, having a size of $\frac{1}{1500}$ to $\frac{1}{2000}$ of an inch in diameter. A most careful examination showed the presence of no other forms. Now, what were these cells? To me they appeared only as coloured epithelial ones, and certainly, with the exception of colour, they could not otherwise be described. Their average size was about *four times* that of those of the adult goat.

I am, therefore, of the opinion that the blood-cells of the mammalian embryo are only epithelial cells, exactly as are most of their other individual free cells, and that they have taken the function of oxygenation provisionally, discharging it until the direct connection of the embryo with its mother, exactly as is the case with the embryos of oviparous vertebrates up to the time they escape from the egg.

Fig. 11.



First formation of blood in mammalian vertebrates, from fœtal goat of 14th day, having no vascular connection with the mother, but bloodvessels ramifying on allantois. *a*. Coloured nucleated blood-cells, uniform in size and appearance. Size 1-1500 inch. *b*. Blood-corpuscles of adult goat to show the relative size. Size 1-7000 inch. (Magnified 360 diameters.)

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This view of the whole matter makes this entire class of phenomena pretty clear. *The blood of embryos, as such, is of an epithelial nature, and simply provisional; and therefore cannot be compared with the blood of adults, which is elaborated from the food, and which, holding certain typical relations with the animal as a specific individual, the corpuscles have a uniform type-character of their own.*

Both Kölliker* and Wharton Jones,† as above cited, speak of having examined the blood of mammalian embryos. But it appears to me that they have overlooked the fact of the necessity of such examinations being made before any direct connection with the mother had taken place. Thus, Kölliker examined the human embryo of three months, and Jones, an embryo ox of an inch and a quarter long. This can well account for their finding several kinds of corpuscles; as, 1st, the corpuscles of the adult; 2d, coloured nucleated cells; and 3d, uncoloured nucleated (epithelial) cells;—in fact, a mixture of adult with embryonic blood. Moreover, the "granule cells," those both coarsely and finely so, which Jones alleges he found, appear to me to have been only modified epithelial cells in an uncoloured state; or perhaps the cell-products of some of the vascular sanguineous glands, already spoken of.

Thus far, our attention has been directed to the blood of one division of the animal kingdom—the *Vertebrata*. In the other division, however, quite another class of phenomena is met with. As has before been stated, the coloured blood-cell belongs to the vertebrates only. It is with them a special formation, and has a broad physiological signification when viewed in connection with their respiratory apparatus. But in the *Invertebrata*, the anatomical peculiarities do not demand it, as we shall hereafter learn when discussing this point. The blood or rather nutritive liquid of invertebrates has but one kind of cells. These are granular, and closely resemble the chyle-corpuscles of the vertebrates, and of which they can properly be regarded as the analogues. This is so with the classes *Articulata* and *Mollusca;* but in the *Radiata*, this liquid has even a less organized character, for with them it is only the food of the animal in a triturated and partially digested condition, and contains no special corpuscles.

Wagner‡ first suggested the idea that with the Articulates and

* Kölliker; *loc. citat.*

† Jones; Phil. Transact. 1846, pp. 63–101.

‡ Wagner; Physiology, translated by R. Willis, p. 278.

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Mollusks, the blood was *chyle in circulation*, and nothing more. Recently, Prof. Agassiz has taken up this subject, and extended it throughout the animal kingdom. Without discussing the point here, I will only say, that he divides the circulating nutrient liquid into three kinds, viz: *chyme*, *chyle*, and *true blood*. The first belongs to the *Radiata;* the second to the *Mollusca* and *Articulata;* and the third to the *Vertebrata*. This view has much the semblance of truth, and is borne out by many well-ascertained facts. I shall here adopt it provisionally, omitting the consideration of some intricate questions which it involves. If we examine the blood of any of the Articulates or Mollusks which have a distinct circulatory system, we find in it many irregular, granulated, colourless corpuscles, which have some variation of size. These are the only corpuscles it contains, and have been called the "white blood-cells." Anatomically speaking, they consist of a pale plasmatic membrane, or, more properly, involucrum, surrounding a number of granules, in the centre of which last you can, in some, perceive a nucleus. Their shape is generally spherical, but many other roundish shapes are seen. Great numbers of the same kind of granules in a free condition are floating in the liquid. I think it questionable if many of these bodies have a true membrane, or, in fact any covering except one of a simple plasmatic character, such as any group of granules floating in a formative plasma would gather about them. There are others, however, quite small, which have a true membrane. As to their genesis, it appears to me, from the repeated observations I have made, that it takes place exactly like that of the chyle-corpuscle among vertebrates—by the expansion of a primordial utricle into a vesicle, inside of which are collected granules. That this is so, would appear from the fact that in the field may be seen numerous pale vesicles, having a few small granules, and which are apparently these corpuscles in a developing state. After these vesicles have increased by expansion, the granules become more numerous; they may remain in a scattered condition in the cell; or they may collect in the centre, and, by a kind of condensation, form a nucleus, as is sometimes seen. The corpuscles thus formed are colourless, throughout these two classes; in the Annelids, which have red blood, this colour is not due to the corpuscles, but to the liquid in which they float.

In a genetic point of view, the blood of this division, thus simply constituted, has no other peculiarities worthy of mention. The

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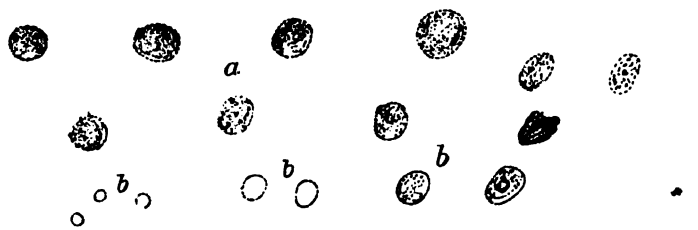
anatomical questions as to the relations of their digestive with their circulatory systems, cannot be properly discussed here.

With this description, I close that which relates to the genesis of normal cells, which exist in the economy, either in an entirely free and independent condition, or are never blended into tissues.

Fig. 12.



The blood of Invertebrates, taken from *Melolontha subspinosa*, consisting of only one kind of corpuscle, colourless, and this resembling the chyle-corpuscle of Vertebrates. *a*. Corpuscles filled with granules, some of them nucleated indistinctly. *b*. Their formation from vesicles, as in the case of the chyle-corpuscles. Although the above is a representation of the blood of insects, yet the same is seen in the other classes, the *Arachnida*, *Mollusca*, and *Crustacea*.

We now come upon the consideration of the genesis of another class of cells, viz: those which exist in connection with, or united into, continuous tissues—of which they form the elements. This of course includes all of the other special tissues of the animal economy. Such are the *Muscular*, the *Nervous*, the *Fibrous*, the *Cartilaginous*, the *Osseous*, and the *Adipose* tissues.*

III. Nerve-Cells and Nerve-Tissue.

The nervous tissue is the noblest of all in the economy, and, more than any other, has a uniformity of structure throughout the entire animal kingdom. It is composed of two parts, an *active* one, consisting of free cells, and a *passive* one, the agent of the former, composed of tubes, and which may be regarded as the metamorphosed products of the cells. A consideration of the phases of genesis of this tissue, in both its simple and compound states, necessarily involves many points of discussion, which, otherwise situated, would scarcely belong to this work.

As in former cases, we shall, by learning some of the historical relations of the subject, be enabled all the better to comprehend its de-

* The epithelial cell has already been considered: therefore, those tissues which are formed by its metamorphosis, in connection with fibrous and other tissues, cannot again come up for consideration. Such are *Mucous* and *Serous Membranes*, the *Skin and its appendages*, and the *Teeth*.

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tails; I propose, therefore, a brief review of the labours and opinions of those who have heretofore studied this subject.

I know of no tissue which has received so much attention as that of the nervous system. This may have been due to the fact that anatomists, recognizing its general relations to the higher conditions of life, have sought a thorough understanding of these last, by the most careful study of its elements.

Then, again, I think it is true that most good working physiologists have a preference in the study of tissues; and, if I may thus express myself, it has been the good fortune of the nervous tissue, that so many excellent investigators should have had for it such a predilection. As will soon be seen, it engaged the attention of many of the most laborious and trustworthy observers, and this too, at that period when new improvements in means of observation were getting into use, and when, also, quite as much care was exercised in observing, as at the present day.

As is true of nearly all the other tissues, we must, in this case, refer to Leeuwenhoek,* as the one who first described *the elements* of which it is composed. This ancient microscopist has described not only the *fibrous*, but also the *cellular* structure of this tissue. Our astonishment is that, in his day, one should be able to recognize so exactly these parts, and make their proper distinctions. But the instruments of his day did not allow him to perceive any of the *details* of these elements. This statement obtains also for the succeeding hundred years; and although Ledermuller† studied it, he did not make much progress beyond what was then already known. It is true that Prochaska,‡ Fontana,§ and others, have left many descriptions of what they observed in studies of this kind; but, although they may have prepared the way for subsequent discoveries, they certainly laboured under some optical illusions, besides mistaking abnormal for normal appearances. It was the opinion of Fontana, that the nerve-fibres were tubes, in which there existed an actual circulation.

But it was not until after the commencement of the present century that observations were made possessing much importance. In 1816, Treviranus‖ made some advances; and, although he was in-

* Leeuwenhoek; Opera, tom ii. p. 309.

† Ledermuller; Microscopische Gemuths-und Augenergoetzungen, Nuremberg, 1768, p. 63.

‡ Proschaska; Structura Nervorum, Vienna, 1779, p. 68.

§ Fontana; Traite sur le Venin de la Vipère, Florence, 1782, tom. ii. p. 202.

‖ Treviranus; Vermischte-Schriften, Göttingen, 1816, p. 128.

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clined to adopt the views of Fontana, as to the anatomical relations, he rejected the idea of a circulation. Moreover, I think he was the first to declare that the cellular and fibrous parts have the same elementary constitution.

As yet, the difficult points remained to be settled, and I cannot conceive that, with the instruments then in use, this could be done satisfactorily.

About the year 1830, a new impulse was given for researches of this character, by the improvements in microscopes; and, as Henle* justly remarks, it led to nearly as many errors as truths on all these subjects; but they were instructive errors, and, as we shall soon perceive, were the precursors to finally correct opinions.

In 1833, Ehrenberg† took up the study of the cortical substance of the brain, and recognized, beside the nerve-cells and tubes, what appeared to be a third element, apparently consisting of rows of transparent globules, united by a delicate film. These are the now well-recognized varicose tubes, their beaded aspect being undoubtedly due to pressure, the very slightest of which is sufficient to produce it upon their filmy walls. This gave rise to a lengthy discussion, as to the nature and character of these beaded tubes—and there can be no wonder that the whole should still have remained so unsettled, when we consider that the appearance being artificial, each observer was likely to meet with dissimilar phenomena.‡

Treviranus§ was the first to declare positively that these appearances were due to an artificial condition. In this, he was well supported by Valentin‖ and others; and subsequently, Müller, Remak, and others renounced their old and opposite view for this.

The discovery which Valentin¶ soon after made, of the delicate membranous envelop of the nerve-tubes, tended to give clear ideas of the real nature of the above-mentioned phenomena. For, in the presence of this membrane, could be traced the various beaded appearances. The work of Remak,** which appeared soon after, and

* Henle; Traité de l'Anat. générale. Trad. par Jourdain. Paris, 1843, tom. ii. p. 340.

† Ehrenberg; Poggendorf's Annalen, xxviii. p. 451.

‡ Among those who participated in this discussion may be mentioned the names of Müller, Volkmann, and Remak.

§ Treviranus; Beitraege zur Aufklaerung, &c. &c. Bremen, 1835–7, ii. p. 25.

‖ Valentin; Ueber der Verlauf und Enden der Nerven, Bonn, 1836.

¶ Valentin; *loc. citat.*

** Remak; Observationes anatomicæ et microscopicæ de Systematis nervosi Structura, Berlin, 1838.

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which is justly entitled to the name of comprehensive, opened a new field of discussion from the peculiarity of some views there entertained. Remak thought that the nerve-tubes or fibres consisted of three portions: 1. Of an exterior envelop of cellular tissue, which, by its local increase here and there gave the knotted aspect to the whole; 2. A delicate membranous tube, lining the first; and 3. A broad flat band lying within the last. This view was immediately attacked by Valentin;* and soon after, Schwann,† in his great work, pointed out its true meaning, and fully showed wherein lay the error. But he did this by falling back upon his cell-doctrines, and seeking its true explanation in the genesis of those parts from pre-existing nucleated cells. In fact, I think it would have been difficult to have reached the point in any other manner. But, notwithstanding what Schwann accomplished—clearly recognizing many of the relations of the contents of the tube, yet the whole was not yet fully explained, and some phenomena were often presented apparently not reconcilable with the above view. It was perhaps on this account that, about the same time (that is, of Remak's work), Purkinje‡ expressed a still different view. He thought that the tubes had three primitive elements: 1. An exterior sheath; 2. A soft medullary mass, and 3. A central portion, transparent, and which he termed "*axis cylinder.*"

If I correctly understand the matter, this *axis cylinder* of Purkinje corresponds in some respects with the *flat central band* of Remak. But I must confess, with Henle,§ that this point is far from being clear.

But the subsequent researches of Henle‖ have made this matter pretty distinct, for he has pointed out the cause of the error. The conclusion of this matter is this: The nerve-tube, when examined in a very fresh state, is found to consist of a sheath inclosing *liquid* contents of a pellucid aspect. In this liquid, however, changes soon occur. A kind of coagulation of it is perceived; and that portion of it next to the sheath becomes of a white homogeneous aspect, constituting the "white substance of Schwann." But the central portion remains clear, and has the aspect of a nearly transparent axis. This is the *axis cylinder* of Purkinje. When the nerve-tube is subjected

* Valentin; Repertorium, 1838, p. 78.

† Schwann; *loc. citat.* p. 174.

‡ Purkinje; Bericht ueber die Versammlung in Prag, 1838, p. 177.

§ Henle; Traité d'Anat. génér. &c. tom. ii. p. 344.

‖ Henle; *loc. citat.* tom. ii. chap. xii.

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to even very light pressure, this last is forced out, and assumes a variety of globular and varicose appearances.

As to the real nature of these phenomena, I shall express an opinion when giving an account of my own observations.

Those discussions, therefore, which seemed to be required in order to get at the truth in these subjects, may be said, as far at least as relates to the nerve-fibres, to have ended some years since; and this was perhaps with the labours of Henle and Valentin.

It now remains, before closing this historical sketch, to notice the progressive condition of the other element of this tissue in an enlarged state—the *ganglionic vesicles* or *cells*.

The existence of these bodies did not, on account of their size, escape the attention of the older anatomists who used the microscope upon the nervous tissue; but their nature or use was not at all understood. And although Ehrenberg* had noticed and figured them, yet the first good description of them was given by Valentin.† He not only described them, but noticed their caudated prolongations, and alluded to their similarity of form to that of the egg.‡ Subsequently, Schwann pointed out their cell-relations; and since then they have not been a matter of dispute, excepting as to their anatomical relations or connections with the nerve-tubes. That some direct connection of such kind did exist, was first inferred from their caudated prolongations, which seemed to be very convenient to attach nerve-tubes. This was first *suggested* by Remak,§ and since then, other observers|| have advocated the same view. Still later, Kölliker¶ has advanced the same view, stating that in the lower animals each of these vesicles may give rise to *more* than one tube, but that in the higher, a single efferent fibre is the rule.

This is a point to which I have given some attention, and in closing the historical review of this tissue, it may be well to allude to my own views of the matter.

Some time since, I made a series of examinations of the human brain, and that of other animals, to ascertain for myself the real nature of this point. I found *no* appearances justifying the opinion that a *direct connection* ever exists between the ganglion vesicles and

* Ehrenberg; Poggendorf's Annalen, 1833, xxviii. p. 458.

† Valentin; *loc. citat.* 1836, p. 77.

‡ Valentin; Nov. Act. Acad. Leopold. xviii. p. 196.

§ Remak; Observ. Anat. Microsc. de Syst. Nerv. Struct., Berlin, 1838.

|| Will, Todd and Bowman, Kölliker, &c. &c.

¶ Kölliker; Siebold and Kölliker's Zeitschrift, 1849.

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the nerve-tubes, either by the latter joining on to the nucleus or to the caudations of the former.*

I will add, that such is also the conclusion at which Professor Wyman,† of Cambridge, has arrived, from his researches into the nervous system of the Batrachians, and in which the above point received a special consideration.‡

Such are some of the historical relations of the nervous tissue, by the keeping in mind of which we shall the better understand its present condition, both as regards structure and function.

At present, we recognize this tissue as consisting of two elements —nerve-cells (including, of course, ganglionic vesicles), and nerve-tubes. The former is the true histological element, and out of which the latter is only a metamorphosed form. The genesis of each, according to my own observations, now engages our attention.

The formation of the nerve-cells occurs quite early in fœtal life. At first, the whole tissue, whatever is to be its subsequent condition, consists of cells. These are nucleated, and have an exceedingly delicate membrane, inside of which there are generally fine granules. In Mammalia, they are of about 1-2000 of an inch in diameter. Whether they are the vitelline cells metamorphosed, or whether they are new formations from the beginning, out of these last, my own observations do not allow me a decided opinion, for I have never been able to examine sufficiently young embryos in a satisfactory manner.

I am inclined, however, with Valentin,§ to the latter opinion.‖

* It is true, that I have not repeated the observations of Wagner upon cartilaginous fishes; and from the full statements of so excellent an observer as Kölliker in the affirmative, the question is indeed an open one; see his Mikroscop. Anat. Bd. ii. p. 390, *et seq.*

† Anatomy of the Nervous System of Rana pipiens.—Smithsonian Contributions to Knowledge, Washington, 1853.

‡ For additional writings on the relation of the ganglionic globules to the nerve-tubes, see especially Hannover (Recherch. microsc. sur le Syst. Nerv. 1844, p. 69, pl. viii. where he has described and figured them from Helix and Limax); Helmholtz (De fabric. Syst. nerv. evertebrat. p. 10), and Will (Müller's Arch. 1844, p. 76). Hannover is quite in favour of the view of their direct material connection. See also Müller's Physiology, Jourdan's French Translat. Littré's Edit. Paris, 1851, vol. i. p. 560, where the direct connection of the ganglionic globules and the fibres is urged with considerable weight. See also a review by me, of Prof. Wyman's work above quoted, in Silliman's Journal, Sept. 1853, where this subject is touched upon, and its historical relations given.

§ Valentin; *loc. citat.* Entwickelungsgeschichte, &c. p. 183.

‖ As to the genesis of the ganglionic vesicles or cells, it is the same as that of the common cells. As I have before remarked, they are these last in a hypertrophied condition. This opinion is founded upon the fact that I have traced them, in point of

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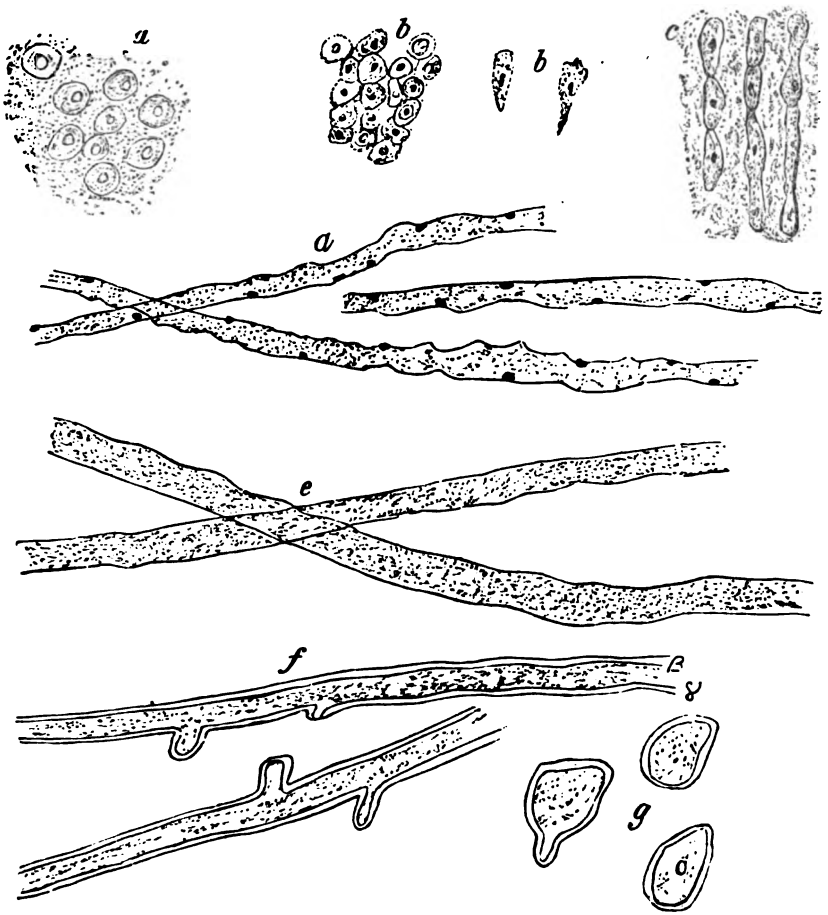
These primitive nerve-cells have a remarkably uniform aspect, and are generally situated in a light gray, punctiform blastema. In the

Fig. 18.



Nerve-cells; their transition into tubes, and the appearance of the nerve-tubes of the cerebro-spinal system. The first three figures from brain and spinal cord of fœtal goat of 14 days; the remaining ones from nervous system of tadpole. *a*. Nerve-cells from brain of fœtal goat, their formation occurring according to "my own mode." *b*. The same, from spinal cord, beginning to arrange themselves in a linear manner; *b's* caudate cells. *c*. Still farther arranged, and tubes appearing. *d*. Tubes formed; the nuclei of the old cells remaining in the walls. *e*. A nerve-tube from a spinal nerve, without double contour. *f*. Nerve-tubes from same, with double contour. *β*. Axis-cylinder. *δ*. White substance of Schwann. *g*. Globular and cell-like forms assumed by expressed portions of axis cylinder.

embryo of a goat, half an inch in length and fifteen days old, these cells had already formed in that part which was to be the

size, all the way from the former to the largest of the latter. But as to the point whether *any* cell may attain this development, or whether it is the peculiar destiny of a *few*, I cannot say.

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cerebrum. In the spinal cord, they had just passed on to the condition of fibres; still, they had left their marks behind, and it was easy to perceive the phases of the change. It would appear that the first step of this fibre-change is a partial arrangement of these primitive cells in a line or series. Then their delicate walls coalesce, and a continuous tube is formed, the nuclei of the component cells still persisting. At first, this tube has a most delicate appearance, but afterwards it may assume a more opaque aspect by the appearance of granules on the wall.

I have said that this tube is formed by the coalescing of the walls, but, as far as my own observation goes, this is a point which has not been demonstrated. On many accounts, it is a matter of just inference; but I have often thought that it might well be formed by a plasmatic membrane, enveloping a row of cells lying contiguous, or nearly so, to each other. At any rate, the persistence of the nuclei in their walls, for a brief period after their formation, shows their cell-origin. It would appear that, subsequently, these nuclei disappear for the most part, and there is then left a delicate tube with transparent contents. The double contour of this tube is due without doubt to the portion of the contained liquid lying next to the sheath, becoming more opaque than the rest, producing the "white substance of Schwann." When the liquid contents of these tubes escape, they very naturally assume a globular form, and their exterior portion in contact with the surrounding liquid in which they float becomes coagulated, and the whole retains its original form. Afterwards, a coagulation of that portion of the liquid of these globules lying next to the periphery may occur, and so these globules may have a double contour as well as the nerve-tubes themselves.

The diameter of these primitive tubes, thus formed, is probably about that of the primitive cells; but after manipulation, however careful one may be, there appears to be a deviation from their original aspect. The fluid contents of the tubes escape either partially or wholly—in the former case, giving it a very irregular appearance, and in the latter, allowing the membranous sides to lie against each other, giving it more the aspect of a delicate thread. But I cannot think that these last delicate thread-like fibres are ever formed as such from the beginning, or, as Remak has supposed, that they are the "*earlier* forms" of the larger fibres. The notions entertained by some of the earlier microscopists, that these tubes formed a circulating system, arose from the globular forms which the contents of these tubes often assumed, exactly as would occur if they had

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made their escape. I scarcely need say, therefore, that all such appearances are artificial.

As I have already remarked, it was the opinion of Valentin, formerly, that these delicate tubes are the sheaths proper of other tubes of a more opaque character. This opinion has since been clearly shown to be erroneous, and I may add, that what I have recently seen, in my own observations, is to the same effect. Thus, in the gradual development of the nervous system in the tadpole, you have first the primitive cells, and then, in the midst of these, and by changes just described, appear these delicate tubes, without the intervention of any other.

Such are the phases of genesis of the primitive nerve-cells, and their transformation into the white nervous fibres, as I have traced them in fœtal life.

As for another set of fibres, found and described by Remak—the gray or *so-called organic* ones—they need not here detain us long. In showing what they are I cannot do better than to quote the words of Remak,* who was among the first to study them. He says: "They are not tubular, that is, surrounded with a sheath, but naked, being transparent, almost gelatinous, and much more minute than most of the primitive tubes. They almost always exhibit longitudinal lines upon their surface, and readily separate into very minute fibres. In their course, they are frequently furnished with oval nodules, and covered with certain small oval or round, more rarely irregular, corpuscles, which exhibit one or more nuclei, and in size almost equal the nuclei of the ganglion globules."

The opinion of Müller,† who followed Remak, is to the same effect. With such a description, they would certainly seem to be entitled to the name of a distinct set of fibres. Moreover, they are found in the nervous system of organic life in the ganglia. But within a few years doubts have been raised as to the nature of these fibres as thus described; and there are good reasons for now believing that they are rather a form of simple fibrous tissue than true fibres of nerve. I do not propose to discuss the matter here, for I have not investigated to that extent, some of its points, that I should wish to do. But, as yet, my opinion coincides with that of Henle, who regards the common gray nerves as only solid cords of nervous fibres like those

* Remak; *loc. citat.* 1838, p. 5.

† Müller; Physiologie du Système Nerveux, &c. trad. par Jourdain, Paris, 1846, tom. i. p. 126.

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belonging to animal life, and therefore that the fibres noticed by Remak and others belong to a different tissue. Such, at least, is what I should deduce from my own limited observations; and as such, the genesis of these bundles of primitive fibres would be the same as that of the same fibres in an isolated condition.

But besides the *true nerve-fibres* (if I may thus designate them) and those *reputed ones* of Remak and Müller, there exists another set, found in the ganglia of the sympathetic system; but they are not peculiar to it alone, for they also occur, but more rarely, in the cerebro-spinal tissue. They are distinguished from the first by their smaller size (being about one-third), and their even, smooth appearance, having all the aspect of a small tube filled with homogeneous minutely granular matter; moreover, they want the double contour of the former. These may be called the *organic nerves*, and this in contradistinction to those fibres of Remak, or those bundles of *ordinary nerve*-fibres which apparently constitute the gray nerves, and which are only a conventional aggregation of the common nerve-tubes.*

The genesis of these *organic nerves* takes place, according to my own observations, in very much the same way as that of the others we have just described. There is this difference, however, that in the common nerve-fibres there appears to be a kind of liquefaction or clearing up of the contents of the cells after they have united to form the tube; and this produces that glass-like transparency for which they are noted. But in the organic fibres, the cell-contents remain,

* A few years since, the question whether or not there really existed a set of fibres peculiar to the sympathetic system, was discussed by some of the continental observers. The conclusion at which they arrived is as I have stated above, viz., that the *gelatinous* fibres of Remak and Müller are not nerve-fibres, but that the nerve-fibres peculiar to the sympathetic system, or nearly so, are such as those of which I have just given the description.

See Kölliker, Die Selbstandigkeit und Abhandigkeit des Sympathischen Nerven-systems, an abstract of which is in Mr. Paget's Report on Anat. and Physiology, Brit. and For. Med. Rev. July, 1846.

After all, there is some obscurity about the matter, if we take Remak's statement; for he says that the fibres described by him are much smaller than the primitive nerve-tubes. In this respect they would correspond to the *true organic fibres* I have just described. But in all other particulars they do not agree. Is it not probable that Remak is mistaken in his idea of their size?

For some special details on this subject, see Kölliker, Mikroskop. Anat. ii. p. 523, and Müller's Physiology, transl. by Jourdain, 2d ed. by Littre, Paris, 1851, vol. i. p. 563.

This is a point which studies on the lower animals will tend much to elucidate; and see the writings of Leydig in Siebold and Kölliker's Zeitsch. f. Wissensch. Zool. *passim*.

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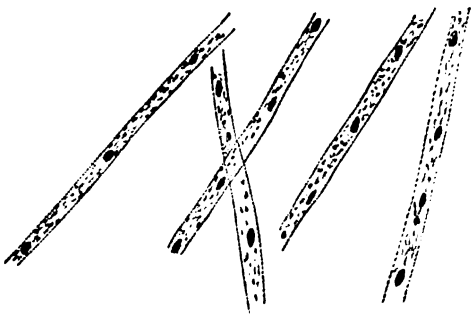
the nuclei are partially persistent, and therefore the tube presents a grayish-white homogeneous aspect.

The figures which Schwann has given correspond quite closely with what I have observed.

Such are the more general genetic relations of the nervous tissue in both its elementary and compound conditions. As I have before remarked, the greatest uniformity throughout the animal kingdom exists. It is true that among some of the invertebrates the nerve-tubes may not have the same aspect, but homologically they are the same, arising from cells which everywhere have not only an identity of function, but of aspect also.*

Fig. 14.



Gray or gelatinous nerve-tubes, from sympathetic system of a snake (*Heterodon Platyrrhinos*). The presence of nuclei in the tubes shows clearly their cell-origin. Compare the figures I have given with those of Schwann.† (Magnified 350 diameters.)

IV. Muscle-cells and Muscular Tissue.

This tissue has a universality second only to the one we have just been considering, and like it, also, has an homologous structure wherever occurring. But, besides these similarities, there is another, an histological one, and which did not escape the penetrating mind of Schwann. So that it may be said that the muscular and nervous tissues resemble each other in nearly every point of view more closely than any others in the economy. They are properly, therefore, considered in succession.

But we will first refer to its history and criticism, which will enable us the better to appreciate some of its important bearings.

* With some of the Acalephæ, the nervous tissue consists of cells only, and instead of nerve-tubes there are beaded rows of cells—an illustrative histological point. See Agassiz, Mem. Amer. Acad. iv. 1850, p. 232.

† *Loc. cit.* Syd. Soc. ed. pl. iv. fig. 8.

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And in so doing, I cannot pretend to take up a retrospective view of it in all its common histological relations, for these would be of but little interest. But its history and criticism, as a *cell-tissue*, is what concerns us. This, of course, does not date back farther than the time of those numerous and valuable histological researches which just preceded the ever-memorable labours of Schwann. Krause* recognized that the primitive fibres were composed of a series of globules. The nature of these, however, he did not fully understand, and the same is true of other investigators of that time; but as clear ideas of its essential structure could only be founded upon a knowledge of its developments, Valentin† appears to have been the first who studied this tissue in an intelligent manner. He recognized the primitive fibre-cells in the ovum, and their successive metamorphoses until the formation of the fibre itself; also the arrangement of the granules, until, he says, they appeared like a string of pearls. In fact, Valentin appears to have proceeded as far in these studies as he well could, considering that he did not then recognize the grand laws of cells which were soon to be brought out by Schwann.

Schwann combined the labours of those who preceded him with his own, and thereby the force of the problem of the genesis of a tissue which remains so short a time in its cell condition, was solved. He pointed out its *necessary* cell-origin, and the phases which, from this last fact, it would pursue. These facts could not be better established, and I am not aware that any advance has since been made.‡ The researches soon to follow could be little else than confirmatory of these first, or such as would relate to its function and mode of operation.

This last (the *modus operandi*) required studies as to its granular

* Krause; Handbuch der Menschlichen Anatomie, Hanover, 1833, p. 57. Another edition of this appeared in 1841.

† Valentin; *loc. citat.* Entwickelungsgeschichte, p. 268.

‡ It may be well to allude to the opinions of Valentin on the genesis of this tissue, and which have been published subsequent to the work quoted above. He thinks that in the interior of the tube produced by the fusion of the cells, there is a central cavity, which remains throughout life. This cavity at first contains the nuclei of the cells inclosed, but after the absorption of these, it is filled with a gelatinous liquid. Between this central cavity and the wall of the tube, are developed the fibrillæ around the nuclei of the cells (what cells?), their increase in number going on at the expense of the cavity. These fibrillæ at first are pellucid, but afterwards become granular, and suddenly assume a striated aspect.

Vide Berlin Encyclop. vol. xxiv. 1840, p. 212; and Müller's Archiv, 1840, p. 204.

I scarcely need say that Bowman's results have shown the erroneousness of this view.

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constituency, or the arrangement of the granules in the interior of these primitive cells thus coalesced, giving rise to the striated aspect. In 1840, Mr. Bowman* published his comprehensive memoir, in which the whole subject was most ably treated, and the paper has since been one of reference in this and other languages. Bowman showed that the elementary structure of the muscle is such, that it is not only capable of being divided lengthwise, thereby forming threads, but also crosswise, forming disks—thus showing that it is composed of particles of equal size, united and arranged in two directions, each one standing equidistant from the surrounding neighbouring ones. This view, sufficiently substantiated by the care of the author's (Mr. Bowman's) own researches, has been since confirmed by many good Microscopists. But a corresponding view which it involved, has not been so satisfactorily made out. This was the view that each of these particles was a cell, and therefore that the whole muscle was only a bundle of small cells, arranged after a regular manner. This view was advanced by Drs. Carpenter† and Sharpey,‡ and is based upon appearances they have witnessed upon prepared specimens. As I have just stated, this point has not been equally clear to others; and, in my opinion, as I shall soon show, it is rather a forced interpretation of appearances.

When a subject had been so thoroughly wrought out, as this had, in its earlier years, it cannot be strange that recent times have not much advanced its condition. And so, since the memoir of Bowman, I do not think that much has been done to change the face of the matter. To be sure, much has been written upon debatable points, and to show that optical illusions have often been mistaken for real appearances.

The *Comp. Rendus*, of 1847, contains several memoirs upon this subject, and especially that of Prevost, in which the genesis of this tissue, with many peculiarities, is traced over a wide range of material.

But, by far the most comprehensive work in this direction, of late years, and in which the finer and more delicate points are wrought out with the improved means we now possess, is the memoir of Le-

* Bowman; Philosoph. Transact. 1840.

† Carpenter; A Manual, or Elements of Physiology, &c. London, 1846. Also, his other more recent physiological works.

‡ Sharpey; Human Anatomy. By R. Quain. Edited by Wm. Sharpey. Second edition, London, 1846.

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bert.* And I am happy in saying that subsequent investigations of my own have elicited the same results; and I place the more reliance upon these, since drawings which I made, before the work of Lebert was accessible to me, correspond quite closely with these last.

But however accurate and careful may have been the researches of Lebert, they have a conformatory, rather than an original value, and may be regarded as bringing to the test and showing the truth of the earlier opinions of Valentin and Schwann.†

What these were, may be learned from a description of what I have wrought out for myself, and which will now engage our attention.

If we confined our range of study of this tissue to the higher animals, in which it can scarcely be said to exist at all, except in an ulterior condition, our knowledge of it would be imperfect. We shall do well, therefore, to commence with the lower forms in which it persists in its very earliest stages, that of eels, and as such performs its functions. We find it in this condition in some of the Radiata. Such is the muscular tissue of the pedicle of the attached medusa-form of *Tubullaria*, and, when examined, will be found to consist of cells arranged in a linear series. When the pedicle is in a state of relaxation, these cells are spherical, and are filled with granules. The contraction results from simultaneously becoming flattened, or disk-shaped—their long diameter being at right angles with the way of contraction.

Here you have motion due to the direct agency of cells; in other words, the simplest form of active muscular tissue which is of *cell-origin*. But a muscular or motor tissue may be even more simple than this. In the instance just cited, we have seen that the cells were filled with granules; and I am convinced that to move-

* Lebert; Annal. des Sci. Nat. 1849, et Mai, 1850.

† The following are the names of some papers on the genesis and intimate character of the muscular system, which have appeared within a few years:—

Stannius; Müller's Archiv, 1847, p. 440.

Cramer; Guy's Hospital Reports, vol. vi. part i.

Dobie; Annals of Nat. History, Feb. 1849.

Lebert; Comp. Rendus de la Soc. de Biologie. Paris, 1849, p. 76.

For the Histology, Anatomy, &c. of the Muscular System in all the classes of the animal kingdom, with many interesting details, see Comparative Anatomy by Siebold and Stannius, trans. &c. by Burnett, under the head Muscular System of each class.

See, also, several anatomical and physiological text-books, especially those of Carpenter, Sharpey's edition of Quain, Kirkes and Paget, and Jourdain's translation into French of Müller.

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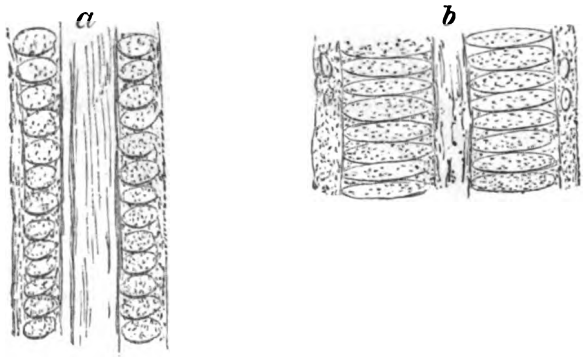
ments of these last is due the change of form in the cell, producing contraction. By a kind of mutual attraction which they take on, they become more closely aggregated; a decrease of space is the result, and the cell-membrane containing them follows their movements, and at the same time brings their lever to produce the secondary results. It is therefore evident that the movements of these granules do not depend upon their being enveloped by a membrane, although the possession of this may increase the definite character of the motion.

Fig. 15.



The *cell*-muscle of the pedicle of the attached medusa-form of *Tubullaria* (nov. sp.), showing the contraction due to cells. *a*. Natural relaxed state. *b*. The state of contraction by flattening of cells.

This even more simple form of tissue to which I have alluded, is found in many Bryozoa, and in the integument of many Entozoa, and especially of the class Nematoidea. It consists of a collection of granules, which are not allocated by an investment of cell-membrane.

In the genus *Alcyonella* of the Bryozoa, if the muscle which moves one of the tentacles be examined, it will be found of a fusiform shape, and consisting only of a sac of that form, containing these primitive granules; and, as I have repeatedly watched it, the contraction takes place sequent upon a mutual attraction of these granules, by which they occupy a less space.

We can conceive of no motion simpler than this, and this may be taken as the ultimate expression of muscular movement, divested of those levers and other means for the increase of power which belong to the compound forms of this tissue, giving it, in fact, its peculiar composition. But let it be remembered that *in this mutual attraction of these primitive granules lies all muscular contraction, whether it be in the lowest or highest forms.*

As we advance into higher forms, we perceive that not only has

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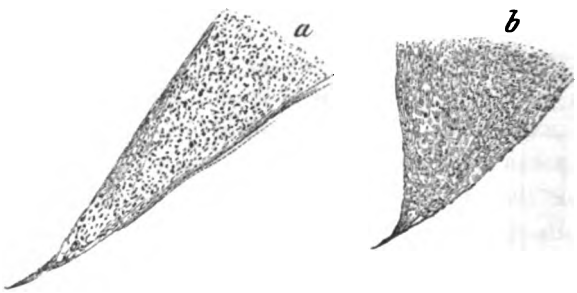
this tissue advanced to the condition of being definitely arranged and allocated by cell-membranes, but these have coalesced so as to form tubes, as is the case with the muscles of *organic life;* but in the muscles of *animal life*, in which this tissue has reached its highest stages of metamorphosis, there is a step in advance of this; for, not only have the cells coalesced into tubes, but their contents, the granules, have become definitely arranged, giving rise to the striated aspect.

Fig. 16.



The tentacular muscle of *Alcyonella* (nov. sp.), being composed of a fusiform sac filled with granules. *a*. Natural or relaxed state. *b*. State of contraction.

We will now look to the phases of this muscle formation. In the young embryo, nucleated cells are seen in those localities where the muscular tissue is to appear. I think, with Valentin, that these are formed from the vitelline substance, and are not metamorphosed vitelline cells; at any rate, they are not easily distinguished from those about them which are to form other tissues, if we except their little larger size. Their first change towards a condition of organization above that of cells, consists in their tendency to be arranged in a linear series, or rows. There then appear rows of nucleated cells, having granular contents; next, these cells coalesce and form tubes, at the same time their contents becoming more thoroughly granular. There are, then, at this stage, tubes with homogeneous granular contents, with the nuclei of the component cells still persistent. It is on this stage of development that the fibres of the *organic* muscles rest, and the above, therefore, constitutes their description.

The development still continuing, these fibres, thus formed, increase in size by interstitial deposition of granular matter; at the same time the nuclei, and other remains of their cell origin, for the most part disappear. Then the granules, thus accumulated, begin to be grouped

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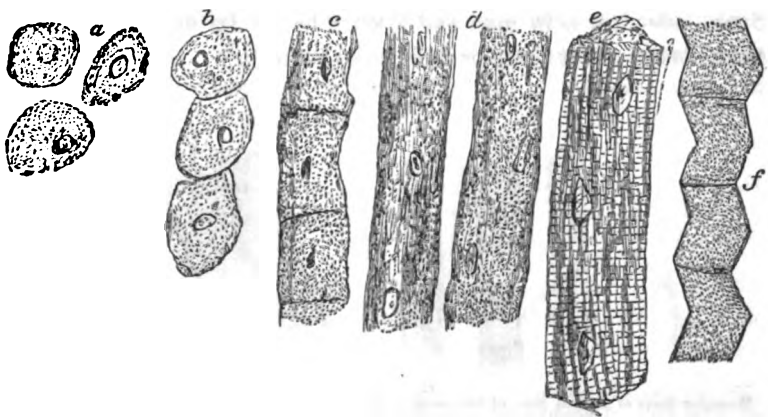
in a regular manner, and when this has been accomplished, the whole presents a very uniform, regular aspect, constituting the true *striated* muscular fibre; but, when fully-formed, it is only in those belonging to the lower animals that any traces of the original cell-structure can be perceived. Certainly, in Mammalia, there is no trace left that can be perceived in the normal state. Still, however highly metamorphosed, this tissue does not lose its original cell-traces. For, in a condition of it produced by defective nutrition, and which may be properly termed *retrograde metamorphosis*, consisting in the unravelling of its structure, the marks of the old cells are seen; exactly, if I may use the comparison, as when a house goes to decay, its finish is stripped off, and you see, what was before hidden, that the rafters and beams have composed its real structure.

There is another appearance which is sometimes seen, and which, as it shows beautifully how the traces of the ancient cells perish, may well be mentioned here. I refer to the plication of the fibre often observed in specimens of boiled beef. It is plicated or folded upon itself. But this plication takes place at the weakest point, which is at the union of the old cells with each other. Therefore, the length of each plication should be that of the primitive muscle-cell; and in a

Fig. 17.



Muscular fibre and its formation; from very young tadpole. *a*. Muscle-cells as they exist in the embryo. *b*. The same, linearly arranged. *c*. Coalescing and forming a cylinder. *d*. Cells completely fused, but nuclei still persistent, the granular matter unarranged constituting the muscular fibre of organic life. *e*. Still farther progressed, the granules definitely arranged forming *striæ*, but nuclei visible here and there, constituting the muscular fibre of animal life. *f*. A plicated muscular fibre of boiled beef, the plication taking place at the junction of the ancient cells. The appearances of muscular fibre of the higher animals, and in which no nuclei are visible, I do not figure, for any one can easily see it. I have given only its histology.

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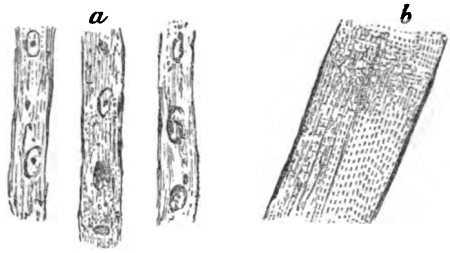
specimen which I recently saw, and made the measurements, I found this to be correct.*

It will be seen by the above description that the muscular *fibre*, and *not* the *fibrilla*, is the true embryological cell-element, the production of the *fibrillæ* being an artificial division, exactly as is the transverse cleavage. Moreover, it will be seen that the beaded character of this *fibrilla* is due to a regular arrangement of granules, rather than to a longitudinal series of cells.

In the formation of the fibre and its development, as its increase takes place by interstitial growth or the internal deposition of granules, it would follow that those of the embryo would be much smaller than those of the adult. That this was really so, was made out by Harting,† some time since, from comparisons he instituted between the fibres of embryos and their adults. Recently, however, I have had occasion to arrive at the same result. The muscular fibres of the *gluteus maximus* of a goat of two months (the natural term is five months) I found to be only one-fourth the size of those of the adult. From the phases of development, as I have just described them, of the two kinds of muscular fibre, viz., *animal* and *organic*, it would follow that the size of the latter should be much less than that of the former; in fact, it ought to be exactly what the former is before the changes for striation take place, or as it is in the embryo. Such, indeed, it is in man and Mammalia, as far as my observation goes—being about three or four times smaller.

Fig. 18.



Muscular fibres of a fœtal goat of two months, from gluteus maximus, showing relative size with that of the adult. *a*. Muscular fibres from gluteus maximus of fœtus. Size, 1-1800. *b*. Muscular fibres from the same of adult. Size, 1-500. (Magnified 350 diameters.)

The fact of the fibres in the adult being so much larger than those in the embryo, may explain how the increase of the muscular tissue,

* Henle gives a figure of the same appearance. *Vide* Traité d'Anat. générale, &c. pl. iv. fig. 4, F.

† Harting; quoted in M. Paget's Report on Anatomy and Physiology. Brit. and For. Med. Rev. April, 1846.

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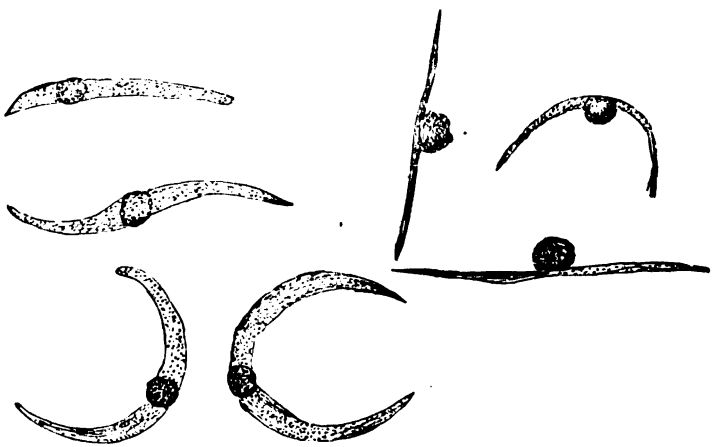
as a whole, takes place, either from the natural growth of the body, or from continued exercise. For, as there is no evidence that muscular tissue is developed *de novo* except in the embryo, the increase in size, for instance of the *biceps* and *triceps* of the arm, after exercise, is due to the *interstitial* growth of fibres, which from disuse have hitherto remained in their embryonic condition.

As the subject of muscular tissue will not again come up in this work, its function being too evident to be separately discussed in a following chapter devoted to *function*, I may properly here notice another point of much interest and importance. I refer to the *dependent* relations existing between the nervous and muscular tissues. As is well known, it was the opinion brought forward by Haller, that the muscular tissue had an inherent contractility, aside from any influence of the nervous system. Subsequently, however, this view was disputed, it being argued that, in the cases in which *detached* portions of this tissue exhibited contractile movements, these were due to the presence of a small bit of nerve in them, and which was the agent of the action. Within a few years, however, and since investigations of this kind have been more minute, and extended to the lower animals, phenomena have been seen justifying the view of Haller, although based upon different and much more scientific data. The nerves, in their distribution in the muscular tissue, do not enter the fibres themselves, but form anastomoses on their surface. If, therefore, isolated fibres can be seen, in the field of the microscope, to contract and relax, it is evident that such

Fig. 19.



Muscular fibre-cells, ad. nat. del., from human spleen.

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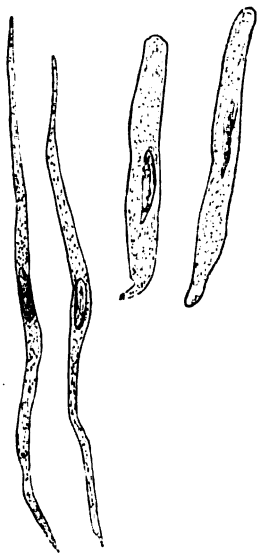
motion cannot be due to nervous influence (for there are no nerves), but rather to the inherent power of the tissue itself.

This was seen some time since by Mr. Bowman,* and more recently by Professor E. Weber;† and still later, I have had the good fortune to observe it several times while making some researches upon the viscera of insects; and, as I had in my memory the observations of the two above-named observers, I watched it carefully, and was quite satisfied that it thus occurred.

Fig. 20.



Muscular fibre-cells from the iris. Copied from Lister's paper.

Moreover, in the muscular tissue of *Tubullaria* above mentioned, the contraction can be distinctly observed to take place through the agency of the cells and their contents; there positively being no nervous filaments whatever entering the organs.

As physiologists, we may with candour ask, why should not simple cells or utricles have this power instead of its being entirely referable to a production of cells in the brain or nervous system? We can as well conceive of a muscle-cell having this power as a brain-cell. It is true, however, that in the more important adaptive motions of life, there is necessarily presupposed a unity of power under the guidance of volition or its equivalent.‡

* Bowman; *loc. cit.* Phil. Trans. 1840.

† Weber; Archiv d'Anat. générale, Janv. 1846.

‡ In the preceding account, I have made no allusion to a peculiar celloid tissue,

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V. Fibre-cells and Fibrous Tissue.

Under this head, I include all those forms of tissue commonly known by the names of *cellular* and *areolar;* for microscopy has wonderfully simplified our knowledge of its characters. It is true that the relations it sustains in the economy are mostly those of a mechanical nature, and on this account it might seem proper that its consideration should be omitted. But then, again, owing to its very simple character, its presence is almost universal, and of all the tissues, it is the one most easily and rapidly reproduced. On a future page, we shall learn that its pathological relations are of the most important character, and for this reason alone, if not for others, it should here be considered in its normal histological conditions.

Functionally, full as much as structurally, it is allied to the muscular tissue last considered, and in some instances it assumes the active conditions of the latter in a very imperfect manner.

I have, therefore, taken it up in this connection. We will first refer to its history and criticism, brief though it may be.

Although Leeuwenhoek, Malpighi, and other of the older anatomists were well acquainted with the common anatomical relations of this tissue, which, from its mechanical importance, they studied well, yet its histological characteristics cannot be said to have been learned until a much later period, and when better means of investigation came into use. This was not before 1833. At about, or soon after,

found in various parts of the body, and now pretty clearly shown to be of a muscular nature. The tissue here alluded to is found in parts possessing contractile properties, but which contain nothing like ordinary muscle. It is made up of long, large, nucleated, queer-looking fibres, the uncouth remains evidently of the characteristic muscle-cells. Their histological and local peculiarities have been pretty thoroughly worked out by Kölliker (Zeitsch. f. wissenschafl. Zool. i. Abth. 1, 1848). He has shown that the muscles hitherto regarded as composed of "smooth fibres," are really made up of elongated cells with an oblong nucleus, which he calls "muscular-fibre cells." He has found them in the nipple and its areola, in the dermis, Crampton's muscle, the digestive canal, the bladder, in a part of the prostate and vagina, the arteries, the veins and lymphatics, the ureter, the urethra, the Fallopian tube, the uterus, the dartos, the vasa deferentia, the trachea and bronchia, the spleen, and many other organs. See Comp. Rend. de la Soc. de Biologie, 1849, p. 156; and art. Spleen, in Todd's Cyclopædia of Anatomy and Physiology. The composition of the iris is of the same fibres. See a paper by Lister, Quarterly Journ. of Micr. Sc. No. I. Oct. 1852. For the comparative histology of the muscular tissue, with many interesting details in every class of the animal kingdom, see Comparative Anatomy by Siebold and Stannius, trans. from the German, &c. &c. by Burnett, under head Muscular System, *passim*.

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this time, a number of microscopists published accounts of its ultimate structure. Among these may be mentioned the names of Krause,* Wagner,† and Valentin.‡ Each and all recognized that it was composed of fibrillæ which were formed by the alteration of cells.

The subsequent labours of Eulenberg§ and Gurlt‖ made these points the more clear. I need not here point out the distinctions and differences they made out. Soon afterward, Schwann gave an individuality to the whole, by referring its genesis necessarily to cells. Its formation, in all its phases, he has pretty clearly indicated; and, although on some minor points he may not have been fully correct (at least, according to more recent researches), yet the general outline of the genesis was fully made out.

He showed the existence of the two kinds of fibres, the yellow and the white; the former existing alone in ligaments, tendons, aponeuroses, &c., the latter in the middle coat of arteries, ligamentum nuchæ, &c.; but both kinds are present in the areolar tissue generally. Since Schwann, the labours of Gerber,¶ Henle,** and Todd and Bowman†† among others, have increased our knowledge of its more minute details, its functional and other characteristics; but I am not aware that they have changed materially its aspect in an histological or genetic point of view.

More recently, however, than these writers, this tissue has been the subject of interesting inquiries, as to its *purely* mechanical relations in the economy, as occurring in some localities; as, for instance, in the *skin* and *dartos*, where it has an irritability and power of contraction. This is undoubtedly due to nervous influence of a reflex character. And it may well be questioned if the fibres thus capable belong to the fibrous tissue, and are not rather a low form of unstriated muscular fibre, being intermediate between the muscular and fibrous tissues.

In my opinion, such is the true state of the case, and it may serve to explain the fact that, in some instances, true muscular fibres

* Krause; *loc. cit.* 1833, vol. i. p. 13.

† Wagner; Lehrbuch der vergleichende Anatomie, 1834, p. 61.

‡ Valentin; Ueber den Verlauf und die Enden der Nerven, Bonn, 1836.

§ Eulenberg; De Tela elastica, Berlin, 1836.

‖ Gurlt; Lehrbuch der Vergleichenden Physiologie des Haussaeugetheire, Berlin, 1837, p. 19.

¶ Gerber; Handbuch der allgemeinen Anatomie, &c. Berne, 1840, p. 134.

** Henle; Traité d' Anat. générale, &c. tom. i. p. 374.

†† Todd and Bowman; Physiological Anatomy.

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have been thus found.* The true fibrous tissue, therefore, has no immediate connection with the nerves, and sustains merely mechanical relations.

We will now refer to its genesis as based upon my own observations. On some account there may be a propriety of dividing this tissue into several varieties as Schwann and Henle have done, but certain it is that it is composed of two elements only, wherever found. These are what have been well designated as the *white* and *yellow* fibres.

The former consist of inelastic bands of a very variable size, and present markings with their long diameter. This lining, however, does not indicate a finer subdivision, for the fibre cannot be split up into fibrillæ.

These fibres, apparently thus composed, present often a wavy aspect; thereby allowing of some extension and retraction. Such is the element of which the tendons, ligaments, aponeuroses, fibrous membranes, &c., not to mention the pathological products, are composed. It is emphatically a cell product, and the phases of formation can be easily watched. First, you have round or oval nucleated cells; these become fusiform, or lengthen at both ends, their liquid contents becoming more opaque. In this state, they become united together in a serial manner, not so much by a coalescence of the contiguous walls as by a joining together through the intervention of a plasmatic blastema. Thus formed together, the nuclei remain for some time, and there is an irregular splitting up of the old cell into finer fibres, though not of a uniform character.

These formative cells not having a proper individuality, at least as to size, the fibres formed from them have a variable dimension; some of the larger ones have a size of 1-500, and the smaller ones of 1-2000 of an inch in diameter. This splitting up of the cell, to which I have just alluded, appears always to take place from its periphery towards its centre; so that, in the process, you first perceive that the elongated cell is bifid or trifid, &c., this extending more or less its whole length. The phases of formation thus described can be traced in the embryo, or perhaps full as well in the pathological products of a fibrous character, in which this tissue persists on its embryonic stage.

The *yellow* fibres may, as Schwann has said, be regarded as distinct in their character and formation. They consist of long smooth fibres, curled upon each other when no extension is made. Their size, like that

* See Comp. Rend. des Séances de la Soc. de Biologie, à Paris, 1850, p. 38.

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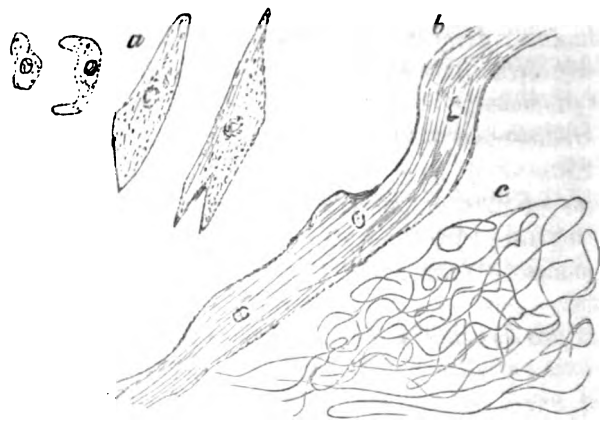
of the other variety, is variable, being between 1-3000 to 1-7000 of an inch in diameter. Their locality is in the middle coat of the arteries,*

Fig. 21.



White fibrous tissue and its formation, from a tendon. *a*. Fibre-cells in their early stages of growth. *b* United into a cylinder and split longitudinally, the nuclei still seen. *c*. A representation of the filament separated from the fibres—their coiling, twisted aspect. (Magnified 250 diameters.)

the chordæ vocales, the ligamentum nuchæ, and the ligamenta subflava, besides being combined with the white fibres in various parts.† The mode of genesis of this tissue has been a point upon which different physiologists have expressed dissimilar opinions. Schwann‡ speaks of having examined the ligamentum nuchæ of a fœtal sheep, which he found composed of longitudinal fibres containing nuclei of cells. He therefore infers their formation by the coalescence of cells, as is true of the other variety. But, according to his own account, his observations were not complete or fully satisfactory. The opinion of Valentin§ is quite different, for he thinks they are formed by the apposition of matter around fibres which are formed by the coalescence of cells.

Henle‖ regards them as formations from the nucleus, or, as he would call them, *nuclear-fibres;* exactly as are formed many fibres found in vegetable tissues. He, however, bases his opinion more upon ana-

* Henle thinks that the elastic tissue of the middle coat of the arteries is different from that of the ligamentum nuchæ, &c. (*Vide* Traité d'Anat. gén. tom. i. p. 440.) But this difference may be a local instead of an histological one.

† For an account of the localities of this tissue, *vide* Eulenberg; *loc. citat.* p. 13.

‡ Schwann; *loc. citat.* p. 151.

§ Valentin; Müller's Archiv, 1840, p. 216.

‖ Henle; Traité d'Anat. générale, &c. tom. i. p. 487.

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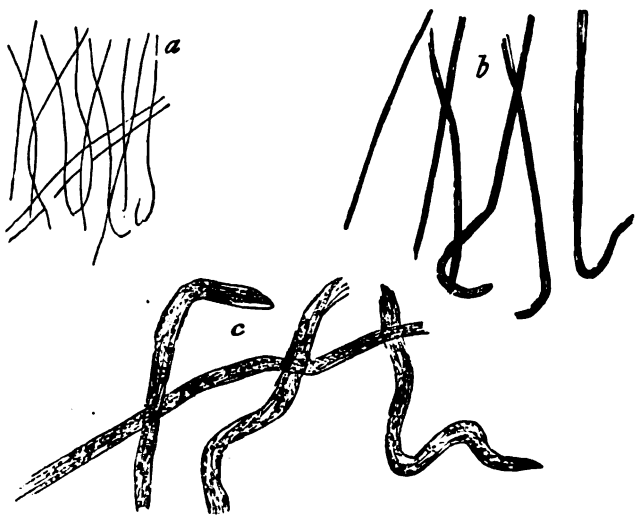
logy than upon real observation. It is true, as he observes, that, because they do not appear affected by acetic acid, and, under any treatment, show no unequivocal nuclei, they resemble nuclear formations. But this does not prove them to be such, although it would certainly seem to show that they are not of direct cell-membranic origin. My own investigations have been made upon the ligamentum nuchæ of fœtal and adult goats.

This tissue seems to make its appearance at a very early period. In a fœtal goat of 21 days, its existence was marked by the appearance of long and delicate fibrillæ having a diameter of 1-25,000 of an inch. These were most probably formed by the linear aggregation of granules or utricles, as I have described on a preceding page. It did not appear to me, therefore, that they had a cell origin, as is true of the white fibrous tissue.

Fig. 22.



Yellow Fibrous Tissue and its Formation. *a.* Fibrillæ from ligamentum nuchæ of a fœtal goat of 21 days; size, 1-25,000. *b.* Same, from the same locality, of a fœtal goat of four months. *c.* Fibrillæ of tissue in its adult form; size, 1-3500.

In a fœtus of the same animal, of four months (that is, one month before birth), this tissue was still of a grayish-white aspect, and the fibres were about 1-9000 of an inch in diameter, having the aspect of threads, and revealing no farther structure after the action of acetic acid. In the adult goat, they had a yellowish cast, and were of a diameter of about 1-3500 of an inch. Any treatment by the strongest acid showed no farther structure.

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From these facts, it would appear that the yellow fibrous tissue *is not of cell origin, but is a form of fibrillated tissue*, which increases regularly with the animal until it has attained its full size. In the new-born goat, it is nearly three times smaller than in the adult, and therefore resembles the gradual growth of the muscular fibres, of which I have spoken on a former page.

VI. Adipose Cells and Adipose Tissue.

This tissue is always found associated with the one last considered, and Schwann, in his chapter on areolar tissue, considered it as a part of it. In the present case, I have thought proper to take it up in a separate division, but in the close connection of succession. The adipose tissue is strictly a cellular one, although these cells are interwoven generally with the fibrous element which serves as its mechanical support. These cells are spherical when not subjected to pressure, have a smooth contour, although they soon lose it when removed from the body and placed in a lower temperature, for their oily contents contract by cold. Their size is very variable, sometimes they are observed quite small (1-2000 of an inch), and then again they are so large as to be distinguished as glistening points by the naked eye.

As usually seen, they appear as simple vesicles, filled with oil, and it is rare that any trace of a nucleus is seen. Fat-cells, therefore, cannot be said to be *nucleated cells*, whatever opinion may be entertained of their origin. This fact has given rise to some dispute as to their formation. The question is, are they to be regarded as cells having always a pre-existing nucleus, or are they vesicles, which may or may not contain nuclear matter. Valentin* has noticed their appearance in the human fœtus (between the third and fourth month) in the palm of the hand; he describes them as collections of little cell-like bodies, which also exist isolated from each other. But these facts throw no light upon their primitive formation, a point which, from direct observation, is not yet well made out.

Schwann very naturally inferred their cell-origin according to the usual mode he described, and his opinion appears to have been confirmed by the fact of recognizing in the walls of some a nuclear body. Henle's† opinion is to the same effect. But the view which I am obliged to take, as based upon my own observations, is different; it is,

* Valentin; *loc. citat.* Entwickelungsgeschichte, p. 271.

† Henlé; Traité d'Anat. gén. tom. i. p. 424.

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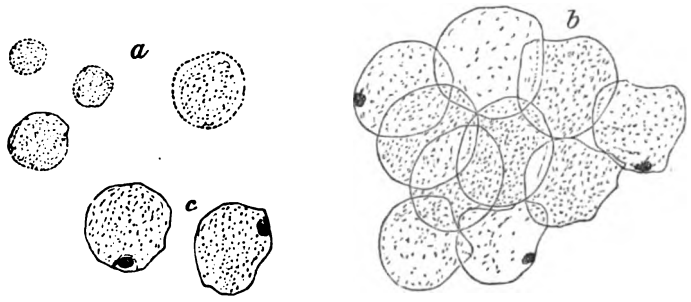
that they do *not* always form around a pre-existing nucleus, but arise as simple vesicles. These vesicles are formed primarily, according to that method Ascherson* has pointed out, and to which the attention has been called on a former page—simply by the formation of a plastic membrane of albumen, around a minute globule of oil. This body then increases and dilates by the accumulation of oil within, ultimately producing the vesicle of a full-grown size. During this growth, nuclear bodies may form within, in the same way—by the union of oil and albumen. This is the reason why you sometimes meet with cells having, in their interior, one, two, three, or even more vesicles or nuclei; or, there may be a single one of a well-marked character, having the aspect of a single nucleus. Such are the phenomena I have frequently met with, and especially in the formation of these cells in the mesentery of very young tadpoles. It is true that there are frequently seen whole groups of cells, in each of which is perceived a uniform nucleus. I therefore wish to be understood, that I do not deny that these bodies may exist as nucleated cells, but insist that they are often formed as simple vesicles.

Fig. 28.



Fat-cells and their formation, from bottom of orbit of eye of a dog. *a*. Vesicles without a nucleus, filled with fat. *b*. Fat-cells fully formed, irregular in shape from pressure, and for the most part without a nucleus. *c*. Two cells with traces of nucleus or granular matter adhering to membrane. (Magnified 250 diameters.)

VII. The Cartilaginous and Osseous Tissues.

I have thought best to consider these two tissues under a single head, because the one (the latter) can be regarded only as the other furnished with a mineral product, so combined with it, that the functions of organic shall be carried on in connection with the more mechanical relations of inorganic life. We will, however, take them up separately, and that in the order in which they are produced.

* Ascherson; Muller's Archiv, 1840, p. 46.

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In cartilage, we have a good example of the union of the simple, with the metamorphosed cell structure; the former being individual cells; the latter, the fibrous tissue in its finer and coarser forms. A consideration then of this tissue, in connection with the fibrous one we have just passed, is quite proper; and it will appear the more so, when we shall have shown that in some instances they pass imperceptibly into each other, and could not be distinguished, excepting for the *simple cell* element of the one, and the absence of it in the other. A review of the historical conditions of this subject as a whole, will well repay us, in rendering more clear some of the obscure points of the development and passage of the one into the other.

As we have seen on a foregoing page, the history of the cartilaginous tissue as one composed of cells is intimately connected with the history of the development and nature of cells in general; and in it, it may be said, the cell doctrines of animal tissues took their origin. It may, therefore, seem unnecessary to again pass over this field. But, in this connection, it is important as including the details of the subject.

As Henle has justly remarked, it is quite singular that a structure so apparent as is this in most of its relations, should have remained so long imperfectly understood. For its study, in anything like a satisfactory manner, does not date back but a few years previous to the active labors of Valentin and Schwann.

The older anatomists, who, in the historical accounts of the other tissues, seemed to have seized hold of points in a most wonderful manner, do not appear to have entertained very clear and correct ideas of its intimate anatomical and physiological conditions. The earlier labours of Weber* and Wagner† may be said to have laid the foundations of its more thorough study soon after. Others, it is true, and among whom may be mentioned Krause,‡ were labouring here at the same time. But in 1834, Purkinje,§ who has, as it were, identified himself with this structure and that of bone, published some observations which may truly be said to be the first embracing the histological features of this tissue, for he traced in it the early conditions of those corpuscles of bone now known under his name.

* Weber; Meckel's Archiv, 1827, p. 233.

† Wagner; Lehrbuch der vergleichenden Anatomie, Leipsig, 1834, p. 62.

‡ Krause; Handbuch der menschlichen Anatomie, Hanover, 1833, bd. i. p. 48.

§ Purkinje and Deutch; De penitiori ossium structura observationes, Breslau, 1834.

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In the following year, the work of Valentin,* so often alluded to, appeared, and in it were contained many valuable details. Valentin watched the appearance of this tissue or cells, in very young embryos. He recognized the cartilage-corpuscles, and the cavities in which they are formed, but did not agree with Purkinje, that the former were only early conditions of the osseous corpuscles.

However, Valentin's opinions were well grounded in observations, and the interpretation of these last was as correct as well could be, considering the then existing ideas of the elementary forms of structure.

Soon after this, came the observations of Miescher,† published with those of Müller, which were remarkable for the care with which they were conducted, and the correctness of many of the views entertained. To Miescher is due the establishing of one point, viz: the necessary connection between cartilage and bone, the former as the invariable precedent of the latter.

The same remarks, with but little alteration, will apply to the contemporaneous work of Meckauer,‡ and all these may well be said to have paved the way for those labours of Schwann, upon which the whole grand idea of the cell was based. To what Schwann has done, it would be almost superfluous to allude. As far as regards the simple genesis, he did not, perhaps, advance farther than Valentin before him: but he traced out its phases as the basis of cell-doctrines; and on this account his observations on this tissue were much more numerous than upon any other. It is quite remarkable, however, that this very kind of cell should also have been the one, the study of which led to the first doubts as to the universality of Schwann's and Schleiden's cell-doctrines.

Since the work of Schwann, the labours in this direction have belonged for the most part to points other than those of its genesis, such as its nutrition, and its relations to the osseous tissue. In this connection may be mentioned the work of Henle,§ which will soon arrest our attention, when I speak of the history of bone-studies; of Toynbee,‖ devoted especially to the nutrition of cartilage, and then

* Valentin; *loc. cit.* Entwickelungsgeschichte, p. 26.

† Miescher; De inflammatione ossium eorumque anatomie generali. Accedunt J. Müller's observationes de canaliculis corpusculorum ossium atque de modo, quo terrea materia in ossibus continetur. Berlin, 1836.

‡ Meckauer; De penitiori cartilaginum structura symbolæ. Breslau, 1836.

§ Henle; Traité de Anat. générale, &c. Trad. par Jourdain. Paris, 1843, tom. ii. chap. xiii.

‖ Toynbee; Memoir on the non-vascular tissues. Philos. Trans. 1841.

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of Todd and Bowman,*—in which Mr. Toynbee's views of the genetic relations of this tissue and its transformations are embodied. In 1844, Valenciennes† published a memoir on the cartilage of the cartilaginous Fishes, and the Mollusca. The subject received extensive attention, and, as I have since laboured some in the same field and met with similar results, I will state here the conclusions at which he arrives, although they have a more prominent zoological than histological importance. "1. That the cartilage-cells are generally arranged in such regular plans that it would be possible to determine by microscopical examination the order or even the genus of the animal, from the character of its cartilage. 2. That none of the cartilage-cells have, in any species, canaliculi communicating with them; and, 3. That gelatine and not chondrin is abundant in the cartilages of cephalopods." Such results are of more importance than at first would be supposed. They relate to the condition of this tissue, as found in that class of animals resting on the lowest point of the vertebrated kingdom, and where it so persists on its embryonic type that we have a chance to study its peculiarities; and, as will hereafter be shown, our clearest views of the nature of ossification are derivable from allied though not from the same sources.

Quite recently, there has appeared a very valuable paper on the formation of cartilage and its nutrition, by Dr. Leidy.‡ Of this paper it is hardly necessary for me to speak; for, being wrought out with the characteristic care and elegance of the author, it has become well known to all familiar with this subject.

Such are the prominent points in an historical retrospect of the cartilaginous tissue. It is now proper that our attention should be turned in a similar manner to one of its ulterior conditions, viz. that of bone, which need occupy us but for a brief period.

Singular as it may seem, it was the opinion of some of the older anatomists, that bone was not formed *out of* cartilage, but was a product *de novo*, replacing the latter. So thought Albinus,§ and others of his time.

It is true, however, that, however obscure may have been the *derivative* nature of this tissue, both Malpighi‖ and Leeuwenhoek¶ had

* Todd and Bowman; Cyclop. Anat. and Phys., Art. Osseous Tissue.

† Valenciennes; Comp. Rendus, Nov. 25, 1844.

‡ Leidy; Amer. Journ. Med. Sc. January, 1849.

§ Albinus; Academicarum adnotaliorum. Libri octo. Leyden, 1754. Lib. vii. cap. vi. p. 77.

‖ Malpighi; Opera posthuma, London, 1797, p. 47.

¶ Leeuwenhoek; Anatomia Ossium, Leyden, 1689, p. 11.

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perceived to some extent its *immediate* composition, recognizing what they called "fibres." Most willingly do I omit a consideration of these elder authors.

Well-grounded labours, and those the influence of which extended forward into the modern times, do not date farther back than the time of Purkinje,* whose results were published by Deutch, in 1834. He recognized the corpuscles known under his name, with the canaliculi, and the concentric arrangement of the lamellæ. But this signification does not appear to have been clearly made out. He, or rather Deutch, supposed that these radiating lines (canaliculi) were tubes filled with calcareous matter, and therefore served some ulterior purpose in ossification. Soon after this, the work of Miescher,† already quoted, appeared. He followed closely in the paths already trodden before him, but expresses a different view from that of Purkinje as to the nature of the canaliculi. He did not find them filled with ossific matter; neither could he trace them through the concentric lamellæ. I notice this point here, because it well shows upon what minute points of structure men were at that time engaged.

As I have previously remarked, a high estimate is to be placed upon the labours of Miescher, not only from the great care with which they were made, but because he recognized the necessary pre-existence of cartilage for the formation of bone, and supported the view in a manner which would be well worthy of researches of the present time. This foundation being laid, the labours immediately subsequent were upon still more important points—the immediate phenomena of the osseous formation, and the part which each portion played in producing this result.

In the works of Gurlt,‡ Schwann,§ and Gerber,‖ these subjects have received the most careful attention, and as the views therein expressed, although dissimilar, will frequently be alluded to hereafter, it may be well to notice them here in a particular manner.

Schwann, from the thoroughness of his studies in this direction, should be entitled to a very correct opinion as to the nature of cartilage, in both its primary and secondary conditions. He was in-

* Purkinje; *loc. citat.*

† Miescher; *loc. citat.*

‡ Gurlt; Lehrbuch du vergleichenden Physiologie des Haussaengetheire, Berlin, 1837.

§ Schwann; *loc. citat.* 1839.

‖ Gerber; Handbuch der allgemeinen Anatomie der Menschen und des Haussaengetheire, Berne, 1840. Or Gulliver's edit. London, 1842.

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clined to adopt, as correct, the opinions of Miescher, who preceded him, and especially as to the important point of the identity of the Purkinjean corpuscle with the pre-existing cartilage-cell, and *not* with its *nucleus*. He therefore regarded the canaliculi "as hollow prolongations of the cells into the cellular substance." This may appear a small matter; but, as will soon be shown, on it rests the clearest doctrines of osteogenesis, viz. those of *immediate substitution*.

Gerber, on the other hand, and with him, Gulliver, I think, are of the opinion that the Purkinjean corpuscle is the *nucleus* of the cartilage-cell, and that the canaliculi are nuclear stellations. This view has, I well know, some *analogous* support in the stellate nuclei of many vegetable cells; as, for instance, those of the common geranium (*Pelargonium*). The same view was entertained by Todd and Bowman.* Now, if the opinion that the nucleus and the cell are histologically the same, as I have advocated on a previous page, be assented to, it would appear of no importance to cavil about this point; for, whatever capacity would be given to the cell, would be given to the nucleus also, and therefore we could look for stellations of the one as well as of the other. But this point has other bearings, as we shall soon perceive.

The investigations of Mr. Erasmus Wilson,† made in the summer of 1841, seem to have been attended with particular results. He had watched the development of bone, and expressed the opinion that the canaliculi deserved the name of "converging tubuli," from their manifest tendency towards the Haversian canal; and as this tendency appeared constant, it gave him the impression that they served as a necessary means of connection of the periphery with the centre of the concentric lamellæ. I am not aware, however, that Wilson entertained any positive opinion of his own on the mooted point of Purkinjean corpuscle and cartilage-cell.

On this topic, the view of Henle‡ is different from both those of Schwann and Gerber above mentioned. He "regarded the Purkinjean corpuscles as the cavities of cells, of which the thickened sides form the fundamental substance, and the osseous canaliculi are the tubes which penetrate the cavity of the cell in the thickened sides of this last, as are the porous canals of vegetable cells."

But I must confess that this description, given in Henle's own

* Todd and Bowman; Physiological Anatomy, p. 108.

† Wilson; The Anatomist's Vade Mecum, London, 1842.

‡ Henle; Traité d'Anat. générâle, &c. Trad. par Jourdain. Paris, 1843, vol. ii. p. 409.

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words, does not appear clear to me, neither do I think the reasons for it, subsequently stated, sufficient for its adoption.

Flourens,* in the same year, published his results upon the formation of bone. But they relate chiefly to another department—its production in connection with the periosteum and the so-called medullary membrane, the existence of which he supports, regarding it as a mere extension of the former. These observations were continued, and published in 1845;† and in the same year there appeared a long paper by MM. Brullé and Rugenny.‡ To this last, however, I need not here allude. The researches of Mr. Tomes§ were quite extensive, and were published soon after. He described, among other things, the ultimate structure of bone lying between the canaliculi and lacunæ, as consisting of minute spherical granules, and not being a homogeneous substance, as usually described. In regard to the other microscopic points, his view is that, after the formation of the ossific tubes (canaliculi) in the process of ossification, the latter are filled up by a deposit of osseous granules, and while this deposition is going on, small cells are left, which are the rudimentary Purkinjean corpuscles.

More recently, and within a few years, important researches have been made; and it is singular that, after so many varied opinions should have arisen, some of the earliest views are advocated. Thus, Hassall‖ agrees with Schwann, and says: "The Purkinjean corpuscles are to be regarded as complete corpuscles, the canaliculi of which are formed by the extension of the cell-wall, which is proved by watching the formation and development of bone."

This same view has since received a very able confirmation from the studies of a most excellent observer, Dr. Leidy;¶ and these were made upon the ossifying frontal bone of the human embryo of two inches. Here he traced the immediate transition of the cartilage-cell into the Purkinjean corpuscle.

In this connection, the name of Dr. Sharpey** should not be overpassed without mention. His studies upon osteogenesis have been quite extensive, and he has revived the old opinion, that true bone is

* Flourens; Comp. Rendus, 1841.

† Flourens; Comp. Rendus, Dec. 8, 1845.

‡ Brullé and Rugenny; Annal. des Sc. Nat. Nov. 1845.

§ Tomes; Cylop. Anat. and Phys. Art. Oss. Tissue, March, 1846.

‖ Hassall; Microscopical Anatomy of the Human Body, London, 1847–50.

¶ Leidy; Proceed. Phila. Acad. Nat. Sciences, 1848.

** Sharpey; Human Anatomy, by Jones Quain. Ed. by R. Quain and Wm. Sharpey, London, 1847; Introduction.

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sometimes developed *in membranes*, and without the pre-existence of cartilage. This point he thinks he has demonstrated by watching the development of the cranial bones; and Dr. Carpenter (as he affirms in his *Human Physiology*) mentions that his own researches have confirmed them. For a description of these changes I must refer to his work, as they cannot be clearly and laconically stated here. A criticism upon this view will be passed when I state my own results; and, by way of conclusion of this historical view, I will add that, quite recently, two excellent observers, Kolliker* and Robin,† after a somewhat comprehensive *practical* review of the whole subject of osteogenesis, express their concurrence in the old opinion of an *intramembranous* form of bone-genesis. This last would certainly be enough to make one hesitate as to an opposite opinion; but, as I shall now proceed to show, the whole matter can be quite definitely comprehended by proceeding step by step.‡

The true cartilage tissue, when existing as such, is always invariably the same. Two varieties, however, dependent upon a degree of organization, are formed. The first, called *cellular-cartilage*, or comprising those of a transient nature, and ultimately passing on to the formation of bone, consists of nucleated, well-defined cells, lying in a semi-solid punctiform stroma. The second, called *fibro-cartilage*, consists of the same kind of cells, lying in a network of fibrous tissue, which last is only a farther developed condition of the punctiform stroma. In this last, this fibrous tissue may so increase that the true cellular element entirely or almost entirely disappears; and hence the transition of *fibro-cartilage* into *fibrous tissue*. Another division of cartilage is into the *temporary* and *permanent;* but this is conventional only, for they differ in no essential particular from each other.

It is evident from the above view that all cartilage is originally the same, viz. *cellular*, appearing as such in the embryo. Its primitive formation there takes place, according to my own observation, in the following manner: At those points at which cartilage,

* Kölliker; Anatomie Mikroskopische, &c. bd. ii. p. 376.

† Robin; Mem. de la Soc. de Biologie à Paris, 2ieme ann. 1850, p. 119.

‡ I regret being unable to allude in some detail to a comprehensive Memoir on this subject by Tomes and De Morgan, recently presented to the Royal Society. It is not yet published, but there is an abstract of it in the Lond. Med. Times and Gaz. Oct. 2, 1852. From this abstract, I should judge they do not believe in the formation of bone without a pre-existent cartilage. It would also appear that they think that the *nucleus* of the cartilage-cell becomes the Purkinjean corpuscle.

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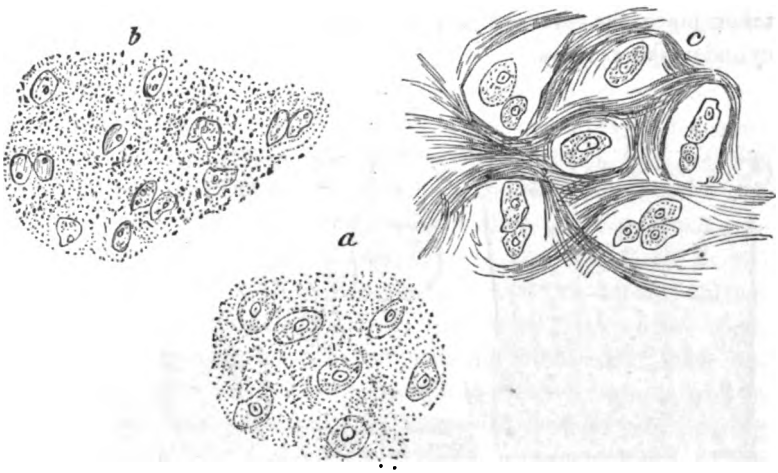
and afterwards bone, is to appear, there exist cells, which, as to physical characteristics, cannot be distinguished from those which are to form other tissues. A part of these cells are condensed into a punctiform stroma, leaving open spaces here and there, in which persist the original cells, from one to three or four in each cavity; there is, then, a finely granular stroma, in the midst of which lie free nucleated cells. At first, this separation is marked out by faint lines only; but, subsequently, it becomes more marked, and, finally, after consolidation, presents the true *cellular cartilage*. As this stroma is formed *closely* about the cells, it is not correct to say that *cavities* are at first formed in the stroma, and in which the cells repose. However, subsequently, the cell-membrane, lying in direct contact with the stroma, blends with it and is lost; and then there appears a cavity in which the *nuclei* of the old cell persist. But as these changes take place at an early period, one does not often see anything but the nuclei; and as these are nucleated, and resemble cells, it is proper that they should be designated as such.

Fig. 24.



Cartilage and its formation. *a*. Cartilage just forming, appearing as cells lying in a finely punctated stroma, from vertebral column of fœtal goat of one month. *a*. Cellular or temporary cartilage, from femur of a fœtal goat. *c*. Fibro-cartilage (permanent), the punctated stroma having become fibrillated, giving the whole a nidiform aspect. From articular cartilage. (Magnified 822 diameters.)

In *fibro-cartilage*, the same early changes occur, but the stroma is farther developed, and a fibrous or fibrillated tissue is the result; when this has occurred, the cells (or rather cell-nuclei) lie in *nidiform* cavities, each of which quite resembles a bird's nest with

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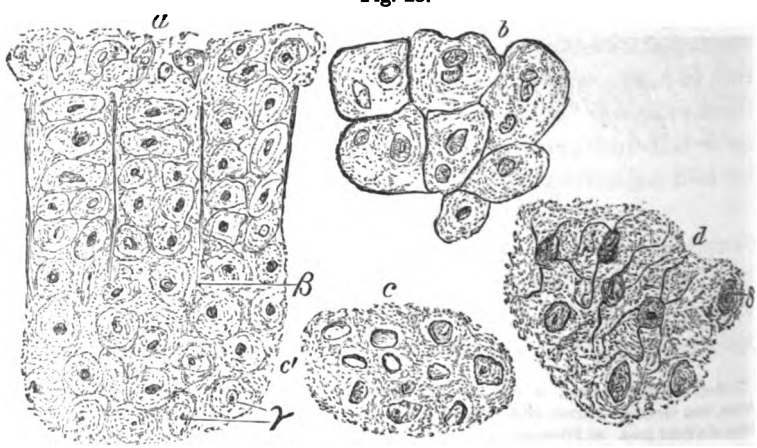
eggs. It is only the *cellular* form of cartilage which is developed into bone, and this osteogenesis is, as we shall now perceive, only a kind of displacement of one material for another. We will suppose the cellular cartilage is formed; then, as the ossific matter is about to be deposited, the vascularity of the cartilage is much increased, so that the substance presents a pinkish hue; then a kind of liquefaction of the intervening stroma appears to take place, so that the cartilage-cells seem no longer confined irregularly, but are, for the most part, free to assume almost any relative position.

A tendency with them to an arrangement in linear series is then manifest. These rows run parallel with the long diameter of the bone, and are separated from each other by the intercellular matrix, which consists of the partially liquefied stroma. It is thus, when these rows are formed, that the future bone may be said to consist of a fasciculus of tubes in which the cartilage-cells are situated; and I have often thought that, if a tranverse section could be then made, it would present a very uniform appearance of this kind. This intercellular matrix constitutes the primitive ossific rete, in which the calcareous salts are first deposited. This first deposition having taken place, the cartilage corpuscles are situated in cup-like or rather cylinder-like cavities.

Fig. 25.



Formation of bone in cartilage, from tibia of fœtal goat of two months. *a*. Cartilage-cells arranging themselves in a linear manner, and septa appearing between them. *c*.' The stroma and cells so far ossified that all are blended together. *b*. Transverse section of *a*, showing the cylindrical tubular arrangement. *c*. Ossification still farther progressed, the aqueous portions being absorbed, the cells are brought nearer together, and appear shrunken and irregular. *b*. Still farther progress, the canaliculi shooting out from the cells, in some of which there is a trace of the old nucleus; thus showing that the Purkinjean corpuscle with canaliculi are the transformed cartilage-cells. *β*. Appearance of septa fading away. *γ*. Nuclei disappearing. *δ*. Nucleus persistent. (Magnified 200 diameters.)

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Before this has occurred, however, the cartilage-cells, and the substance immediately surrounding them, are likewise charged with calcareous matter. The cells become smaller, and, in contracting, assume irregular forms; the septa, separating the tubes in which they formerly lay, become more and more indistinct, from the universality of the calcareous deposition. Finally, there is perceived a grayish mass, with but little regularity, and variegated with these irregularly shaped bodies—the future Purkinjean corpuscles. But we will look at the process a little more minutely. By this calcareous deposition, the aqueous parts of the tissue disappear or are absorbed, the size of the whole is reduced, and the cartilage-cells brought nearer together; the tissue, therefore, is much more compact, but still does not lose all of its original characteristics. The tubes, of which we have spoken, form the *concentric lamellæ*, the corpuscles having arranged themselves in a regular manner around in it; and, therefore, a transverse section shows the bone to be made up of *solid cylinders*, instead of *hollow tubes*, as before the deposition of the calcareous matter.

The cartilage-cells are transformed into the Purkinjean or osseous corpuscles. This I have clearly observed, and have traced all the phases of the change. When they exist in the cup-like cavities, their nucleus is prominent. But as ossification goes on, the nucleus gradually crumbles away, and, by the time ossific matter is deposited in the cell-walls, little of it is seen. The cell-walls, however, remain, but have become shrunken, and, from the absorption of the aqueous portion of the intervening substance, they are brought nearer together; however, they hold regular concentric relations to the central portion of that which was the tube, and this central portion is to be the Haversian canal. When the cartilage-corpuscles begin to shrink, radiating lines are seen running from each; these appear to commence with the corpuscle and radiate from it, and in shooting out in every direction, they meet with those of neighbouring corpuscles, and thus are joined together on every side. Thus far all seems clear. The cartilage-cells lie in the midst of an amorphous stroma; this last partially liquefies; the cells then arrange themselves in rows; these rows are separated from each other by this intercellular stroma, and form more or less regular tubes; in the walls of these tubes the ossific matter is first deposited, and afterwards in their contents, both cellular and granular. These tubes form, each, a system of concentric lamellæ—from the arrangement of their contents in a concentric manner—and this occurring around a central

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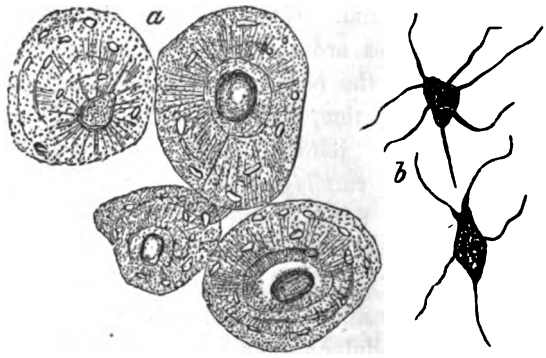
cavity, forms the Haversian canal. The Purkinjean or osseous corpuscles are the pre-existing cartilage corpuscles changed by the ossific processes.

The next question is, what are the canaliculi? Are they prolongations of the cartilage cell-membrane, as Schwann has supposed? As far as my own observation yet goes, I must say that I have seen no evidence that such is true, excepting their general appearance. On the other hand, there are facts which oppose such a view. In the first place, the canaliculi do not commence to form until the calcareous matter begins to be deposited, and, as the cell-membrane is then either filled with calcareous matter or absorbed, it could not well send out prolongations. Moreover, these prolongations are often of such a length, and branch and rebranch so often upon themselves, that it can scarcely for a moment be entertained that they are thus formed. I ask, is it not more probable that they are channels for the escape of aeriform matters from the interior of the cell? For the cell, existing in the midst of an ossifying mass, would retain its animal matter longer than the rest of this tissue; and this animal matter would give rise to gases seeking their escape in every direction by percolating the surrounding semi-solid mass; and in behalf of this view, perhaps Wilson has rightly named them "converging tubuli," formed as they are by currents naturally seeking or converging towards the nearest outlet.

I do not, however, express this as a well-grounded opinion, but simply as a probable hypothesis. We shall soon see what other reasons there may be for its adoption.

Fig. 26.



Microscopical view of Transverse Section of Bone, from Femur (human). *a*. The concentric lamellæ, with Haversian canal and Purkinjean corpuscles. (Magnified 175 diameters.) *b*. Purkinjean corpuscles isolated, to show irregular form and the canaliculi. (Magnified 450 diameters.)

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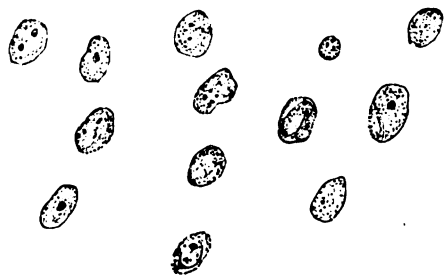
In these phases of formation, some of the nuclei of the cartilage cells, or even other adventitious cells of a small size, may not be dissolved, but become ossified as such, as are the cells in some of the lower fishes. You would then find them as cell-like granules, scattered irregularly through the osseous tissue. These are not the granules spoken of by Mr. Tomes, but distinct cells of a larger size, varying from 1-3000 to 1-2000 of an inch in diameter. It is in this way that I account for the occasional presence in the spongy tissue of the long bones of small spherical bodies, first discovered by Dr. Holmes, of Boston, who met with them early in 1849. Subsequently, however, Lebert* has fully noticed them. As their presence is not constant, and does not apparently involve any pathological condition, the above view of their nature appears to be correct.†

Such appears to be the mode of proceeding of the formation of the *compact tissue* of the bones of the higher vertebrata, as I have studied it in fœtal goats. The process is simply one of *substitution*, with that contraction and modification of form which must necessarily follow when a soft is replaced by a sclerotic tissue. This law of substitution is everywhere the same; but, to be sure, there is a difference in some of the steps of its progress in the different kinds of bones.

Fig. 27.



The peculiar long corpuscles sometimes met with, from medullary tissue of head of femur. The corpuscles, for the most part, have nuclei, some of them several. (Magnified 350 diameters.)

The spongy character of the internal or middle portion of bones appears to be produced by the absorption, by the numerous vessels there situated, of the lighter portion of the primitive cartilaginous

* Lebert; Compt. Rend. des Séances de la Soc. de Biologie à Paris, Oct. 1849, p. 149.

† I think the "ossified cells," as they are called by Tomes and De Morgan, must be the same bodies. See their "Memoir on the Structure and Development of Bone," read before the Royal Society, but of which I have seen an abstract only in the Med. Times and Gaz. London, Oct. 2, 1852.

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base, and a consolidation towards the periphery. This, however, is a point having a teleological bearing, for by such an operation, the bones possess the greatest strength and lightness combined, attainable with the same amount of material.

These phenomena of genesis just described belong especially to the higher vertebrates.

In the lower classes, they are far from being of this complicated character. Such is the case with many fishes. The concentric lamellæ do not exist, and therefore there does not occur any linear arrangement of the cartilage-cells; but they become ossified *in situ*, their canaliculi radiating from every side, giving the whole a most regular and beautiful appearance. Everything is left exactly as when a soft tissue, cells, nuclei, and all, giving you the impression that it had *suddenly* been charged with calcareous matter. Such appearances may be seen in some of the bones of the sword-fish.

The cartilage of cartilaginous fishes may be said to differ from the common cartilage of the higher vertebrates. In fact, it cannot be properly called true cartilage, but is, if I may so express myself, osseous tissue in a cartilaginous dress. This will be clear if I make one or two explanatory remarks. As has already been stated, Valenciennes* has shown that the cartilages of cartilaginous fishes and the Cephalopods contain gelatine, and *not* chondrin. Now, Müller† has shown that all ossifying bone contains chondrin and not gelatine, and after the process of ossification has taken place no chondrin is found, but all is gelatine. Therefore, bones are, so to speak, *gelatinous*, and not *chondrinous*. The same we have just seen is true of the so-called cartilages of the cartilaginous fishes. This will make clear the statement just made. I much regret that no examinations (at least of which I am aware) have been made of the chemical character of the embryonic condition of cartilage in these lowest fishes, for I should venture to predict, upon the above premises, that they would be found *chondrinous* and not *gelatinous*.

The tissue forming the cartilaginous skeleton of these fishes, as I have had an opportunity to examine it, is composed of oval or spherical cells, resembling the common cartilage-cells at an early period of development. These have become *hardened in situ*, but not *calcarified*, and never have I met with any having the canaliculi

* Valenciennes; *loc. citat.* Comp. Rend. Nov. 25, 1844.

† Müller; Poggendorf Annalen, Bd. xxxviii. p. 316; or Ueber den feineren Bau der Geschwülste, Berlin, 1838, Erster Band, pt. ii.

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radiating from them. From all these data, I think we may justly conclude that, in these lower fishes, we meet with bone-cartilage and not with true bone.

The cartilaginous and osseous tissues, as we have thus just described them, may well be said to belong to the *vertebrata* only. Among the *invertebrata*, the only class having any claim to our consideration in this respect is that of *cephalapoda*. With these I have no experience; but Siebold* says that their skeleton has a structure like that of the true cartilages of the *vertebrata*. But this similarity is probably an *analogous* instead of an *homologous* one; exactly as may be said of some of their other tissues; for our most comprehensive knowledge of these two kingdoms leads us to infer that all true homology ceases with the boundary of each.†

In regard to the mooted point, whether true bone is ever formed *without* the pre-existence of cartilage, it follows from the above that, as true bone contains osseous corpuscles—and these are modified cartilage-cells—the answer would be in the negative.

VIII. Crystalline-Lens Tissue.

This is the last tissue to come under our consideration, and I should have omitted it here altogether, had I not watched the phenomena of its genesis. This genesis takes place from cells, originating, almost without a doubt, according to my own mode of cell genesis already described.

As would be inferred from its very function, there is perhaps no tissue in the economy having so delicate a structure. It is no wonder that the attention of the early anatomists should have been excited to the consideration of the fact that an *organized* structure should be able to act exactly like an unorganized vitreous substance. Its very function precluded any correct knowledge of its structure with the ordinary aids of vision, for it is nearly transparent. However, from studies upon the inferior animals, its tissue had long been recognized to be "fibrous." But how these fibres were arranged so as not to interfere with the transmission of light was the mystery. This point Sir David Brewster‡ first elucidated by means of polarized

* Siebold; Comparative Anatomy by Siebold and Stannius, trans. from German, &c. &c., by Burnett, vol. i. § 231.

† Since the above was written, I have had an opportunity to study the cartilage of the Cephalopods as illustrated in the common Squid (*Loligo illicebrosa*), and my observations agree with those of Siebold above referred to.

‡ Brewster; Philosoph. Transact. 1833.

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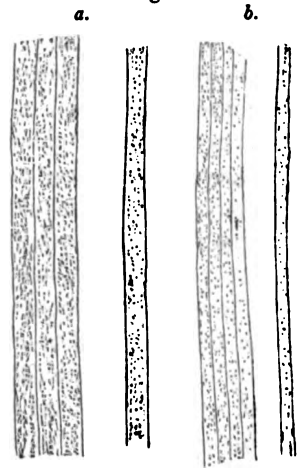
light, and showed that they were united by dentated edges which lock into each other.

Since the days of cell studies, this tissue has been shown to take its origin in cells like that of others. This was first pointed out by Valentin* from studies upon embryo sheep. Since then the same has been observed by Werneck† and Schwann;‡ the last of whom has applied his cell-theory to the phenomena observed.

These last three observers agree as to the nature of the metamorphosis producing the fibres, viz. that the fibres are simply elongated cells, which cells, Schwann has concluded, arise always from a pre-existing nucleus, because many of them were found nucleated. However, Schwann freely admits that his observations on this subject have not been complete.

More lately still, the subject has been examined by Martin Barry§ and Mr. Toynbee,‖ both of whom take an opposite, or rather different view from that of Schwann, viz. that the fibres are formed by

Fig. 28.



a. Crystalline-lens fibres from a cat. *b*. Crystalline-lens fibres from an owl (*Strix virginianus*). All represented slightly granular for effect.

the coalescing of several cells into one, forming at first a tube after the manner of that of muscular fibre.¶

* Valentin; *loc. cit.* Entwickelungsgeschichte, p. 208.

† Werneck; Anemon. Zeitschrift, bd. v. p. 414.

‡ Schwann; *loc. cit.* p. 100.

§ Barry; Embryological Researches, third series.

‖ Toynbee; Memoir on the Non-Vascular Tissues. Philos. Trans. 1841.

¶ In anatomical and histological works, the fibres of the crystalline lens are described as having dentated edges, which fit into the corresponding structure of the

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The following is what I have observed both as to the genesis of the cells and their transformation :—

In the fœtal goat of two months old, and six inches in length, the crystalline lens is biconvex, and made up of three parabolæ, the apices of which meet in the centre, thereby leaving at that point a spherical triangular space. The central portion of the lens, extending to near the periphery, is opaque, and this opacity is greatest in the centre, becoming less and less so as you approach the outer edge. The part most opaque is composed of minute granules or utricles, which, as you approach the outer edge, are larger and larger, until on the very border of the opacity they are seen as quite sizable vesicles, some of which are nucleated, in fact, cells. Thus you have, in this opaque central portion, an appearance of all the transitionary stages of minute utricles to large vesicles and cells ; and this development has occurred by mere expansion of the former, and *not* by the *Schwann mode* of cell development.

At the confluence of the opaque portion with the clear peripheric border, there is perceived a tendency of the cells or vesicles to arrange themselves into rows or serial longitudinal groups, which rows are parallel with the long diameter of the parabola. Then, by the confluence of these rows of cells, tubes are formed, and from these the fibres. Therefore these tubes have their origin, *not* by the elongation of single cells, as Schwann has supposed, but by the coalescence of piles of cells into one continuous tube. These observations, therefore, correspond with those of Barry and Toynbee, as above mentioned. In this instance it was truly beautiful to observe, how the clearing up of the structure took place just in proportion as the constituents assumed the cell-form; thus showing that this tissue only attains its wonderful function by the remarkable process of cell-genesis—a process by which form and order appear to start out of chaos, and that which is dark and obscure is made almost as clear and transparent as light itself. And when one has seen these

contiguous fibres. This serration was, as is well known, first pointed out by Brewster with the lenses of fishes. But from a rather detailed examination of the subject of late, I am satisfied, that, whatever may be said of it with fishes, it does not belong to the higher animals. With *perfectly fresh* specimens from man and many of the higher mammalia and birds, I have been unable to perceive the least dentation. The fibres are perfectly smooth and contiguous; this has been the invariable appearance when very fresh specimens were used. I cannot but believe, therefore, that this crenulated condition is due to a change in the fibre after death ; a change, perhaps, like that leading to like appearances in the blood-corpuscle, and due to a partial abstraction of its liquid contents, the sheath of the fibre becoming ruffled.

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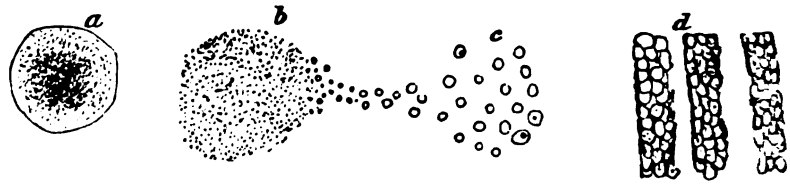
things, does it not bring home to the mind with suggestive grandeur and beauty, the recollection of that fiat of the Deity, which is recorded—"And God said let there be light, and there was light?"

We will now see how these phenomena are manifested in *oviparous vertebrata*. In the chick which I have especially examined, the phases of formation are best examined at about the eighth day. Here, as in the former instance, a part is cellular and granular, and another part tubular, or soon to become so.

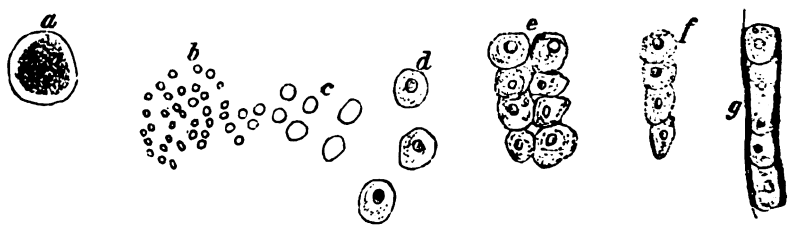
Fig. 29.



Formation of crystalline lens in mammalian vertebrates, from fœtal goat of 2 months. *a*. The lens with a dark centre, clearing towards periphery, where there is a light transparent border. *b*. Minutely granular structure of centre, composed of very minute utricles, which become visibly vesicles as you approach the outer edge. *c*. Vesicular or cell-structure of portion where pellucid border passes into transparent periphery. Some are distinctly nucleated. Size 1-2000. *d*. Structure of transparent periphery composed of tubes formed by linear aggregations of the cells.—*a*, *b*, and *c*, magnified 225 diameters. *d*, magnified 450 diameters.

The minute vesicles are here observed, gradually expanding, until they have reached a certain size, when they become nucleated. There are, therefore, both nucleated and non-nucleated cells. When the cells have become nucleated, they begin to arrange themselves into rows, and then by coalescence form tubes. There is, however, this difference between those of the goat and chick, as I have observed: In the goat, several rows of cells form a tube, while in the chick the same result is obtained by a single row. Afterwards, the nuclei gradually disappear, and beautiful transparent fibres appear.

Fig. 30.



Formation of crystalline lens in oviparous vertebrates, from chick of 8 days. *a*. The lens with a dark granular centre, and transparent border. *b*. Granular or minutely utricular structure of the central portion. *c*. Vesicular structure of portion intermediate between the centre and clear border. *d*. Vesicles that have become nucleated. *e*. The same arranged in rows. *f*. The same coalesced or fused partly. *g*. A single row of cells, forming a tube, not so much by a coalescence of their own walls, as by a plasmatic membrane enveloping them. (Magnified 350 diameters.)

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The crystalline-lens, therefore, is truly a tissue of cell-metamorphosis, and from its delicacy and beauty has well thus briefly arrested our attention.

It will be seen that the phenomena of this metamorphosis, as I have observed them, are of the same nature as those involved in the formation of muscular fibre, that is, by a kind of intersusception, and *not*, as Schwann has thought, by immediate fibre-transition.

With this, I close the genesis of cells in particular, and their metamorphoses into special tissues.

CHAPTER IV.

CELL-FUNCTION, OR THE PRODUCTS IN CELLS.

In the preceding chapter we were engaged in tracing out the genesis of cells in particular, and the phases of their metamorphoses into special tissues. However carefully this was or may have been done, it gave us no insight into the conditions of cells as *individual*, active agents. As yet we have become acquainted with them only as physical objects, and we know them as we know a machine, by its size, shape, and general aspect. Such is the broad distinction between anatomy and physiology: the former teaches us nothing of the latter, and the *vice versa* is nearly equally true. Our knowledge of what cells *do* (not become) must be learned of itself, and proved by experience. I propose, therefore, to consider them in this light, taking up each class by itself, as was done in the genesis.

I should mention, however, that cell-function, or the products *in* cells, can belong only to individual cell-structures, or those in which the identity of the organ as such is not lost in a compound tissue.

I. Epithelial-Cell Function.

The epithelial cell is the great secreting agent of the animal economy. By it, the essential products of life and its reproduction are eliminated; and on it, rest all the grand phenomena of nutrition and secretion. Its full discussion, therefore, would involve many points for consideration, which otherwise would not fall within the domain of this work. I have already pointed out its anatomical peculiarities, which, however diversely it may be situated, are invariably the same.

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It is the *essential* structure in all glandular organs, constituting the skin, and covering all mucous and serous membranes. These are facts so well recognized in science, that their consideration in detail is not necessary. It would be easy for me to take any gland, and, by reference to my sketch-book, demonstrate its leading features. The *mammary gland*, for instance, could be taken, and by reference to observations throughout the mammalian vertebrates, its intimate structure be shown to consist of ampullæ, lined with epithelial cells, by which alone the milk is formed or elaborated from the effused plasma of the blood.

Or, in the same manner, I might take the liver, which appears to have an existence in the animal economy, even more common than that of the heart itself,* and show that the elimination of bile is performed by cells alone among the lower invertebrata;† they exist as simple cells, having no basement stroma and no ducts; in fact, in the simplest form. As we advance, we find these cells having more complicated relations with nutrition and secretion, and wrapped into an organ of a separate and distinct character. Without entering into any of these discussions as to the minute anatomical relations of these cells, and which has been a very mooted point of late, I think I may say that the fact of the cell-origin of the bile or its analogue is quite clearly established.

The remarks just made belong in common to all the other glands of the body which elaborate liquid products, wherever they may be situated. It is not for me to take up here their anatomical relations. I have finished with them when I make the general statement as a scientific fact, based upon my own, as well as upon the observations of others, that the peculiar products of each gland are eliminated by them *as nucleated cells*. If it is asked *how* such is accomplished, I should reply, by the action of the cell-wall; it being the peculiar property of the walls of liver-cells, for instance, that they should elaborate bile from the *plasma* of the blood, which transudes through them and is changed to bile by coming in contact with the nucleus. The same is true of other secreting cells. Beyond this, physiology and experience have taught us nothing; and what is more, it is now difficult to conceive how they ever can. The *why* that one cell should

* The remark of Haller is not broad enough. He says: "Laté per animale regnum hepar dominatur, et si paulo angustioribus finibus continentur, quam aut intestinum—aut con."

† Siebold; Comparative Anatomy, by Siebold and Stannius, transl. by Burnett, vol. i. § 87. (This I have seen myself also.)

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secrete bile, and that another, exactly like it in physical aspect, should secrete milk or saliva, will long remain an enigma in physiological science.

The *solid* products in epithelial cells now deserve our attention, and they possess full the interest of those we have just been considering, because they have an individuality of their own. I refer here to the embryo as the product of the *ovum*, and to the spermatic particle as the product of the *sperm-cell*. What the function of these two cells is, and how it is discharged, and what is the nature of the product, is worthy of our consideration in a more than usual degree. Some detail will, therefore, be necessary.

We will first refer to the homological conditions under which the ovum and sperm-cell are to be regarded. The ovum is the sole product of the ovary, and the sperm-cell that of the testicle. In a histological point of view they are the same, and although it might be difficult to make this plain with the higher animals, the conclusion is *forced* upon us when we study some of the invertebrata. With many of these last, there are no outward distinctions of sex, and those which are internal are purely microscopic. With many of the Radiata, upon which I have of late been considerably at work, this is especially true. The testicles are situated exactly as are the ovaries, and the testicular ampullæ correspond in number, size, and general aspect with the ova, and one can be distinguished from the other only by its contents. This is so, for instance, with the asteroid Echinoderms.

In the Bryozoa, which are androgynous, I have found these distinctions even less marked. Thus in the genus *Alcyonella*, there are a few cells in one part of the animal, which go on to form ova, while other cells, apparently identical in physical points, go on to form spermatic particles.

Among the Annelida, the same is seen in a truly beautiful manner. Take, for instance, the genus *Nais*. Here you find the ova, situated in a row on each side of the alimentary canal, each consisting of a little grain about the size of a pin's head. In the male there are similar bodies, having exactly the same locality; these are sperm-cell capsules; and the only difference I have found between these little grains is, that they are white in the male, and yellow in the female.

Among the higher invertebrates, as, for instance, with the Articulata, these homologies of the two organs are distinct, but less so than in the instances I have just quoted. From all this it may be safely inferred, that the ovary and testicle, as individual organs, consist only

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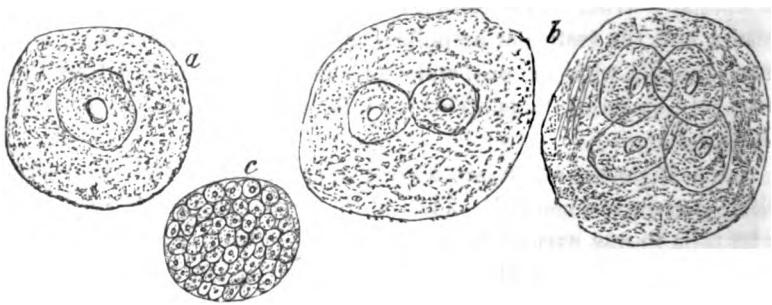
of a tissue which supports a cell-structure, which is the essential part. Now as this cell is an epithelial cell, we can truly say that, histologically, they are one organ. But perhaps the best part of the argument is yet to come, for by illustrating the processes they undergo, the morphological identity will be clearly made out, although we should not forget that there is a broad teleological difference.

The ovum exists at first as a simple nucleated cell, and it goes on to the attainment of its full size by the endogenous formation of cells within it. This last takes place out of the fertile blastema, which transudes its walls, and according to the laws of cell-genesis already pointed out. Having attained its full size, this endogenous increase ceases, and then the ovum is ready for fecundation. By this last process, it is endowed with an individual potency, upon which succeed changes ending in the elimination of a distinct being. These phenomena have their first expression in the segmentation of the vitellus, dividing equally into two, then into four, eight parts, and so on, until the result of the last division is quite small, the whole being reduced to a granular mass, out of which the embryo is shapened and formed.

Fig. 31.



Ova and the vitelline segmentation. *a*. An ovum in natural state. *b*. Segmentation singly and doubly occurred. *c*. Segmentation completed, and ovum ready for the morphological changes of the embryo. (Reduced size.)

But if this segmentation, for this definite object, is always indicative of an impregnated state, the simple phenomenon of segmentation has not this signification, for it occurs in many ova, to a limited extent, before fecundation has taken place. This I have observed with the ova of fishes and many of the Articulata.

You may ask: What, then, is the signification of vitelline segmentation? To this it may be replied, that it is the first and only unmistakable expression of vitality we know a cell or an ovum to possess. In common cells, and in the unfecundated ovum, it is an abortion;

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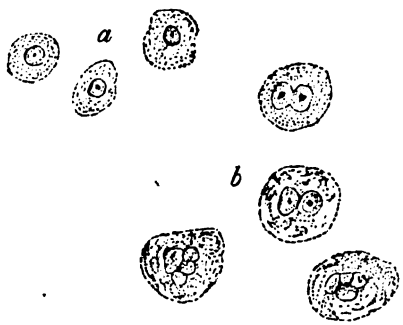
but from the act of fecundation it receives an individual power, so that it goes on, ending in the preparation of the material from which

Fig. 82.



Ova of common Cod (*Gadus morrhua*), showing limited fissuration of vitellus. *a*. Ova resembling nucleated cells. *b*. The same, with fissuration to number four.

the new being is evolved. Here it has acquired a positive character; and perhaps the best evidence that segmentation to a definite end lies at the bottom of the primitive development of all new individual beings when produced by means of eggs, is the fact that it occurs most extensively in those portions of the embryo that afford the highest expressions of the animal as such, viz. the nervous system.

The changes supervening upon this vitalizing process of segmentation, and which are the working out of the future embryo and the allocation of its organs, constitute the science of embryology. They involve details as numerous as the types of the whole animal kingdom, and cannot, therefore, be taken up here. What I have here said of the animal ovum, belongs equally well to that of the vegetable. For this I cannot rely upon my own investigations, but trust to the excellent researches of Robin.*

We will now refer to the sperm-cell, the analogue of the ovum. First, it is seen as a simple, nucleated, epitheloid cell. It increases, and after having attained its adult size, it experiences processes which mark its vitalizing character. This consists, as in the ovum, of a segmentation of the nucleus or vitellus, first into two, then into four, eight, &c. parts, the parent vesicle expanding in the meanwhile. So that, after a time, we find the parent vesicle filled with many

* See an article by Robin, in Compt. Rendus, 1849, titled "Ovum, the existence of an, as well in the male as in the female of Plants and Animals; producing in the one case spermatozoa, and in the other the primitive cells of the embryo."

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nucleated cells. These, of course, are of a uniform size, and usually of an even number of the multiples of two.*

The next changes are those concerned in the immediate formation of the spermatic particle, which always occurs according to one of two modes, the character and peculiarities of which will be made clear as we proceed. The parent vesicle is filled with nucleated cells, produced by segmentation of the nucleus or vitellus. This is the point from which we will now start. According to one method, and which I have called the *special-cell mode*, the process of division here ceases, and *in* each of these cells is produced a spermatic particle. But according to the other method, and which I have called the *fasciculus mode*, a kind of liquefaction or farther subdivision of these cells ensues, until the whole substance is reduced to a fine granular mass. Then out of this mass, by longitudinal fissuration, the spermatic particles are produced; as, if I may use this homely comparison, a stick of timber is split, with the grain, into sticks; the whole forms a fasciculus, and hence the name of this mode. After this general statement we will look at the subject a little more carefully.

It will be seen that the preliminary changes of the genesis of the spermatic particle are identical with those of the ovum. This great fundamental fact at once establishes a unity throughout nature in the process of reproduction. To settle this fact, my own observations have extended over the entire range of the animal kingdom, embracing most of the grand types of structure. Beside, I am now extending it to *species*, as rapidly as time and opportunities will permit. The fact, however, I now consider as well authenticated as any in histology.

Our next inquiry is, how is the spermatic particle formed? According to the *special-cell mode* it is *a metamorphosed nucleus*. Of this I have satisfied myself beyond a reasonable doubt. It can be well observed among the mammalian vertebrates, or in the Arachnida. According to the *fasciculus mode*, the particles are formed, probably, from an arrangement in a linear series of the granules produced by the extended subdivision. These unite, and, blending, form continuous threads. But this is a point of which I am far from being equally well satisfied. The phases of this change can be best observed in

* Sometimes this segmentation is not regular, and then the number of the daughter cells does not correspond to the multiples of two; thus, there may be seven instead of eight, one of the cells not segmenting. This is sometimes observed likewise with the ovum.

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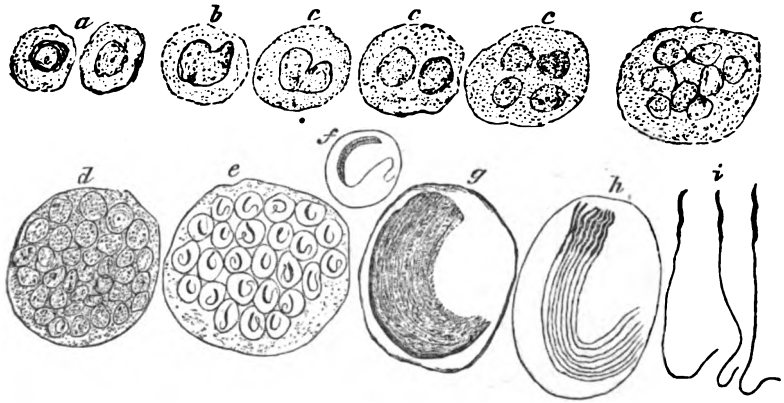
the *insessorial* birds. It may be asked if, in this case, it is not the nucleus of the pre-existing cells which is metamorphosed? To this

Fig. 33.,



Formula of the two modes of sperm-genesis. *a*. Epithelial cells of the testicle nucleolated, which are to be sperm-cells. *b*. The same with the nucleus undergoing fissuration. *c*. The fissuration still progressing. *d*. The parent-vesicle filled with cells, the result of subdivision. At this point the fissuration ceases, when the spermatic particles are produced according to the *special-cell mode*. *e*. Formation of spermatic particles according to the *special-cell mode*; each divided cell producing one particle. *f*. One of the cells still higher magnified, showing how a spermatic particle replaces the nucleus. *g*. Formation of spermatic particles according to the *fasciculus mode*; the granular mass being the result of the liquefaction of the cells of *d*. *h*. The formation taken place, the spermatic particles lying in a bundle. *i*. Spermatic particles isolated, showing plicated aspect of head.

I should be inclined to answer negatively; and the evidence against any such supposition is, 1st, you perceive the whole granular mass to be divided in this way; and 2d, the fascicular manner in which the spermatic particles are found in the parent-cell could not, seemingly, in very many instances, have been attained, only by such a mode of formation. It is, however, a remarkable fact which I have observed, that the number of spermatic particles thus produced, corresponds with the number of cells existing before the granular liquefaction takes place. Thus seeming to show that the individuality of the cell is not lost by this last change, but appears again with a certainty in the result produced. Thus, for instance, a parent sperm-cell contains sixteen vitelline cells; these liquefy and become a granular mass, but out of this last are produced sixteen spermatic particles. There is not, therefore, a real unity in their genesis after the preliminary changes have occurred, if we are to judge from appearances alone; but if we consider the identity of the nature of the cell and its nucleus, as formerly established, this unity is established.*

* Since the above was written, some special investigations I have made on the spermatic particles of insects, would lead me to think the point still an open one, if all

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Fig. 34.

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Figures of spermatic particles of a representative of each family throughout the animal kingdom, showing the variety of forms met with, beginning with the lowest and ending with man. 1. Astrangæ Danæ. 2. Alcyonella (nov. sp.). 3. Tubullaria laryngæ. 4. Actinia marginata. 5. Asterias spinosa. 6. Echinocidaris punctulata. 7. Mellita quinquepora. 8. Tænia (nov. sp.). 9. Lumbricus terrestris. 10. Ascidia amphora. 11. Botryllus stellatus. 12. Polyclinum (nov. sp.). 13. Ostrea edulis. 14. Mytilus edulis. 15. Anodon fluviatilis. 16. Unio complanatus. 17. Eolis Bostoniensis. 18. Buccinium obsoleta. 19. Limax agrestis. 20. Pilumnus Harrisii. 21. Pagurus longicarpus. 22. Epeira vulgaris. 23. Hæmatopinus suis. 24. Melolontha subspinosa. 25. Antiope vanessa. 26. Libellula grandis. 27. Platessa flesus. 28. Leuciscus chrysoleucas. 29. Salamandra symmetrica. 30. Tropidolepis undulatus. 31. Rana pipiens. 32. Heterodon niger. 33. Emys picta. 34. Gallus domesticus. 35. Muscicapa tyrannus. 36. Fringilla melodia. 37. Didelphis virginiana. 38. Neotoma floridana (lateral view). 39. Sciurus hudsonicus. 40. Capra capricornis. 41. Canis familiaris. 42. Man (lateral view). From 1 to 7, and from 10 to 16, inclusive, magnified 1500 diameters; the remaining, 350 diameters.

The SPECIAL-CELL MODE of the genesis of spermatic particles occurs in the *Mammalian Vertebrates;* all the *Grallatorial Birds*, also the *Accipetres* and *Scansores;* in the *Chelonian, Anourous Batrachian, Saurian* and *Ophidian Reptiles;* in the true *Osseous Fishes;* in the Arachnida; the families *Libelullidæ, Phryganeidæ, Phasmadæ*, and *Andrenidæ* of the Insecta; in all the *Crustacea;* in the *Gasteropod Mollusca*, and in all the *Radiata.*

The FASCICULUS MODE occurs in the *Insessorial Birds;* the *Caudate Batrachians;* the *Plagiostome Fishes;* the *remaining families* of the Insecta; the *remaining Mollusca;* the *Annelida.*

The spermatic particles eliminated according to the *special-cell mode* generally consist of a staff, to which is attached a delicate tail.

On the other hand, those produced according to the other mode have a filiform aspect, there being generally no distinct division between the staff and a tail.

The variety of forms, of which each animal has one, and one only, is quite extended, and presents to us a field of study having not only the deepest interest, but the highest zoological importance, and especially as relating to classification; for as the spermatic particle is the simplest representative we have of the potential whole of the animal, so may we well regard its forms as constituting the basis of a classi-

spermatic particles are not developed by the *special-cell mode*, being therefore metamorphosed cell-nuclei, and subsequently grouped in a regular fasciculate form. Thus, the very long filiform spermatic particles of some of the Lepidoptera, I have seen developed in special cells, while in other cases these particles may be observed in such a perfect fascicular form as to indicate that they were developed in bundles; however, these bundles are never observed within parent cells, as is true of those of the sparrows. In these last (sparrows), both the size and the length of the particle would seemingly preclude the view of their special-cell development; for, in some instances, the number of particles thus grouped in a parent cell was such that had they had their development in cells specially, these cells must have been so large that a corresponding number could not have been contained in that parent cell. But the subject needs a careful revision as to this point.

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fication founded upon the simplest and most natural data. But both a description of these forms, and a detail of these classific relations, is a subject of itself alone.*

There can now be no reasonable doubt that the function of the spermatic particle is that solely of fecundation, and that this last is produced by it alone. In the first place, the morphological identity of the two particles, the *ovum* and *sperm-cell*, as I have already pointed out, would of itself lead us to infer that the *special* product of the latter was for individualizing the whole of the former. In fact, such an inference would be in perfect accordance with our highest ideas of the unity pervading natural phenomena. But, aside from this induction, the point has been settled by direct observation.

The experiments of Spallanzani† proved this point as clearly as it well could be at that period. Since then the observations of Kölliker,‡ Newport,§ and Agassiz,|| have conclusively settled the matter. The evidence consisted in having the ovum so situated that nothing of the semen but the spermatic particle itself should come in contact with it. I will add to the above the evidence of my own observations, which serve not only to elucidate the above-mentioned point, but also to show that one, or at least a few particles are sufficient for a single ovum. In some of the low Acaridæ, as, for instance, those of the genera *Sarcoptes* and *Dermaleichus*, the generative organs of both sexes are situated on the inner surface of the abdominal sac. In the female these consist of ova, and in the male they are sperm-vesicles.

Now the number of ova and sperm-cells correspond quite closely; so that while in some species you find ten or twelve ova, there appear to be only about the same number of spermatic particles.

There is no semen proper, for the sperm-cells discharge their contents as spermatic particles, and the only liquid is that which belongs to the cells. The long period of copulation of these animals is secured by the strong anal hooks of the male fastening into the abdomen of the female.

Here, then, just the experiment you desire, is performed by na-

* For a farther discussion of this point, see a paper of mine, Relations of Embryology and Spermatology to Animal Classification. Proceedings of the Amer. Assoc. for the Advancement of Science. Albany meeting, 1851, p. 312.

† Spallanzani; Nouvelles Recherches sur les dicouv. microscop. London, 1769.

‡ Kölliker; Die Bildung der Samenfäden in Blaschen, Nuremberg, 1846.

§ Newport; Philosoph. Transact. 1851, p. 169; and Proceed. Roy. Soc. June 17, 1852.

Agassiz; Oral communication.

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ture; and a better proof of the sufficiency of a few particles could not be obtained.

From similar experiments, it has been shown that, in the act of fecundation, the *simple contact* is enough; in other words, the spermatic particle loses none of its material form by discharging its function of vitalization.* If it is asked, *how* this function is performed, to this it must be replied, that this has not yet been reached by physiological inquiry. And what is still more worthy of remark, it is difficult, in the present state of our relations to science, to conceive how we shall ever be much better able to seize hold of the intimate nature of these phenomena. We may call it by this name or by that, and such words may be adopted, although they serve to cover up our ignorance; for they are, in some way, and to some extent, expressive of conditions which the scientific mind by analogy alone can comprehend. But, at the same time, it should be remembered that the *ultimata* of all science having anything to do with vitality, must ever rest on words or conditions equally as vague and unsatisfactory.

However, some special considerations on this point will be offered in the philosophical portion of this work.†

* See farther, a discussion of this intricate subject in Silliman's Journ. Nov. 1853.

† The bibliographical relations of the subject of spermatic particles here deserves notice. The older anatomists, Leeuwenhoek,* Ledermuller,† Spallanzani,‡ Gleichen,§ and others, not only recognized their existence, but even studied their form and peculiarities. This is especially true of Spallanzani. At the beginning of the present century, they excited considerable attention, and were especially studied by Prevost and Dumas,‖ and others; and the same is true of the time of the improvement in microscopes, about the year 1830. Dujardin,¶ Czermak,** Treviranus,†† Von Siebold,‡‡ and Wagner,§§ each contributed considerable towards the advancement of these studies. But, on the other hand, they tended to retard its true progress, by asserting that these particles are *animals*, a view which was supported by Valentin,‖‖ Gerber,¶¶ Schwann,*** Pouchet,††† and others. To Wagner‡‡‡ and Kölliker§§§ we are indebted for a complete

* Leeuwenhoek; Anatomia, seu interiora semen, Lugd. 1687.

† Ledermuller; Physikalische Beobachtungen du Samen thierchen. Nuremberg, 1756.

‡ Spallanzani; Nouvelles Recherches sur les décour. Microscop. London, 1769.

§ Gleichen; Abhandlung ueber du Samen, und Infusions thierchen. Nuremberg, 1788.

‖ Prevost et Dumas; Annal. des Sc. Nat. tom. i. and ii.

¶ Dujardin; Annal. des Sc. Nat. tom. vii. pp. 291–297.

** Czermak; Beitrage zur Lehre von den Spermatozoen. Vienna, 1833.

†† Treviranus; Tiedemann's Zeitschrift, vol. ii.

‡‡ Von Siebold; Müller's Archiv. 1836, s. 232, and 1837, s. 381.

§§ Wagner; Fragmente zur Physiologie du Zeugung, 1837.

‖‖ Valentin; Repertorium, 1837, p. 134.

¶¶ Gerber; Allgemeine Anat. &c. p. 210.

*** Schwann; Mikroscop. Untersuch. &c. Berlin, 1839.

††† Pouchet; Théorie positive de l'ovulation spontanée, &c. Paris, 1847, p. 321.

‡‡‡ Wagner; Hist. de la generation, &c. Bruxelles, 1841, p. 26.

§§§ Kölliker; *loc. cit.* Berlin, 1841.

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The great fundamental fact at which we have arrived by these studies, is the morphological identity of the ovum and sperm-cell; and, by way of conclusion, I subjoin, in a tabular form, their expression, in a brief manner:—

The Sperm-Cell.	*The Ovum.*
1. Is a nucleolated cell.	1. Is a nucleolated cell.
2. The nucleus, increasing and becoming granular, undergoes segmentation.	2. The nucleus (vitellus), increasing and becoming granular, undergoes segmentation.
3. The result of this segmentation is, that each of the subdivided cells forms a spermatic particle.	3. The result of this segmentation is, that *all* the subdivided cells, by a metamorphosis, form an *embryo*.
4. The function, then, of the sperm-cell, is to eliminate the vitalizing spermatic particles.	4. The function, then, of the ovum, is to eliminate the vitalized product, the *embryo*.
5. In the lowest sense of the term, the spermatic particle is *alive;* that is, it is an organized acting form. But it is so only in the lowest sense, since it holds no relations whatever with the external world. On this account, it can never be an *animal*.	5. In the lowest sense of the term, the embryo is *alive;* that is, it is an organized acting form. But it is so in the lowest sense of the term only, since it sustains no relations to the external world. It is *not an animal* until it does.

The above views were published by me in a memoir, presented to the Amer. Acad. of Arts and Sciences, July 1851: Researches on the Origin, Mode of Development, and Nature of the Spermatic Particles among the Four Classes of Vertebrated Animals.

NOTE.—In the foregoing account of the morphological relations of the spermatic particles as cell products, nothing was said relative to analogous phenomena in the vegetable kingdom. The subject of the existence of true spermatic particles in plants has been such a mooted one, that I did not think proper to introduce it in the text. But for my own part, I believe that, as to morphological conditions, the phenomena of reproduction in the animal and the vegetable kingdoms, are analogous throughout. In the study of nature we must not let names confuse our ideas of things; and, call them what you will, I think there is no doubt that there are in plants particles for fecundation, exactly corresponding to the spermatic particles in animals. The doctrine of vegetable spermatic particles has been treated with ridicule by Schleiden (Principal of Scientific Botany, &c., translated by Lankester, London, 1849, pp. 99, 359); but aside from the weight of analogy, recent investigations have shown that it is not so easily disposed of. Without discussing the subject historically, I will allude first to the remarkable memoir of Robin, on the existence of an ovum, as well in the male as in the female of plants and animals; producing in the one case spermatozoa, and in the other the primitive cells of the embryo; see Compt. Rend. 1849. The object of this memoir is to show the con-

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explosion of this view, besides otherwise treating the subject in a very profoundly physiological manner. They have figured and described many forms, and given the whole subject an individuality it never can lose.

But by far the most comprehensive article that has appeared on the subject of the spermatic particles, is that of Wagner and Luckardt, in the Cyclopæd. of Anat. and Physiol. Art. Semen.

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plete morphological identity of the male and female reproductive primordia, throughout all organized forms. Not only are the cell-phenomena of the male and female products of the same kingdom identical morphologically, but also of the same sexes of the different kingdoms. Leaving out the weight of analogy, *à priori*, the observed processes of cell life clearly indicate that there are in plants male particles of fecundation corresponding to the spermatic particles with animals. These are the pollen-grains with the phanerogamia, and the contents of the antheridia with the cryptogamia.

See, for a farther consideration of this subject, a memoir of Thuret on the anthers of Chara, &c. Ann. d. Sc. Nat. xiv. p. 65, but especially the work of Hofmeister, "Vergleichende Untersuchungen der Keimung, Entfaltung und Fruchtbildung höherer Kryptogamen, und der Saamenbildung der Coniferen." Leipzig, 1851.

Cilia.—We now come to the consideration of another part of our subject, of the products of epithelial cells. I refer to the formation of *cilia;* and although it is not, it is true, an endogenous production of cells, like those we have just considered, yet its discussion cannot be better taken up than in this connection.

These cilia are solely the product of epithelial cells, and give to those on which they are found, a function different from that of the true secerning organ. They are minute teat-like processes of the cell-membrane, capable of moving backward and forward, and therefore have a waving oscillatory motion. These cilia exist on the epithelial cells of those membranes which have an external outlet, and therefore may there be regarded as truly excretory organs; for by this oscillatory movement towards the external opening, they are able to convey an astonishing amount of material. As bearing out the homology of the epithelial cell, and the ovum, they are found existing on the surface of the latter, especially among the Mollusks and Radiates; and here their potency as organs of locomotion cannot be appreciated, unless one has studied these lower forms, for they really constitute the active agents of their life and motion.

When existing on cells, they are upon one of its surfaces only, studding its crown, or the part opposite that by which the cell is attached to the basement-membrane. When existing upon ova, they generally cover its entire surface. Their formation upon epi-

Fig. 85.

Formation of Cilia from Mucous Membrane of Frog (*Rana pipiens*). *a.* A cylindrical epithelial cell, the upper portion of which presents a bulging. *b.* The same, still farther progressed. *c.* This "bulged" portion beginning to split. *d.* Cells on which this splitting has taken place, forming cilia.

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thelial cells, I have had the good fortune to observe, and their genesis here may be considered as an expression of their formation everywhere.

A nucleated epithelial cell exists upon a mucous membrane; if cilia are to be formed from it, it lengthens and becomes somewhat cylindrical in shape. A part of the end which is free then becomes a little constricted, or partially separated from the rest, as though a minute thread had been drawn around it. This constricted portion is the part out of which the cilia are to be formed. It then divides into fasciculi, each of which splits up into cilia.*

The formation of cilia, therefore, is only the splitting up of *a portion* of a cell into filaments. But, on the other hand, there is reason to believe that each of these cilia is tubular; in other words, hollow like the finger of a glove. If this is true (and I can assure any one that it is a point exceedingly difficult to settle definitely), there is a deception in the appearances presented; for they then must be regarded as *ex* vaginations of the cell-wall, or the pushing out of teat-like processes from its surface; and this could well take place in that constricted portion which I have described.† But this is a point needing much more study and attention. However, from what I have really observed, I must, for the present at least, regard cilia as *digital portions of cell-membrane.*

They therefore should have the capacity of cell-membranes; and when this is fully considered, it can cause no wonder that all the varieties of motions here observed should be alone due to the intrinsic power of the cells themselves, and without the agency of those tissues by which similar phenomena in higher and more compound life are necessarily connected.

Lasso-cells, or the tactile and prehensile cells on the surfaces of Medusæ.—It may seem somewhat strange that I should include these cells under the head of epithelia. But, from a somewhat careful study I have of late given them, I am inclined to the opinion that, histologically, this is their nature. And as such, a consideration of

* The observations of Kölliker are exactly to the same effect. His investigations were made upon the epithelial cells of the oviduct of Planorbis corneus; *vide* Beitrage zur Kenntniss, &c. Berlin, 1840, p. 33, pl. i. fig. 12. My own observations were made upon epithelia of the mucous membranes of Batrachians.

† For farther details on this interesting subject, I may refer to a paper published some years since. See Histological Researches on the Development, Nature, and Function of Epithelial Structures, Amer. Journ. of Med. Sci. N. S. vol. xx. p. 70.

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their products may be properly introduced here. Even were this last not true, I do not think I could well desist giving them here a careful review, on account of the many beautiful phenomena presented.

And without forestalling my subject, I may say that the studies I have made of these lasso-cells have been to me a source of more delight, and has given me a broader view of the signification of elementary forms, than any other of equal magnitude I have ever taken up.

By lasso-cells, I mean a kind of cell which is found upon the body, and especially upon the tentacles of the *Polypi* and *Acalephæ*, as well also as upon some of the *Mollusca*, and which also exist upon the external surface of many of the internal organs. These cells are generally of an oblong oval shape, of a variable size, and contain in their interior a long filament coiled up upon itself. This filament is thrown out, and thereby serves as a prehensile organ by winding around the minute object to be caught.

This cell is attached by one surface, one of its short diameters, and from the other extremity, the filament passes out. This filament is a part and parcel of the cell, a continuation of its membrane by a slender tube coiled up in the interior; its expulsion, therefore, is by its inversion, or a turning inside out. In other words, and perhaps expressing it more clearly, the whole is a cell, which has a long process of itself extending into its interior, and which is thrown out by inversion, exactly as may be a finger of a glove.

But to look at the subject a little more particularly. This filament is a very delicate tube, and has upon its surface, as observed when out of the cell, numerous little barbed points, which are arranged somewhat regularly, and all of which point backward towards the cell. However long and delicate this filament may be, these barbed points exist to its very extremity. This is a point which does not appear to have been before recognized, and which I did not perceive myself until quite recently, when working with condensed light. There appear to be two varieties of these cells; first, those containing nothing but the tube, and which, as such, connect with the cell; and second, those into which there is a kind of stigma, or stock, from which the filament proceeds, and which is the bond of connection between it and the cell. This stigma is about twice the size of the thread, has a somewhat ventricose shape, and, like the tube, has numerous points. It is of course inverted in being thrown out. Another difference between these two kinds of cells, is, that

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with the stigmated ones the thread is rather short, say from ten to fifteen times the length of the cell, while in those not stigmated the thread is very long, and often sixty to eighty times that of the cell.

That this slender tube is thrown out by a process of inversion, is proved by watching it, as Prof. Agassiz and myself have repeatedly done, and also by the fact that, when in the cell and coiled up, the barbed points on the thread, and especially upon the stigma, where they may be well seen, point forward, or, rather, in the opposite direction to what they do when it is uncoiled. The beautiful design in these barbed points upon this thread cannot fail to strike us with admiration; for, pointing backward, they serve well to retain the object around which the thread is thrown. These cells are truly prehensile organs, and in handling these animals, it is from them that the itching, nettling sensation is produced, and in some of the Actinia they are so large that you can feel the threads on your finger.

These coarser forms, however, are not the ones in which these phenomena of the lasso-thread can be best studied, but with the coral polypi (*Astrangæ Danæ*, Ag.) they can be most thoroughly made out.

But before I proceed any farther, I will refer for a moment to the history of these forms, that we may see what has hitherto been known on this subject. Wagner* appears to have been the first to notice them with the Actinia, and he has described them as spermatozoa. Subsequently, they were studied by Erdl,† who traced their presence in other genera, and described some of their peculiarities. Quartrefages‡ also observed and carefully figured them. In the same way they have been noticed by Siebold.§ With the exception of Wagner, all these authors regarded them as prehensile organs, serving to seize upon and secure their food. However, all their more minute and more beautiful relations were not known, and this part of the subject has been recently taken up by Professor Agassiz,|| who has quite exhausted it; and, in so doing, has brought into notice phenomena and physical peculiarities which, in point of beauty, have, I think, no equal in the whole department of histology.

* Wagner; Wiegmann's Archiv, 1835, ii. p. 215, pl. iii. fig. 7.

† Erdl; Müller's Archiv, 1841, p. 423, pl. xv. figs. 3, 6, 8, 9.

‡ Quartrefages; Annal. des Sc. Nat. xviii. 1842, p. 81, pl. ii, figs. 4, 6.

§ Siebold; Comparative Anatomy, transl. by Burnett, vol. i. § 28.

|| Agassiz; From a forthcoming memoir on the coral polyps, to be published by the Smithsonian Institution; but see especially some notes to the translation of Siebold and Stannius's Comparative Anatomy, § 28. Agassiz's drawings are unsurpassed.

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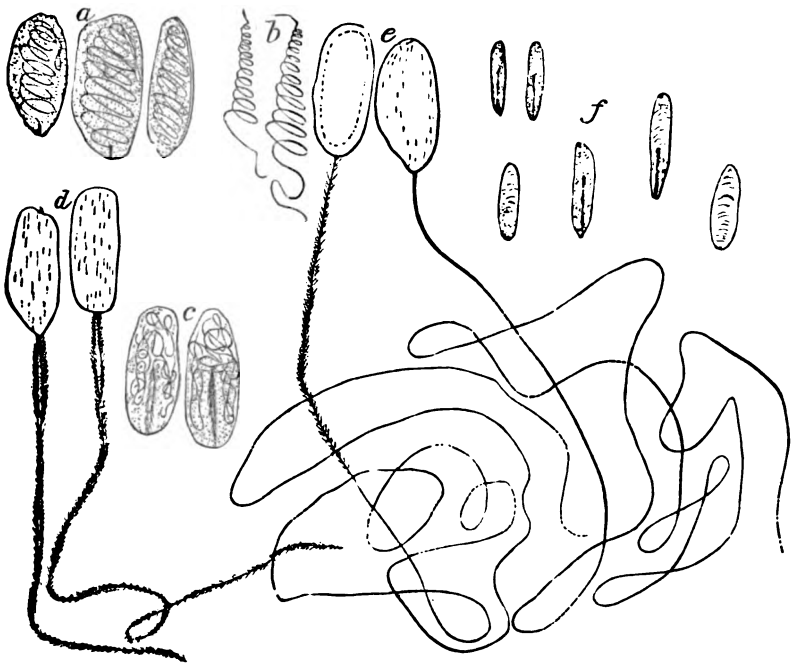
When such is the condition of the subject, it might well be regarded as out of taste for me to take it up with any pretensions for its farther elucidation. But it has been my good fortune to make out clearly many points hitherto obscure; this success, however, I attribute to my superior instrument and some other advantageous means for investigation. I have already mentioned the anatomical points which I have made out for myself; we will now refer to the genesis of this body, as I have had the good fortune to observe it. It exists, first, as a small sphere, in fact, a cell (and an epithelial one too, I think), having probably the same mode of primary formation as other epithelial cells. Gradually, it assumes an oblong shape, and has a dark-lined appearance in the centre. This, I think, is the nucleus, or at least is dark granular matter occupying the central portion of the cell. At the next stage, you perceive the spiral thread or tube faintly appearing in a spiral condition, and, as the

Fig. 86.



Lasso-cells and their formation. *a*. Cells of the simple kind with coil inside. *b*. The cell being dissolved by acid, the coil is left unharmed. *c*. Cells of the stigmated kind, the barbs on the stigma seen to point downward. *d*. The same, with the lasso thrown out, the barbs on the stigma pointing upward, the whole having been everted. *e*. Cells of the simple kind with lasso thrown out, the lasso being very long. In both kinds the lasso is barbed throughout, and so fine that no pen can mark them. *f*. The formation of lasso by deposition of granular matter on inner surface.—From Astrangæ Danæ.

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cell increases, so this thread increases likewise, and becomes more and more distinct until fully formed, when the transparency of the cell-wall allows it to be seen most perfectly. It then appears coiled in a very perfect manner, the width of each coil being about that of the short diameter of the cell. It would therefore appear that the nucleus is the starting-point at which the formation of this tube commences. In the stigmated variety it persists to a certain extent on its original form, but in the other kind it is entirely metamorphosed into the tube. This may account, in part, for the greater length of tube in the latter than in the former. In my opinion, this tube is formed by a spiral deposition of matter on the inner surface of the cell, exactly like the formation of the spiral vessels or threads in vegetable cells by the deposition of cellulose. Not only is this view supported by analogy, but also from the fact that the formation, when first occurring, is invisible, being probably finely granular albuminous matter, exactly as is true of cellulose in the vegetable cell. Farthermore, the formation of the spiral threads in vegetables is of nuclear origin, and such we have seen to be the case with these cells.

It is true that the filament is *solid* and not tubular in the vegetable cell, neither has it any connection with the cell-wall by continuity. But this fact does not invalidate the view I have taken; for if a long fibre is to be formed inside of a cell, and to be capable of being projected from it, and yet retain its connection with the cell, it is difficult to conceive how the formation could have been otherwise; and as the very first phenomena are invisible, it seems proper that we should explain them on the grounds of analogy. But it may be asked, what is the signification of all this? As it appears to me, it is this: You have here an endogenously formed cilium; in other words, one form of a ciliated epithelial cell, having a single cilium which is formed *inside* of the cell instead of *outside*, as is the case with ordinary ciliated cells; for, the cilia on common cells we have seen to be only external protrusions of the cell-wall, while in the present case, it is, in one sense, an internal protrusion. This well accords also with the idea of its true epithelial character, which we have asserted on other grounds.*

* I am not yet fully determined as to the morphological relations between these lasso-cells with their filament, in animals, and the cells with spiral formations, in plants. There is, however, this difference in the outset, as to the structure of the filament. We have seen it to be tubular with the lasso-cells, but in plants it is not hollow but solid, and has a circular, an elliptic, or a quadrangular section. Moreover, there is

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The spiral, in all my examinations, has invariably been from the left to the right. Thus formed, it is ready to be thrown out, and this projection takes place in the following manner: By some irritation of the cell, probably altering the endosmotic condition of its membrane, there is tendency to expel its contents; and, as I have before said, the filament is thrown out by being everted. This is accomplished so quickly as to almost entirely escape the vision, and when all run out, the whole has the aspect of a sac-like body, into which is inserted a long delicate tube.

It may be asked if the barbed points experience this eversion in like manner? My opinion is that they do *not;* and I base this upon the fact that, when the tube is coiled, they occupy an inverse relative position to the one when uncoiled. They would, therefore, be formations *in* the tube rather than *upon* it.*

By way of conclusion, I will add that when one sees a tube, the *whole* thickness of which is not more than $\frac{1}{25000}$ of an inch, and whose walls are studded with fine and regularly arranged points, and also having a delicacy so great that the whole is capable of being almost instantly turned inside out, he naturally views with wonder the almost infinite plasticity of animal membranes, and his mind is so opened to the wondrous capability of organized forms, that there

no evidence that these filaments in vegetable cells are the result of an internal protrusion of the cell-wall, but they appear to be rather the result of a granular deposit on its internal surface. These remarks apply to the common "spiral vessels," as they are called. But the spiral fibres observed in the Antheridia of Charas, Mosses, and Ferns, appear to be of a different character, and if not shown to be vegetable spermatozoa, may prove to be morphologically analogous to the lasso-cell contents.

Upon the peculiarities of structure of the spinal fibres or vessels in plants, see Schleiden's Principles of Scientific Botany, &c. transl. by Lankester, London, 1849, p. 42; Mulder; the Chem. of Verget. and Animal Physiol. transl. by Johnston, Edinb. 1849, p. 420; and upon the last topic (these fibres as seen in the Antheridia of Charas, &c.) see a memoir of Thuret, Ann. d. Sc. Nat. xiv. p. 65, and Meyen's Physiologie, iii. p. 223.

* The following are some of the genera, in all the species of which the lasso-cells may be found: *Actinia, Meandrina, Manicina, Astræa, Campanularia, Syncoryne, Coryne, Cyanæa, Aurelia, Thaumatias, Tiaropsis, Hippocrene, Sarsia,* the Hydroid and true Medusæ. Without doubt, farther researches will show that very many other genera are similarly endowed.

For the biography of the lasso-cells, see, beside the authors already quoted: Wagner, Icon. Zoot. tab. xxxiii. figs. 8, 10, 11, A_1 B, C.; also in Wiegmann's Arch. 1841, i. p. 39; Ehrenberg, Wiegmann's Arch. 1841, i. p. 71, tab. iii.; Kölliker, Beitr. zur Kenntniss d. Geschlichts, v. &c. p. 41; Milne Edwards, Ann. d. Sc. Nat. xvi. p. 223, pl. viii. fig. 9; Philippi, Müller's Arch. 1843, p. 62, tab. v. fig. 9; and Will, Horæ tergestinæ, &c. pp. 79, 81, tab. ii. figs. 23–25.

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ensues a train of reflection, than which nothing can be more serviceable or conducive for his broadest comprehension of physiological science.

II. Chyle and Blood-Cell Function.

In the somewhat extended remarks on the genesis of these cells, there was necessarily included some statement of their function. This, however, was not definite, and the importance of the subject demands that it should be considered under a separate section. We have seen that there are various and widely dissimilar opinions as to their genesis. A still greater want of uniformity exists as to opinions of their function. At first, it certainly appears singular that the use of so common a product should remain so long not definitely and indisputably settled. But those who have been at all engaged on these subjects know full well the difficulties attending their satisfactory study. However, I think there are now sufficient data for a safe and legitimate conclusion respecting this matter. In order that the full idea of the subject may be embodied in one consideration, I shall run over the ground in a somewhat comprehensive manner.

We have seen on a preceding page that the nutritive circulating fluid has a rank quite corresponding to that of the animal in which it is found. In the Radiata, it appears little else than the triturated food, from which the nutritive portions are extracted more by pressure than by solution, for there is no elaboration except that of simple division. The circulating nutritive material is only *chyme*, and these animals cannot be said to have any blood.

Among the remaining Invertebrates a special elaboration takes place; for, beside the oral trituration there is a digestion, and the nutritive liquid is the result of this last. It has a higher character, which is expressed in the presence of corpuscles.

In a low sense of the term, these animals can be said to have blood, but this blood is only *chyle* in circulation. As we pass from the Invertebrata to the Vertebrata, an additional element is introduced, for not only does the circulating liquid have chyle, but true blood also; and which last they have, not because they are vertebrates, but because they are of a higher type of structure, and demand a more extended nutrition.

On a preceding page, I have given the reasons why we should regard the white corpuscle of the blood of vertebrates, and the common corpuscle of the blood of the invertebrates, as analogues (not homo-

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logues) of each other. The coloured corpuscle belongs to the Vertebrata alone.*

It is asked what is its use? The answer to this question includes that of all the corpuscles of the blood; and a very natural way of seeking it, is by consideration of the difference of the nutritive phenomena in these two grand divisions of the animal kingdom.

This difference consists in the amount and extent of the oxygenation of the tissues. The relations of this oxygenation to tissue are mostly those of a vitalizing character. Among the invertebrata, the simple function of nutrition, or the adding of plastic material to be elaborated by the tissues, is performed by the circulatory system, simple as it is: while the conveying of oxygen is accomplished by another apparatus, which of itself has as wide or even a wider dissemination than the former. But with such a plan the oxygenation cannot be very thorough and minute. The general structure of the vertebrates, on the other hand, does not readily admit of a similar arrangement, and even if it did, it could not meet the activity of their functions, which demand a more minutely distributed form of nutrition and oxygenation. This is accomplished by the nutritive and oxygenating systems being contained in one, so that whenever there is borne the means of nutrition, there is also borne those of oxygenation and vitalization. This general statement prepares us for the view which we are about to take; that it is quite natural that such a circulatory liquid should possess two elements, viz.: 1st, those of simple nutrition, consisting of the plasma and the colourless corpuscles; and, 2d, those of oxygenation, consisting of the coloured corpuscles.

Now, if we are led to adopt this view, from a survey of the nutritive functions as occurring in invertebrate animals, we find it confirmed by an analysis of the phenomena met with in all the higher forms. Thus, in the lowest vertebrates, which seem to exist on

* I scarcely need mention that when coloured blood is met with among the Invertebrata, the colouring matter is in the plasma and not in the corpuscles. Such is true of many of the Annelida, where the corpuscles have the same pale, granulated aspect as in the Insecta. Moreover, this colouring matter seems to hold other than necessary relations with this liquid, for Milne Edwards (*Annal. des Sc. Nat.* tom. x. p. 197) has shown that there are Nemertini having colourless blood.

I cannot regard the observation of Wagner (*Zur vergleich Physiol. d. Blutes*, pt. i. p. 8) as correct, when he alleges that he has seen real red corpuscles in *Terebella*.

Both Leeuwenhoek and Hewson seem to have laboured under a similar mistake in regard to the blood of grasshoppers and larvæ. (See *Hewson's Works, Syd. Soc. ed.* p. 234.)

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the confines of the two kingdoms, the myxinoid fishes, and with which oxygenation is very feeble, and almost wanting, the coloured corpuscles are few.* As we pass up the scale the same relation exists. Thus, in the class of birds, which, from the relations they sustain with the external world, have a vitality of the tissues generally not exceeded by that of any other animal in the whole animal kingdom, giving them the greatest amount of strength and agility with the least weight; in these, you find the largest number of coloured corpuscles. In man, we notice the vitality and elasticity of his whole system in proportion to the amount of red or coloured corpuscles in his blood.

We cannot, therefore, resist the conclusion that the coloured corpuscles are necessarily connected with the process of respiration or vitalization by oxygen, of the tissues—in fact, are the *oxygen-carriers*, the *travelling respiratory organs* of this division of the animal kingdom. This conclusion, thus arrived at, is supported by the following facts, of the truth of which any one may easily satisfy himself:—

Dark blood both in and out of the system assumes a lively reddish hue upon being exposed to oxygen, and at the same time carbonic acid is disengaged. The mere serum, which contains no corpuscles, has none of these phenomena.

The consumption of oxygen and the disengagement of carbonic acid is in direct ratio with the amount of corpuscles.†

In the embryo, as we have already seen, these corpuscles are not coloured until they come in contact with the air directly or indirectly; we may therefore regard their red colour as consequent in some way upon their condition as oxygenizers; a fact borne out by all the phenomena of respiration and nutrition with which we are acquainted.‡ The immediate cause of this colour we know very well to be the presence of a chemical constituent called *hæmatin;* but why this should be red instead of blue, no chemistry has yet been able to decide. I

* *Vide* Yarrell, History of British Fishes, London, 1836, vol. ii. p. 468.

† Simon's Chemistry of Man, Sydenh. Soc. ed. vol. i. p. 154.

‡ Simon (Chemistry of Man, Sydenh. Soc. ed. vol. i. p. 120) speaks of blood-corpuscles being found in the thoracic duct of rabbits and horses. Thus apparently showing that these corpuscles become red before they enter the lungs. But I cannot regard such facts as at all influencing the truth of the opinion I have above expressed, for it is quite probable that these corpuscles were here extravasated. Thus, Dr. Davy (quoted in Hewson's Works, Syd. Soc. edition, by Gulliver, p. 277) found a small portion of red crassamentum in the thoracic duct of a man who died suddenly of apoplexy.

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think it pretty well decided that this colour exists in the cell liquid, and not in the membrane.

When these corpuscles thus formed are brought in contact with the air, their reddish tint becomes more pronounced, which is probably due to some chemical change in the hæmatin. They then absorb oxygen in virtue of their being cells, and thus laden they carry it into all the vascular tissues, and receive in return carbonic acid, which appears to displace the former. This rotation of function may give rise to that change in their form mentioned by Mulder,* viz. that the corpuscles of arterial blood are *bi-concave*, while those of venous blood are *bi-convex*. These phenomena, however, I have not seen, and it appears to me to be a most difficult subject of microscopical research; for the moment venous blood is exposed to the air, as being put under a microscope, it becomes arterial.

After all, it may be remarked that, in the present state of our knowledge, it is as difficult as it is hazardous to form any conclusions upon the causes of all the differences between arterial and venous blood. This much, however, I consider well established, that the red corpuscles are intimately connected with tissue respiration, and I shall here briefly state their function, as being that of *the vitalization of tissues, which are constantly experiencing the metamorphosis of life. This metamorphosis involves two prominent phenomena, the consumption of oxygen, and the elimination of carbonic acid; all of which is performed by the agency of the coloured corpuscle.*†

* Mulder; Chemistry of Animal and Vegetable Physiology, translated by Johnston, p. 342.

† I scarcely need say, that a variety of opinions have been entertained as to the function of the coloured blood-cell. Wagner,* Henle,† Wharton Jones,‡ and Newport,§ regard them as the elaborators of the fibrin; in fact, give them that function which I have ascribed to the white corpuscles. But this view appears to me quite untenable for several reasons. In the first place, fibrin is undoubtedly formed in the chyle, and in which no coloured cells exist. Then, again, in inflammation, the amount of fibrin is increased, without an increase (on the other hand, there is rather a diminution) of the coloured cells.

The opinion of Mulder‖ is even still different. He regards their function the production of an oxy-protein compound; for, he affirms that the blood-corpuscles are really surrounded with an envelop of oxide of protein, in the lungs. But these views, based upon chemical data almost exclusively, have not been generally received with favour. As for myself, I do not feel competent to discuss them.

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* Wagner; Physiology, translated by Willis, p. 448.

† Henle; Traité d'Anat. générale, &c. tom. i. p. 457, *et seq.*

‡ Jones; Brit. and For. Med. Rev. xiv. p. 597.

§ Newport; Proceed. Roy. Soc. Feb. 6, 1845.

‖ Mulder; Chemistry of Animal and Vegetable Physiology, translated by Johnston, p. 332, *et seq.*

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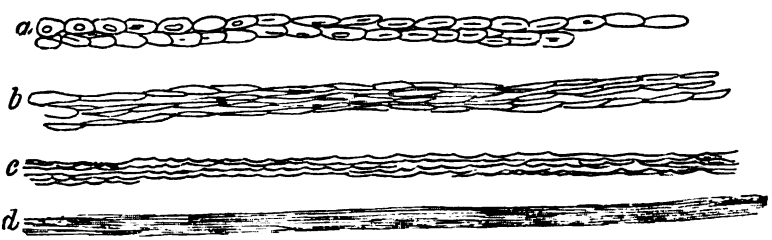
We have thus considered the blood-corpuscle as discharging a function as a simple individual cell, but there is one other point of its use which should be briefly referred to in this connection. I refer to the immediate transformation of these corpuscles into compound tissues. That this sometimes did occur, or in fact was the means by which tissues in general were formed, was long ago entertained by Martin Barry.* But the data on which this opinion was founded have not since been verified. Since then, the observations of Dr. Zwicky,† upon the metamorphoses of the thrombus, go to show that blood possesses the power of becoming organized and developing itself into a tissue. I have not studied the subject of the phenomena to which Dr. Zwicky alludes, but some appearances I have observed in the blood of Amphibia show me that it may be transformed into a fibrillated tissue; but whether it is to be regarded a normal or abnormal condition, I cannot say. Thus, the blood of frogs will often show these changes: At first, the corpuscles are floating about, or lying in a loose irregular manner in the field; next, they begin to arrange themselves in a linear series; then, a coalescence of the corpuscles take place, giving the tube a wavy outline on each side; this last finally disappears, and there appears then a more or less smooth fibrilla. The blood thus changed has a fibrillated, tough feel and structure. This, however, may be a function of reserve in nature, to meet the accidents of the life of tissues; but such phenomena are not involved in the ordinary round of nutritive action, as will hereafter be fully shown.

Fig. 37.



Blood of Frog, undergoing a kind of organization. *a*. Blood-globules arranged in a linear series. *b* and *c*. The same, changing into fibrillæ by flattening and lengthening. *d*. Arranged into fibrillæ, forming a fibrillated tissue.

It now remains for us to inquire the function and use of the white or colourless corpuscles found in the blood. We have already seen

* Barry; Philos. Transact. 1840, p. 601.

† Zwicky; Die Metamorphose des Thrombus, Berlin, 1845.

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that they are formed in the nutritive liquid which is derived from the food. And as formations of this kind I have been inclined to regard them, in both a physical and teleological point of view. Physically, they are the individual expressions of the formative power of that liquid in which they are found; and as such fibrin necessarily enters a good deal into their constitution, therefore, in this respect, they may be regarded as the organized fibrinous parts of the lymph—constituting the vehicle of fibrin in the blood—the elaborators of fibrin. On the other hand, teleologically, they hold certain future relations with that higher form of blood-cell, the coloured corpuscle. Chemistry has proved that the nucleus of this last is mainly composed of fibrin, and my preceding observations have shown us that it is *around* this white corpuscle, as a whole, or in part, or its analogue, that the coloured cell is formed. Thus, in the *oviparous vertebrates*, we have seen that the whole corpuscle serves as the nucleus of the coloured cell; while in the *mammalian* it is the elaborated product of this corpuscle, or its nuclei, which serves the same purpose.

This opinion of the function of the white corpuscles, which is based entirely upon a genetic and physiological view of the subject, finds its support in pathological phenomena. Thus, in inflammation, as the investigations of Andral and Gavarret* have shown, the quantity of fibrin is increased, and this increase is attended with a corresponding increase in the number of white corpuscles, but without an increase of the coloured cells. This I have repeatedly witnessed myself. But whether this abnormal condition of the blood constituents can be said to be due to an excess of fibrin *per se*, or to a want of its ultimate elaboration through the vitalizing agency of the coloured corpuscle, I do not know. But this much is certain, that when tissues begin to be inflamed their bloodvessels contain an excess of white corpuscles.† And when a large viscus, like a lung, is inflamed, this peculiarity becomes general, nearly all the chyle-corpuscles seeming to remain as such in the blood plasma.

These abnormal blood conditions will be more fully discussed hereafter, in their proper place.

I shall briefly state the function of the lymph-corpuscles to be—*To elaborate fibrin from the chyle plasma, and, at the same time, serving, either in whole or as parts, for the bases of the coloured cells.*

* Andral and Gavarret; Recherches sur les modifications de proportions de quelques principes du sang dans les Maladies, Paris, 1842.

† But this condition of things is due to general relations of the blood instead of local ones, as we shall hereafter perceive.

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III. Nerve-Cell Function.

The nervous tissue, taken as a whole, is both the originator and distributer of mental force. In the higher animals, its existence is necessary for what may be called mental manifestations; or, in other words, it is *the* tissue alone in and by which such forces are manifested. But in some of the lowest of the invertebrates this is not true, for in them no nervous system can be detected; yet they have motions as varied and adaptive as others of the same class, which have true nervous tissue.

The distribution of the nervous forces belongs to the tubular portion of this tissue, which has little other than mechanical relations; we shall not need, therefore, to allude to this part of the subject, but our attention will be directed to the cells themselves.

I scarcely need say that the nerve-cells are the producers of nerve-force, and that by them or their analogues, alone, it is eliminated. They are, moreover, to be regarded as the terminal, and not as the transitionary conditions of this tissue as an active one. By their agency, and probably acting as do other cells, viz. by endosmose and exosmose, the nervous power is eliminated; and it is their property to in some way produce this, just as it is the property of another cell to secrete milk. In a mere potential point of view the one instance is just as easily comprehended by the mind as the other. Beyond this I may safely say we know nothing, for here is the end of true physiology. It is certainly the highest philosophy that teaches us that there are boundaries to our knowledge even of the simplest things. We must learn *of* what *cannot* be known. Even were thought, as a cell-product, just as materially expressed as is milk or saliva, should we then know any more about the matter? We see an image on the retina, and the act of seeing is thus visibly demonstrated; but after all, material as it is, does it in the least explain vision? We have already seen that the layer of retina, exposed to the image, is composed of cells; yet as such they receive and conduct an immaterial something to the sensorium. This is a fact which we can appreciate, and which is well settled, but it explains nothing. We can as well conceive that material cells should eliminate immaterial forces as that they should receive immaterial images.

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CHAPTER V.

RETROSPECTIVE VIEW OF CELL-LIFE.

THE preceding account may be said to constitute a biographical sketch of cells in all their conditions of life. This has necessarily been a matter of detail, and we have scarcely paused in our course to consider the nature of the processes under description. In bringing the physiological part of our subject to a close, I have thought proper to take a survey of the whole matter, that we might learn the signification of cells in physiological science. This I shall do in as brief manner as possible. The great outstanding fact, which appears before us as the result of these studies, is, that there is a fundamental unity of all organization. This we have seen to consist in elementary particles, which in both animals and plants are formed upon a common plan. It was the opinion of Schwann and Schleiden, who truly originated this view, that this common plan consisted in the pre-existence of a solid, fundamental body (the nucleus), around which is formed a membrane, ultimately expanding and constituting the cell. It has been one of my objects, in the preceding pages, *not* to show that this view was not founded in fact, but to show that there could not be claimed for it an universality of application. This I hoped to accomplish by an attempt to demonstrate another principle of formation; which is, that the fundamental idea of a cell is *a simple vesicle*, and that a nucleated cell is simply one vesicle containing another within its walls. With this view, the nucleus and cell are identical in nature, and this gives to the whole doctrine a unity quite wanting with any other view. For, according to Schwann, the nucleus and cell possess dissimilar natures. With Schwann, the nucleus is *exogenous* and *germinative;* with me, the nucleus is *endogenous* and *reproductive.* The difference between our views cannot perhaps be better and more briefly stated, and by those familiar with this class of phenomena it will be understood. It is not for me here to recapitulate the grounds on which this view is founded, for I could not more briefly state them than I have done.

The mind so naturally seeks a unity in nature, and especially in the formation of an elementary fundamental part, that plurality of methods as to her workings may of itself be almost considered as an argument against the correctness of any of them. I have often

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been inclined to put that stress upon this point, as almost to lead me to believe that we do not yet understand the *nature* of cell-processes, and that, when we do, there will be perceived an unvarying unity, transcending in beauty and simplicity any and everything we have yet learned in organic life. Still, we must reason from what we *know*, and not from what we would wish; and although this idealism of a unity should always be a conservative element in our studies of natural phenomena, we should bear in mind that it is to be accepted in science only when borne out by unmistakable facts. On this account I still cling to the truth of what has been observed. In both plans of the formation, or rather at the end of it, there is one feature which is alike and common. I refer to the cell being *nucleated*. This is a condition of its most perfect and adult form, which necessarily exists in virtue of its being a complete whole. In fact, I may say that, as a certain form and size is the organic perfection striven for and to be attained by animals, just so is the nucleated condition the organic perfection to be attained by cells. It is true that the view of Schwann involves this idea as well as that of my own, but not, I have often thought, with the same fulness and completeness. For with Schwann, the nucleus being pre-existent and formative, its organic agency is past when the cell has been formed around it, and then the cell is nucleated, in memory (if I may thus express myself) of its past and formative condition. But, according to my own view, the formation of the nucleus is secondary, the sought-for perfection of its adult life, the offspring of it as an organic particle in its fulness of power; and as, in man, his perfection as an individual is expressed in that fulness and potentiality of his qualities constituting true manhood, so, in the cell, it is likewise a secondary production constituting its nucleated character.

But after all these discussions, the cell, as it appears to us—a material elementary particle—has an invariable physiological signification. However produced, it is the culminating point of organization; and if I may, upon so plain a subject, use a metaphor, it is the ground on which life and matter meet, mutually embrace, and then go hand in hand together, but showing in their united countenances that they bear in constant memory the conditions of their first embrace.

But this is only one part of our subject; we have only alluded to it in the light of development; we have simply traced an organic particle to its adult condition. The actions, the "doings" of it, after having reached its prime of life, constitute the remaining portion of

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its biography. Here, also, the same tendency towards a fundamental unity of action is manifested.

We have seen that, although the statement that all tissues take their origin from cells is not true to the letter, yet it is correct according to the true philosophy of the subject. Thus, we have learned that some tissues are formed from granules or utricles, and not from nucleated cells; but at the same time it was shown that the signification of these utricles is the same as that of cells. It may be truly said that the fundamental phenomena attending the appearance of organized structures is always the same, that is, *cellular*. The more immediate grounds of this opinion need not here be reviewed. The experience on which it is based is as authentic as any we possess of other phenomena of the external world.

The two conclusions of these studies of cell-life are, then, 1. The existence of an elementary particle, having an invariable unity of expression, *the cell*. 2. The universality of the application of this particle for the formation of organized parts, *tissue*. The philosophic mind cannot conceive of two results connected with the material world, which come home to it with more significant truth and beauty. For experience has here taught and made manifest to us, that which the highest philosophy has always prophesied—simple *unity* amid ever varied *diversity*.

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PART II.

PATHOLOGY.

CHAPTER I.

INTRODUCTORY.

THE domain which we have just left, Physiology, is by no means so widely separated from the one upon which we now enter, Pathology, as might be supposed from the commonly received doctrines of medical science. I do not make this remark, however, as applicable to anything but their material expressions. For the difference as to essential nature between truly normal and abnormal phenomena, must of course be wide. But in our conversation with the material world, either natural or unnatural, we can learn about it only through the intervention of material forms. It is in this way that we have learned all that we truly know in physiology, and it is in this way that we are now to inquire into pathology. And if from the minute character of our inquiries, the expectation is raised that thereby will be solved the mystery of the intimate nature of disease, that expectation will certainly remain unfulfilled and disappointed. In a general way, it may be said that pathology is but an erring physiology. This expresses a great deal that is true of its nature, and although perhaps not the whole truth, it approximates so closely, that it will serve as the basis of our inquiries.

Such a view is well calculated to remove from the mind many erroneous notions; one of which, for instance, is, that disease is a self-existing entity, which view, if well entertained, cannot but impede our correct interpretation of its phenomena; for we shall be constantly struggling between a fancy and a fact. Then, again, the ideas which we have of health and disease must be relative, since we have no positive data by which the one can be determined in contradistinc-

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tion to the other. Our conception of normal life must be extremely indefinite, and especially so, since the steps of its transition to that which is abnormal have not been well made out.

Another question which arises at the outset is, does disease always have a material expression, and that too of a corresponding and invariable character?

A negative answer to this would be deemed by many as quite *unphysical*, not to say unscientific; but in the present state of our knowledge I must regard it as by far the most correct; for we are to reason from what we know; and although analogy is of great service in such matters, yet we cannot be too careful of its use. We should very naturally say that, in virtue of the great fact constantly before us, viz. that vitality has its expression only in organization, which is tangible and capable of being analyzed, so should we always have a tangible expression of any perversion of that vitality. This may be seemingly very scientific, but at present it is so only negatively; for there are many transitory morbid changes of the vital phenomena, many morbid conditions known by the name of functional, which leave no traces in the matter or organs in which they occur, at least as far as we can now detect, by the most indefatigable research.

Too high an estimate must not, therefore, be placed upon these intimate microscopical studies of pathological conditions and phenomena. In the first place, with all the material product at our command which we could ask, we must expect to know of disease only conditionally; and in the second place, we must not be surprised to meet with many of the best expressed pathological phenomena holding apparently no corresponding material relations.

If these two points be well borne in mind, much error and confusion will be avoided.

But let us return to our original proposition: Pathology is only an erring physiology.

We can understand from this, why the genesis and general laws of pathological cells should be the same precisely as those of physiology. And I make here this general statement, founded upon a pretty widely extended observation, that, *both as to their genesis and general aspect as cells, those which belong to abnormal cannot be distinguished from those belonging to normal conditions of life.* The genetic and general relations of cells in physiology and pathology are, therefore, the same. But the bifurcating point in the road appears when we begin to inquire about the destiny of each. Physio-

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logical cells must always be considered *teleologically*, that is, as having relations with a future and determinate result, in the attaining of which they fulfil their destiny. With pathological cells, however, all these conditions are absent. They exist as cells in virtue of the previous existence of an abnormal formative material, which must have an organic expression of its forces. This expression is necessarily a cell; but there it ends; it sustains no higher and future relation for a definite result.

Abnormal cells, or rather cells produced under abnormal conditions of life, therefore, are not characterized by any type or true individuality. I know very well that some, such as those of pus, tubercle, and cancer, have a uniformity of appearance quite remarkable, but I cannot regard this as having any teleological signification at all, but rather as due to a corresponding uniformity of condition of the abnormal plasma in which they are formed. I do not think that this view at all disparages the scientific accuracy with which they should be described. For experience has shown us that this uniformity of abnormal plasma is so constant that it may always be counted upon in our determination of its products. With these considerations, founded in fact, the distinctions between physiology and pathology as based upon cells, are, therefore, not only broad, but definite, as far as they go. I may say, farther, that they are the only distinctions upon which we can at present insist; remove them, and I can perceive no reason why all our pathology should not at once be resolved into physiology. It may be asked if this resolution of the two thus into one, is not desirable, as simplifying our ideas of organic life both normal and abnormal? I say *no*, even were it possible; for it immediately takes away from the common phenomena of organic life their philosophy as manifested in their teleological bearing.

There is one other result which may well be deduced from the foregoing remarks. It is, that all pathological products are necessarily *infra* formations; they are below the standard of those of health, of which they may or may not take on some of the characteristics.

Pathological formations may be divided into two kinds only, viz: 1. Those which simulate the type of the healthy tissues, called *homœomorphous;* and 2. Those which have characteristics of their own, called *heteromorphous*. All abnormal products are necessarily one or the other of these kinds. It is true that the latter are most important in a practical point of view, on account of their peculiarity

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of life, widely separated as it is from that of the normal forms. Such, for instance, are cancer, tubercle, and pus. Under the former, on the other hand, are included all those forms of tissue which are abnormal, not so much because they are dissimilar from healthy forms, but because they want very much the definite character of these last as active tissues towards a conservative, economical end in the system. They are, therefore, less severe, and more amenable to treatment. But still, in an histological point of view, they are not the less worthy of our consideration.

It is difficult to grasp this subject unless we take it up in a particular manner. Pathological products as material forms are superventions in and upon the healthy parts. In a causative point of view, they are therefore referable to nutrition and its perversions. And I shall take up the subject in this light, even though there is necessarily a blending of physiological with pathological phenomena.

CHAPTER II.

NUTRITION AND ITS PERVERSIONS THE BASIS OF PHYSIOLOGY AND PATHOLOGY.

Two conditions mark the existence of the healthy living tissue. These are, *decay* and *repair;* the former occurring because the tissue is living—the latter, because in order to live it must preserve its physical identity.

This round of actions, constituting the sum total of those making up the life of the adult tissue, has its foundation in a function which we term *nutrition*—a word to which, in later days, we have been inclined to give a more pregnant signification than in former times; and this, because it thus embraces in function, either directly or indirectly, the whole phenomena of animal life. Many have thought that the relations of reproduction, or the origin and rise of the new being, should be viewed as belonging to another category of actions. But they have allowed themselves to be deceived by the importance of these processes; for we have seen, on a preceding page, that such phenomena are only *cellular*, and are therefore only those of nutrition seeking an individuality of expression.

Nutrition, then, being considered as the basis of physiological

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science, it will be my object to show that its *perversions* can be viewed as the foundation of our rational pathology.

But that my meaning may be fully comprehended, I shall run over briefly the leading features of this nutrition as a physiological function; a task which I have omitted in the preceding pages, anticipating, as I did, its consideration more properly in this connection. It is not a new, but it is a most important physiological truth, that the blood or its analogue is the source from which all the conditions of nutrition arise. It therefore follows that in it we should clearly recognize the elements of all these different tissues. The bloodvessels form a series of channels permeating the tissues, and terminating, as it were, in a set of vessels functionally different from either arteries or veins, the *capillaries*, which are the dispensers of the nutritive fluid to the tissues. And although these vessels cannot be traced minutely into every tissue or part, yet their function on such tissues is always indirectly perceived. It is asked, How are these capillaries the immediate agents of this nutritive function? It is by transuding through their parietes the hyaline plasma of the blood, into the parts through which they pass, which plasma is immediately appropriated by the contiguous tissue, or transferred by endosmosis through granular or cell-structures, to those more distant. This hyaline blastema is structureless, but it contains within itself the elements of structure. It is entirely amorphous, but it possesses in a latent form all the individualities of the different tissues. After effusion, it may serve its function as a pure plasma, by bathing the tissue, and filling the vacancies made by liquids passed away. But this, I think, is not common, and belongs almost exclusively to the sclerous tissues. It generally gives rise to more solid products. These are utricles and cells, with all their various metamorphoses. The primitive utricle appears as the first material expression, and in tissues of a purely utricular character, such as the muscular, its development does not extend beyond this point, but as such it is appropriated. But in tissues having a *persistent* cell-structure, these utricles pass on to cells, replacing those passing away. I believe that this hyaline plasma, immediately upon its effusion, and before the primitive utricles have appeared in it, is, whatever be its locality, identical in character. The reason why it has afterwards so many ultimate expressions of development, appears to me to be due to another cause. It is because, directly upon its transudation, the plasma receives, in coming in contact with a tissue, the impress or type of that tissue; so that, whatever the tissue may be, the plasma, in serving any purpose,

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follows directly in train of the *idea* on which the tissue is expressed.

Let me illustrate this doctrine by referring, for example, to the epithelial tissue. This, as we have already seen, is composed of a layer of cells, situated upon, and attached to, a basement-membrane. The cells thus attached, whatever be their function, are constantly passing away, and must be renewed. To effect this last, a plasma is effused by the contiguous vessels. This, as soon as it comes in contact with this tissue, takes on its *epithelial* type, and the primitive utricles developed in it immediately pass on to the ulterior condition of epithelial cells. Other examples might be cited to show, in the same way, this beautiful *type-power* of tissues, without which the continuity of structure could not be maintained. The full appreciation of this idea cannot be too strongly insisted upon; and I will again express it in a laconic way: A liquid containing the elements of structure, upon being brought in contact with a living solid, is immediately impressed with the type-character of the latter, and, therefore, must subserve its repair. It is not properly *a selective* power of the tissue, but a *living act*, occurring because the tissue has an individuality of its own, which it can impart. I regret that I cannot illustrate this by any reference to common examples of animal life; but as it is one of those immaterial acts in physiology, we can appreciate it only by the recognition of the fact. I might perhaps liken it well to the act of fecundation, in which a spermatic particle, by simple contact with the ovum, impresses upon it the full type of the male parent; and, to carry the comparison still farther, if the completeness of an individual can, in this way, be stamped upon an ovum, so, in the same way, in the act of nutrition, may the singleness of a tissue be stamped upon a hyaline plasma.

I regard the recognition and application of this *type-power of tissues* as one of the happiest results of modern physiology, not only as illustrative of the higher tone of our present studies in this direction, but also as enabling us to grasp many of the hitherto hidden forms of function in this science. On account of its value as the hidden spring of the various nutritions, let me still farther notice its character.

If it is asked, what is this *type-power*, I should say that its nature can be best expressed by an imperfect metaphor. It is the *memory* of the immaterial idea on which a tissue is developed, still persistent during its material life. And to carry the metaphor still farther,

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this memory may be bright and active, or may be fast fading away, according to the age of the tissue.

The younger the tissue, the more full and complete is its individuality and type-power; and if it suffers a lesion in its very early life, this breach of continuity is thereby so thoroughly repaired that its physical identity is preserved. This is the reason why in wounds with very young children, the healing of them leaves no cicatrix. The type-power extends fully and completely into the plasma effused for repair, and this repair therefore has all the character of true interstitial nutrition. As the individual advances in life, and passes into or beyond its adult period, this type-power appears to die out, or at least to lose some of its strength. This is the reason why at that time lesions are not perfectly repaired; the material not taking on the character of the contiguous tissue, and an adventitious product occurring in the place of the lost part. The same reason may be assigned for the fact that tissues having suffered no lesion sometimes atrophy—the plasma effused not being appropriated, but, taking on a new character, may give rise to new and morbid forms.

I might enter more fully into the consideration of this most interesting of subjects, and it would afford me much pleasure to take up its illustration in some of the most delicate tissues both in man and the lower animals. But in this place I have thought proper to sketch only its general character, which is to serve as the foundation of considerations of another character, soon to follow.

It is quite necessary that we should be very familiar with this great *high-road* of physiology, in order that we may well know where the *by-road* of pathology divides from it.

Thus far, we have seen that two conditions are necessary for correct and healthy nutrition; these are, 1st. That the plasma effused shall be healthy, and such as may be fit for appropriation; and 2d. That the type-power of the tissue to be nourished shall be sufficient to make the appropriation. Such being the requisites of the healthy nutritive process, the perversion or suspension of one or both of these conditions gives rise to what has been justly termed *abnormal* nutrition—a state of the animal tissues which we have good reason to believe is at the very foundation of pathology. To illustrate clearly this point, we will take up separately each division.

I. Perversions of the Character of the Plasma.

There appears to be a law of affinity or congruity of action in tissues, which must be regarded as a very powerful conservative of

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their integrity. By this, I mean that when the circulatory vessels do not contain the proper elements for the tissues, the latter do not call for the effusion of their plasmatic liquids; or, to impersonate the matter, as would perhaps John Hunter, I should say that the tissues, perceiving the incongruity of the nutritive fluids, refuse to have them effused. But still, it often happens that this inappropriate plasma is effused, and then, not being at all reconcilable to the type of the tissue, yet possessing a certain vitality of its own, which perhaps is still farther urged on by the very fact of its being in contact with a *living* tissue, the course which it pursues is wayward, and thus we have heteromorphous pathological products.

This perverted or abnormal plasma varies much as to its capacity, but is always *below* that of health. Its capacity is expressed in the character of its products. When quite low, granular and low cell compounds are the result. Such in fact are *pus* and *tubercle*, which appear to constitute the lowest expressions of a plasmatic formative power. When of a higher character, it gives rise to the highest pathological products, which often seem to have a kind of individuality of form and function. A good example of this is seen in *cancer*.

The heteromorphous products, then, of pus, tubercle, and cancer, I have considered as due to perversions of plasma. But perhaps the subject will come home more clearly to the mind if I say that *inflammatory products* may be regarded as due to this same perversion of plasma. I do not mean to say that such products can always be traced as the results of inflammation, but as far as yet studied there appears to be here a connection, at least, showing us that when we shall know more of the matter, our most comprehensive idea of inflammation will include all the conditions under which the heteromorphous products occur.

In here touching, then, upon the subject of inflammation, I shall not be considered as diverging from the main point of our discourse.

I do not pretend to define inflammation, because I think we have not yet sufficient data to convey to the mind a clear idea of its character. Still, if I say that there appears to be coexistent with it, a want of healthy relation between the bloodvessels and the blood, I think I have stated pretty clearly all that is really known about the matter; and even here it is very far from being certain if this *absence of relation* is not the first known *effect*, instead of the *cause* of the inflammatory process.

It is, therefore, a waste of time to dwell upon that, of the nature of

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which we have as yet so feeble an appreciation. We must take the results as we find them, waiting for the ultimate cause until we have more data.

I have said that the first visible sign of inflammation is an absence of the healthy relation between the bloodvessels and their contents. This leads to a partial suspension of the function of both, and also causes that function which does occur to be of an abnormal character.

Where the processes otherwise nutritive are very active, scarce any appropriation of this plasma takes place, and the products arising in it, viz. granules and corpuscles, seem so alien to healthy tissues, that they are expelled as foreign substances. Such is *pus* in all its forms, and such, I think there is reason to believe, is *tubercle* also. When, however, the process is less active in its character, the plasma is appropriated to a certain extent; but the tissues thus badly nourished, sink below their normal type, and when these conditions are kept up for any length of time, they seem to take on a character of their own. This is often seen as one of the consequences of a previously acute inflammation of an organ, but especially is it observed in indolent ulcers. It is thus we see that the long-continued use of a perverted plasma, by a tissue, serves to modify the *type-power* of the latter. And in speaking of the minute pathology of some organs on a future page, I shall have occasion to enter more fully into the peculiarities of these changes and their consequences.

It may be asked, how the etiology of cancer can be considered as belonging to the perversions of the plasma? The reply to this is that, although eminently a morbid product, it differs widely from those of which we have just spoken, in possessing in a high degree a life of its own. The plasma in which it takes its origin, has a capability not much below that of health, but still has a character as different as the results produced. Although passing on to the higher cell-structure, its inferior character is betrayed by the objectless nature of its termination; for these cells appear to be the *ultimate* result of a morbid action, rather than the material agents through which a higher function is to be performed.

The cancerous structure, however high it may appear in an anatomical point of view, is aimless and without function.

In speaking thus of pus, tubercle, and cancer, as the results of a perverted plasma—heteromorphous products—I think I can best express their mutual relations and dissimilarities if I use a figure, and say that, normal nutrition being considered the great high-road,

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those forms of abnormal nutrition producing pus and tubercle, would be considered as small roads diverging from it, at nearly right angles. Whereas, that form producing cancer would be considered as a much larger road, diverging at a smaller angle; in fact, often afterwards running parallel with the former. But all these diverging roads never get back upon the main one, and therefore have a termination unlike anything of true function.

In concluding this section, the relations which this perverted plasma holds to inflammation, may be thus briefly stated. We cannot conceive of this want of harmony between the action of the capillaries and the constituency of the blood, unless we suppose at the same time an exciting cause. Now, as it is true that the more we investigate minutely their conditions, the more do we find the inflammatory process coexistent, and as, in the instances of pus and granular forms, the relations of cause and effect can be directly traced, we have a right to infer that the same is true, even when these relations cannot be fully made out.

And so, on the whole, we seem to be justified in regarding inflammation, whatever is its nature, as a condition always preceding, and in all probability causing, this perversion of the effused plasma, and therefore the immediate cause of pathological heteromorphous products.

II. Perversions of the Type-power.

We now enter upon the consideration of quite another class of phenomena. As the vessels are healthy and the blood normal, these phenomena cannot be viewed as being in the same category as those of inflammation we have just considered. I have therefore thought proper to designate the results of such conditions as the *homœomorphous non-inflammatory* products; in fact, they are forms which, while they are really morbid, partake nevertheless of the type of the healthy tissue, as far as that condition will allow. We have just seen how, when the type-power was good and the plasma bad, the dissimilar results were referable almost entirely to the plasma. We shall now see that, where the inverse is true, the results produced have more an affinity with the tissue than with the plasma. In one sense it can scarcely be called a perversion of type-power, but rather a decrease or increase; but, on the other hand, these words do not express the whole, for, beside these variations of amount, there is involved a pathological principle not easily or readily expressed. Suppose that, from some unknown cause, the type-power of an epithelial tissue in

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the body had become changed. The normal plasma is thrown out as usual, but it is not delicately and nicely appropriated as in health, and although the tissue has given it its impress, yet this is all, for the relations of size and shape appear to be absent. There therefore appears a new form which, while it bears the outward aspect of the healthy tissue, is an abnormal product; it is, as it were, the representation of disease under the garb of health. This product is *epitheloid* but not *epithelial*. Such, for instance, is the so-called cancer of the lip, the cancer of the antrum, &c. &c. In the same category may be considered many, if not all, the hypertrophies of tissues, where a product appears with the general character of health, but with the profligacy of disease.

It is not difficult for us to understand how, in some of these immense growths, a sufficient amount of plasma is supplied, for it appears to be a law in the nutrition of tissues that the greater the demand the greater the supply; and so, when the demand even by a morbid product has once been made, it is furnished, and the whole may go on increasing, the vessels conforming to these changes exactly as though a healthy tissue was experiencing a rapid normal growth. But if we thus have products from what, in one sense, may be called an *increase* of type-power, constituting *hypertrophy*, there is quite a different class arising from a *decrease*, in fact a suspension of it, and which ought in this connection to be noticed. I cannot say that it is primitively of tissue origin, but, at any rate, it seems to be a dying out of the type-power; in fact, so thoroughly is this the case that the individuality is gone, and nothing is appropriated; and, in accordance with the law just mentioned, there seems ultimately to be scarce any supply, and then the tissue loses its physical identity, and gradually recedes to its primitive utricular condition. This condition of things has generally been considered in the light of an *atrophy*, but I have thought that it merited a distinction, and propose for it the name of *Retrograde Metamorphosis*. Its leading characteristics may be thus briefly stated: Bichat's definition of life, as applied to an individual, was: "The sum total of the functions by which death is resisted." Now, what applies to an individual, may be taken as, at least in part, applicable to a tissue. Its life consists in the conservation of those two conditions by which its integrity is maintained. These I have already regarded as involved in what is called nutrition—the balancing of decay and repair. Now, whatever function a part may discharge, it is necessary that it (the function) should be kept up in order that the nutrition should con-

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tinue normal; but when from either an unknown cause, or from a suspension of function, the part ceases to have that ever-changing vitality, then it seems to lose its type-power, and the small quantity of effused plasma is feebly appropriated, the vital cohesion of the tissue in a measure disappears; so that although there is strictly no decomposition, yet the individuality of the part is gone, and, with it, all those forces that elevated and sustained its character above that of the primitive elements. We have, then, in place of the normal tissue, what is called a granular mass, and, as such, it cannot be regarded as a special product, for it is the same, whether it occurs in a muscular or in a glandular organ. It consists simply of oil and albumen, uniting in their usual way. But this subject has been treated more fully in another place.* I have said that this condition differs from *true atrophy;* for this last can scarcely be regarded as an abnormal state, it being only a decrease of tissue-function, which we see daily exemplied in the muscular tissue. The same is true in an inverse sense of *true hypertrophy*, which is *not*, as I have before said, the cause of *homœomorphous products;*—they are, both, rather variations of nutrition as to *quantity* than as to *perversion*.

Such is a rapid survey of some of the perversions of nutrition which lie at the foundation of many of our best views of pathological changes. It may be asked if, under this head, may be included the causes of all pathological phenomena having their expression in a material product? With our present knowledge, I do not think that the question can be positively answered. Nevertheless, I think I am safe in saying that the *tendency* of the present inquiry is to show that in abnormal nutrition is to be found the cause of all *organic* pathological changes.

These considerations may, perhaps, serve as the groundwork of our subject; it being now our task to look into the specific character of its details. I shall, therefore, take up first the subject of heteromorphous products—each in its proper character—commencing with the lowest. But before this, the phenomena of inflammation, as elucidated by the microscope, should be considered, and this will serve as a fitting prologue for the ensuing pages.

* For its farther discussion, see an article of mine on "Tissue and its Retrograde Metamorphosis," in the Amer. Journ. of Med. Sc. n. s. vol. xxii. p. 22.

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CHAPTER III.

THE MICROSCOPICAL PHENOMENA OF INFLAMMATION.

I THINK that it is as true in physiology as in psychology, that the nearer we get, or, rather, think we get, upon the very elements of any phenomena in question, the more inclined are we at the same time to believe that, after all, we are mere outside spectators of the scene going on before us. This remark has an apt illustration in phenomena we are now about to describe.

It is evident that if we are to see the real phenomena attendant upon the inflammatory process, we must study the affected tissue in *situ*.

The tissues of nearly all the higher and warm-blooded animals do not admit of any direct experimental inquiry of this kind; so that most of that which relates to investigations in this direction, physiologists have been obliged to make out from experiments made upon such of the lower animals as have tissues of sufficient transparency.

Some of the Batrachian reptiles have been selected for this purpose, and although they are objectionable on the ground that we must, even there, suppose that the same phenomena occur, under similar circumstances, in the higher animals, yet they have formed the only basis we possess of all direct inquiries of this kind.* Kaltenbrunner,† in 1826, was the first to demonstrate these phenomena, and most of the physiological microscopists since have verified and carried out these inquiries. I scarcely need here describe the details of these experiments, for most have read of them, and not a few seen them. However, those made within a few years have led to some change of opinion as to the real character of the phenomena witnessed.

I will describe the processes as they are usually seen to occur in the web of a frog's foot. The interdigital membrane being spread out upon a plate of glass and viewed with a power of 300 diameters,

* Bat's wings can be well used, and, within a few years, the phenomena in question have been quite satisfactorily studied in this way, with the advantage that the bat is a warm-blooded, mammalian animal.

† Kaltenbrunner; Experimenta circa statum Sanguinis et vasorum in Inflammatione, Monachii, 1826. (A rare work, which I have not seen in this country.)

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you perceive the capillary bloodvessels existing as channels in the tissue, and the blood and lymph corpuscles rushing along in them with a kind of pulsatory movement. This is the healthy circulation. But, if the membrane be irritated, the appearances quickly change. In the first place, the vessels become narrower, and there is a consequent increase in the blood's current. This destroys the natural pulsatory movement, and gives a crowded aspect to the corpuscles in the vessels. But this condition of contraction is soon followed by the opposite (constituting the second stage), relaxation, in which the vessels appear larger; in fact, their diameter is increased one-third to one-fourth. Accompanying this relaxation, is a stagnation of the blood's current, and the dilated vessels, crammed with red corpuscles, present a very red aspect. With this cessation of the current, cease the primary phenomena. This constitutes congestion, and after congestion comes effusion. But we are led to inquire the cause of this stagnation. We have first, contraction, and then, relaxation of the vessels, and this stoppage occurs with the latter condition. Now, in my opinion, it is due mainly to a change in the natural rhythm of the vessels or their walls, and when this has once occurred, the relation of the blood to the vessels is quite altered—at least, mechanically—and a "clogging up" ensues. This alteration extending over a small surface, a much larger area is included from the direct connection, so that the clogging up of a single vessel may lead to the same with four or five others connected with it.

It will be seen that in this account I place no stress upon the behaviour of the white corpuscles. Mr. Addison* and Dr. Williams,† both of whom, and especially the former, appear to have studied the subject considerably, take a different view of the subject. According to them, the first changes consist in the sudden appearance of a great number of the white corpuscles, which begin to adhere to the sides of the vessel; and, by a repetition of these adhesions all along, the caliber of the vessel is obstructed, and hence stagnation. If we may credit their statements, there seems to be an actual increase in the generation of these white corpuscles—which appear to have the power of rapidly multiplying themselves—for they suddenly appear, and that, too, from an unknown quarter. So that the primary phenomenon of the inflammatory process is, according to these observers, a change of relation between the bloodvessels and the white corpuscles.

* Addison; Lond. Med. Gaz. Dec. 1840, and Jan. and March, 1841.

† Williams; Lond. Med. Gaz. July, 1841.

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But this view appears to me incorrect, and its fallacy will be apparent to any one who carefully considers its phenomena. In the first place, from what we have already learned of the nature and relations of the white corpuscles, it is, to say the least, quite *unphysiological* to suppose that there is a sudden increase in their generation at the time of the irritation. I have always regarded this as the outstanding objection to this view. Moreover, the experiments were made upon frogs, and the irritation produced by acetic acid; and I cannot but believe that they were deceived by the nuclei of young blood-corpuscles, which were caused to escape, and rendered distinct by the acid, and which quite closely resemble, as we well know, the colourless or white corpuscles. In fact, the action of the acid is so sudden that, in many cases, the replacement of red by apparent white corpuscles, is, as it were, by magic. Then again, there is another source of error. It is well known to all who have experimented much with frogs under the microscope, that the relative quantity of the constituents of their blood varies very much. Thus, in young and full-fed frogs the number of white corpuscles is very great, even exceeding that of the red; whereas, in old and more famished ones, the opposite is true. Now, it is apparent that if the experiment is made with the first, one would be quite likely to be deceived by their unusually great number. While, if frogs be taken in the spring of the year, when half famished, a quite different aspect is presented.

Mr. Addison thinks he finds support for his opinion in the fact that blood drawn from a pimple, the base of a boil, the skin of scarlatina, &c., in the human subject, presents an unusual number of the white corpuscles. This fact may have been correctly observed, but all my inquiries in this direction lead me to believe, that if, in such cases, there is a disproportion between the white and red corpuscles as to numbers, it is not local, but exists throughout the entire quantity of blood. In proof of this I may mention the fact, which I have often verified, that if the finger of almost any cachectic, tuberculous patient be pricked, the blood therefrom will be found to abound in white corpuscles. Moreover, such persons are those most generally affected with cutaneous eruptions, boils, &c.

In making these remarks, I wish to be understood as not urging any objection against the view that, in inflammation, there is an increase generally of the number of white corpuscles, or rather that there is a deficiency of the formation of the red ones out of them.

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For this view I believe correct in the main, but it appears to be rather referable to general and constitutional conditions.

I am therefore inclined to the opinion of Dr. Bennett and Wharton Jones, that an especial abundance of the white corpuscles in an inflamed part is neither a frequent nor constant occurrence. And this, I may add, is borne out by the experiments of Remak, who observed that those portions of blood *first drawn* in inflammation contained but few white corpuscles.

What, then, are the primary phenomena of inflammation as observable by the microscope? As we have just seen, they may be regarded as the following: 1. Contraction of the vessels, with a consequent acceleration of the current. 2. Dilatation of the vessels. 3. Stasis, or cessation of the current. This constitutes that condition of congestion which necessarily precedes the inflammatory process. But the confusion must be avoided of supposing that these phenomena *constitute* inflammation; for this is not really so; and we may have them all, as in cases of ordinary congestion, without any inflammation following. But at the same time, I should remark that we ought to be careful about the use of this word *congestion;* for it would appear that the congestion preceding inflammation is quite different from that which is a simple hyperæmia of the vessels. They may pass into each other, but the general experience is that there is no necessary connection. But in the initiatory phenomena of inflammation, what is the cause of the stasis? I have already expressed the view that it might be due to a disturbance of the natural rhythm of their action. But this, I am free to confess, does not explain the whole matter. We know very well that the outstanding causes of inflammation are of two kinds—traumatic or external, and idiopathic or internal; and, as far as we understand the subject, the sequent phenomena are the same in each and both. If we say that inflammation is due to an altered relation between the bloodvessels and their contents, I do not think we are any nearer the ultimate cause; for this is only expressive of a condition.

In inflammations of a traumatic origin, we regard the disturbance as commencing with the parietes of the vessels; but in those which are idiopathic or constitutional, we trace the first trouble to the blood, which subsequently acts upon the vessels. But there is, I believe, a third form, not originally due to either, and which has been well described by Mr. Paget.* It may, in one sense, be called of nervous

* Paget; Lectures on Inflammation, &c. in Ranking's Abstract, No. 12; or Lectures on Surgical Pathology, &c. 2 vols. London, 1853. Vol. i. p. 292, *et seq.*

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origin, and I can see no reason why the nerves should not so control the vitality of the vessels as to lead to these phenomena. Notable instances of this form are, inflammation of the conjunctiva after severe use of the retina; the excited state of the optic nerve being communicated to the filaments of the nerves of the conjunctiva, causing them to alter the nutrition of the part; also, inflammation of the testicle from severe irritation of the urethra, which is only to be explained by a disturbance of the nervous power controlling the nutrition of the testicle.

There are also many phenomena connected with neuralgia coming properly in the same category.

As yet, therefore, we cannot look for the cause of inflammation in any one circumstance alone; or, in other words, we as yet know nothing of the intimate nature of those causes producing the initiatory phenomena observable by the microscope. Undoubtedly the chief of these last is the stagnation, and, in my opinion, when we have discovered its relations, we shall then have learned quite definitely about the whole matter.

In conclusion, I will say that, if we are to remain on the ground of demonstrable facts, it cannot be said that the microscope has been of eminent service in elucidating the real nature of the inflammatory process. In fact, that it should be, would be expecting too much. With means of physical examination, we can only seize hold of physical appearances. The forces of matter and that which concerns vitality, cannot, at present, be studied by these aids. And it is my opinion that the intimate conditions of this pathological state are not only beyond microscopy, but beyond chemistry. The microscope, in revealing to us the outstanding phenomena, such as I have described, has accomplished all that could be justly expected; and although this is not much, it has served at least as a *physical* basis of our knowledge of the subject.

The secondary phenomena of inflammation commence immediately succeeding upon the stasis. They consist of two kinds; first, the effusion of the plasma as a hyaline liquid into the contiguous parts; and second, the formation of utricular and cell products in this plasma.

These formations constitute the heteromorphous products, which, in pursuance of the remarks I have hitherto made, I have seen fit to consider as at least the sequelæ of the inflammatory process. In the following chapter, I shall take them up in special detail.

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CHAPTER IV.

HETEROMORPHOUS PATHOLOGICAL PRODUCTS.

I. Utricles or Granules.

We have already traced, in a rapid way, the features of abnormal nutrition, as a condition; we will now take up the description of its material expressions.

Thefluid hyaline blastema, we have already seen, is the first product of the diseased action. Rarely, however, does it remain as such, but advances toward a higher state.

The chemical nature of this fluid, as proved by analysis, is *oleo-albuminous*—that is, composed of oil and albumen. But these elements as they first exist are transparent, and some combination is necessary that they may be made visible, appearing as separate particles.

This is accomplished in physiology exactly as in pathology, and I have already given its description. The utricles thus formed are also in appearance exactly like those of health, and like them, too, they are the basis of all the higher cell-forms. These forms may be the *pus*, the *tubercle*, or the *cancer* cell.

The most simple heteromorphous product is of course a plasma or blastema perfectly hyaline, which has not sufficient vital capacity to give rise to even these low products. Such are, for instance, many serous effusions, transudations of the larger bloodvessels, which analysis shows to be mostly composed of water. Notable instances of this are the effusion of a *healthy* blister, and the effusion into the plasma consequent upon the more *laudable* forms of inflammation of that organ. As such, they may be entirely absorbed exactly as they were thrown out.

A little more active form of this liquid is that in which these utricles appear, but which have not capacity enough to pass on to cells. Such are many effusions, from the so-called scrofulous inflammations, or those occurring in individuals whose natural standard of vitality is below that of true health. These are never wholly absorbed, but leave their marks behind them in the form of adhesions, lymph, &c. &c. These last have the lowest form of organization—the fibrillated structure, and the details of which are exactly like those of health already described.

Utricles and utricular formations constitute a large portion of pa-

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thological products, and a large portion of these products are directly referable to the inflammatory process. I have already said that a utricle is only a cell in embryo; but in these low plasmas the ulterior state of a cell is the fate of *comparatively* few utricles. In fact, this remark is true, literally, of all pathological plasmas; and this is the reason of the wide disproportion in numbers and amount of utricles and cells in pathological formations. *Most* utricles do not get beyond their primitive condition. This is a state of things quite fortunate; for, were every pathological utricle capable of cell-development, there is reason to believe that our physiological condition would oftentimes be obliged to recede before engrossing pathological growths.

In utricles, then, we have the commencement of solid abnormal products, and these utricles have always the same aspect; they may have a lower or a higher destiny, according to the vital impulse of their blastema. We will now look to their ulterior forms.

II. Cells and Cell-forms.

The cell is the highest condition attainable by a heteromorphous pathological product; it is the ultimate and not the transitional form. These cells, born with no preconceived end or aim, and arising in a blastema which has no physiological impress, have lives ending in abortion. They fulfil no function, except in some instances that of a feeble and imperfect reproduction; they soon pass away, or if they grow (not developed), they show their want of any affiliation with the economy, by their being recognized by it only as foreign bodies.

Most of them are as imperfect in their physical conformation as they are in their physiological destiny. On this account, they can, with the exception of some of the higher forms of cancer, be recognized as morbid by their anatomy alone. In our physiological studies, we have been led to place a high importance upon the *nucleus* of the cell, for it not only indicated the adult and potent condition of the cell, but had otherwise a vital value. In pathological cells, it may and often does exist; but if so, it is generally imperfect, and is so little expressive of the power of the cell, that it does not rise to the dignity of unity, but exists in several parts, constituting the so-called *nuclei* of cells.

1. *The Tubercle-Cell.*—This I regard as a very good example of the lowest cells or cell-forms existing in either healthy or diseased tissues. It constitutes the expression of a very slight elevation of an organ-

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ized form above that of a utricle. The plasma in which it is formed is filled with utricles, which have no distinguishing features from those of others. Some (but few) of these utricles increase and form a cell-membrane, containing a clear liquid, in which last, granules appear, but which remain as such, never forming a true nucleus. Thus it is, that, as far as my observation goes, tubercle-cells never have a nucleus proper, but only several granules. These cells thus formed vary in size from $\frac{1}{2500}$ to $\frac{1}{3500}$ of an inch in diameter. As I have sometimes observed them, quite soon after formation in the tissue of the lung, they were even larger than this—about $\frac{1}{2000}$ of an inch in diameter. But they are rarely seen under these circumstances; and as usually observed, taken from tuberculous matter of various organs, they are much smaller, and present a much less regular contour and appearance. They then have a somewhat shrivelled aspect, are rather angular, and the granules are more distinct. This is due, I think, to a partial exosmosis of their contained liquid. Upon being treated with acetic acid, the membrane becomes quite transparent, thus rendering the contents more distinct. The distinguishing characteristics of the tubercle-cell belong, therefore, to its low condition. Moreover, it is an adventitious product, and always exists as non-nucleated, having a finely granular aspect. These peculiarities are sufficiently well marked to make it always distinguishable by the microscope, and especially so, if combined with that *first-sight knowledge* which long-continued observation always gives. So that one may be able, the moment he sees the cell, to pronounce whether or not it is tubercular. Then, again, these cells have a uniformity of appearance quite remarkable, so that, once fully recognized, one would know them pretty certainly afterwards, wherever formed.

Fig. 37.

Tubercle-cells in various stages, from lung of patient dying of acute phthisis. *a.* Freshly deposited cells lying in the structure of the lung. *b.* Old tubercle-cells from tuberculous masses, the usual appearance of the cells as found in the yellow masses. *c.* Usual appearance of tuberculous matter under the microscope—cells in the midst of granules. (Magnified 350 diameters.)

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I do not, of course, regard this uniformity of appearance as any indication of their possessing a distinct type; because, as I have before remarked, morphological types do not belong to pathological structures. Moreover, the tubercle-cell, wherever formed, or in whatever organ, or in whatever animal—horse, dog, &c., is the same. This uniformity, therefore, must be considered as quite referable to the plasma in which they are formed.

But this tubercle-cell, although always present in tuberculous products, constitutes only a small part of their substance; for, from the very fact that it is one of the lowest products, utricles compose by far the greater part of tubercle as a product. And fortunate it is that these granules are nearly all of them thus feebly endowed; for, were they all capable of cell attainment, man would stand no chance against the ravages of a disease which even now, as it is, decimates his numbers.

These utricles are, of course, composed of oil and albumen, but in many instances, and especially when the blood is rich in oily matters, the oil predominates over the albumen, and you then have oily particles scattered through the mass. It is not uncommon, therefore, to see in the field of the microscope great numbers of oil-globules in a free state, beside the utricles and cells. Fat-globules, utricles, and cells are, then, the visible microscopic parts of the tuberculous product; and, from a quite extended series of examinations I have made upon tubercle, not only as occurring in nearly every organ of the human body, but also as appearing in some of the lower animals, I am satisfied that these are the only parts properly belonging to it. It never attains a farther organization; on the other hand, fortunately, all its tendencies are of a retrograding character. Pathologists, guided by external appearances, have divided tuberculous products into two kinds, the *gray* and the *yellow* forms; the former occurring in a disseminated manner, the latter in large masses. But this distinction has no foundation in a difference of the cell, but is due to there being a greater amount of fatty and utricular matter in the yellow than in the gray variety; which last is composed mostly of cells.

In thus giving the tubercle-cell and its accompanying plasmatic forms a distinctness of existence—a kind of individuality, it may be asked if I regard it as a true inflammatory product. To this I reply in the affirmative, meaning thereby that the effusion of the tuberculous plasma is the result of an abnormal condition of the blood generally, which condition, in the present state of science, must be considered

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as belonging to inflammation, as we have been led to understand the meaning and intimate signification of this last.

This abnormal condition of the blood has been called the *tuberculous dyscrasia;* which, judging from its products, has an undeviating uniformity. This leads me to express the opinion which I have long entertained, that the appearance in the system of the tubercle-cell is *always* due to a general rather than to a local pathological condition. I know it is true that its expressions are sometimes simply local, but this is rare, to the misfortune of our race.

The peculiarities of its deposition in the different organs and tissues of the body do not properly belong to this work, and however important and worthy of consideration in a practical and therapeutical point of view, they do not relate to their histology, for this last is always the same.

When the tuberculous product has reached the cell-form, it has attained its highest point of organization. Then, like all morbid products which are produced under physical and not with teleological relations, it dies, and undergoes all those peculiar changes of which an organized product is capable while existing in a living tissue. It retrogrades to its primitive elementary condition, then a chemical dissolution takes place, the aqueous portions are absorbed, a thick wall of cicatrical tissue is formed around the residue, and thus the radical cure of tubercle is sometimes effected. Such, my own experience teaches me, not unfrequently occurs in the pulmonary tissue, and in the lymphatic glands. The course of tubercle, then, is such as we should well expect it would pursue.

But in thus regarding the tubercle-cell as strictly an inflammatory product, it is asked, how are to be reconciled many of those phenomena of its appearance in organs, and especially in the pulmonary tissue, indicating at first a dissimilarity to the usual inflammatory conditions?

I can well appreciate the force of this objection, for I once regarded it as sufficiently valid to make the distinction broad between these two processes.

Louis and other pathologists have shown that it is true that incipient pulmonary tuberculosis generally commences at the apices of the lungs, while pneumonia or ordinary inflammation nearly always commences at the base.

Then, again, both the clinical course and the gross pathological anatomy of these two affections are quite distinct, and seem never to completely fuse into each other. But these distinctions are not more

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wide than the intimate causes of these two affections, as far as we understand them. As I have before remarked, the causes of tubercle are as deep and general as the very constitution of the individual; they are constitutional, and this is the reason why they may be hereditary. On the other hand, the causes of simple inflammation of the lungs are of a more local and particular nature, and due, generally, to direct influences of external agents. These should be sufficient reasons, I think, why their outward manifestations are somewhat dissimilar.

From the very fact that the inflammation of tuberculosis is constitutional in its origin, we might well infer its very low character. This, the soundest practical experience has shown to be true; for although even its very earliest expressions are attended with a general febrile condition, yet this last is always most successfully managed by a *sthenic* rather than an *asthenic* mode of treatment; and this, I may add, is one of the happiest and most important results of modern pathology; and although its first recognition was due to clinical studies, yet we have learned fully to appreciate it from the results of microscopical analysis.

2. *The Pus-Cell and Pyoid Forms.*—We now come upon those products which are, without dispute, directly referable to the inflammatory process. As far as the most careful experience has yet shown, they are never found without there has been a pre-existing inflammation; neither does there ever occur inflammation without their formation to a greater or less extent. Under the above head, I include not only the true pus-corpuscle, but all those cell-like, granular forms with which it is usually associated to a greater or less extent. An inflammation exists, an abnormal plasma of the blood is thrown out, and in this, the first traces of an organizing force are manifested, as usual, by the appearance of utricles. But these have no distinguishing characteristics, looking exactly like those of tubercle or cancer. Soon, however, cells are formed from them in the usual way, and, in what is called "laudable" or creamy pus, they compose the greater part of the solid portion of the pus-liquid. Comparatively few granules or utricles remain as such, and thus is manifested its formative power over and above that of tubercle just described. The cells thus formed consist of a membrane filled with liquid, in which are three or four granular bodies, which, aggregated together, constitute its nucleus. It is, therefore, a granular nucleated cell, having a diameter varying between $\frac{1}{1500}$ to $\frac{1}{2000}$ of an inch, but the average of which, according to my own experience, is about $\frac{1}{1800}$ of

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an inch. I am not aware that its size or aspect varies to any extent, from whatever part it may be taken. The same is true with reference to the other mammalia I have examined—such are, notably, the horse and dog. The true pus-cell, therefore, presents a very great uniformity of appearance, perhaps full as much as that of tubercle. When treated by acetic acid, the cell-wall becomes quite transparent, the nuclei are most distinctly seen, and, if the action of the acid be sufficiently direct, the wall is quite dissolved, and their escape takes place; pure water enlarges them by endosmosis, so that they appear full and plump. There can be no doubt, therefore, as to the existence of the cell-membrane.

Fig. 38.

Pus from an abscess. *a*. Pus-corpuscles, granular and nuclei, indistinctly seen. *b*. The same treated with acetic acid, revealing a clear membrane, inclosing three, four, or more nuclear granules.

However, in these days, a detailed description of the appearance of the pus-corpuscle under the microscope, seems quite unnecessary, for every person can see for himself, and that, too, which no words can as well describe. Although the pus-corpuscle, thus formed, as the product of what is called a true "healthy" inflammation, can always be definitely described, yet all inflammations are not equally active, and their products are very far from being as well formed. The pus-cell being the highest of its cell-products, the others with which we meet are of a lower character—they are cell-forms, but retain a good deal of their utricular character. Such are the *pyoid* corpuscles described by Lebert,* and such also are the *inflammatory corpuscles* of Gluge,† and many other illy-formed and badly-defined bodies which have no proper name, but which are met with in indolent suppurations, and especially in the expectoration.‡

* Lebert; Physiologie Pathologique, Paris, 1845, tom. i, p. 46.

† Gluge; Anatomisch. Mikrosc. Untersuch. zur Pathol. Minden, 1838, p. 12.

‡ Gruby has described very many forms of pus-corpuscles occurring in different parts of the body. He thinks the peculiarities are constant, and therefore puts much stress upon them, a view not at all supported by more recent labours. See Gruby's work in Latin—Observationes microscopicæ ad Morphologiam Pathologicum, &c. Vindobonæ (apud Singer et Gœring); a partial translation of this may be found in the Microscopical Journal, 1840–1842.

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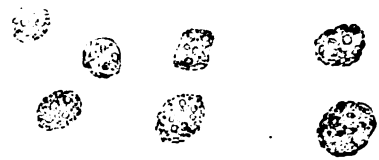
The *pyoid corpuscles*, although often having well-marked characters, scarcely deserve a separate name; for the products of inflammation

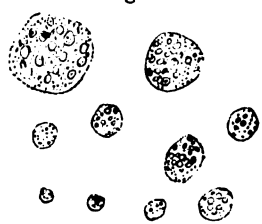
Fig. 39.



Pyoid corpuscles from meningitis, showing that they differ from those of pus in being larger and more delicate. (Magnified 350 diameters.)

being all abnormal, we cannot well expect any adherence to a definite type, but a variation or oscillation to a considerable extent. They occur as the products of inflammations of cachetic individuals, mixed with a few true pus-cells. As I have seen them, they are one-third larger than the latter, and contain, within their interior, many small granules instead of nuclei, and, on the whole, seem more delicate; and, if I may thus express myself, are more rudely put together. They are, therefore, easily distinguished from the "*normal*" pus-cell. Both from their aspect, and the individuals in whom they occur, I should say that they constitute the transitionary form between the tubercle and pus-corpuscle, if the present state of our knowledge of these products would allow us to admit that there ever occurs any transition from one to the other. And then, again, it may be asked if such phenomena would not lead us to believe in this very transition? For my own part, I cannot say that they would or would not; but, at any rate, they have constituted one ingredient of my opinion that tubercle was really an inflammatory product.

Fig. 40.



Inflammatory corpuscles, being only enlarged sacs filled with granular utricular matter. (Magnified 350 diameters.)

The *inflammatory* or *granulation corpuscles* of Gluge were pointed out by him long ago. He regarded them as products of inflammation, but was decidedly in error in attempting to explain their appear-

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ance or presence as the nuclei of blood-cells. Their distinguishing features from those of the pus-corpuscle are much more marked than from those of the pyoid corpuscle we have just considered. And yet, these features can scarcely be described; for the corpuscle, as a whole, shows a want of regularity in almost every particular—seeming to be the rudest expressed form of organic power—under the semblance of a cell. They are simple sacs, filled with a fluid in which float granules, in numbers from two or three to so numerous as to make the whole a semi-opaque object. As might thus be expected, the variability of their size is very wide—from $\frac{1}{5000}$ of an inch to that discernible by the naked eye. However, the size most frequently met with, is that of about $\frac{1}{1500}$ of an inch in diameter. When in contact with chemical agents, they behave like delicate forms; that is, the envelop begins to be dissolved, and therefore is rendered more clear, and finally ruptures, discharging its contents. In this respect, it does not differ from ordinary corpuscles of any kind, the envelops of which have been rapidly formed or are delicate. Their mode of formation might almost be deduced from their physical appearance, for they are only sacs containing granules and fluid. As far as my observation goes, they appear to be formed by the expansion of utricles into larger sacs, and as the cell-process here ends, the whole result is abortive—not only no nucleus being formed, but the cell-like body showing no individuality as to size. Thus, in some forms of cancer, in which a low, indolent inflammation supervenes, they attain a size so as to be easily visible with the naked eye. The same is true of them as found on the inner surfaces of ovarian cysts. But these monstrosities are comparatively rare, and they may be said to have an *average* size, a term which is not properly applied to cells or cell-like bodies having an individuality of their own.

I think it is well determined that they are the results of the inflammatory process alone, and in an effused blastema of a very low order; and this inflammation is of a simple nature, having no peculiarities, as might be said to be true of that of tubercle. On these accounts, much value is to be attached to their presence; since, wherever found, the question of inflammation on other premises cannot be raised. They are met with in dropsical fluids, of any source; as those of hydrocele, cystic disease of the ovary, ascites, &c.; also in organs having a low chronic inflammatory process, or at least where the inflammation is of a resolvable instead of a suppurative character —such as in the sputa of pneumonia, inflammations of the skin, and in that low form of nephritis known as Bright's disease.

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And I will add, that my own observation has shown that their presence in the urine is nearly always certainly diagnostic of this disease.* They are also found in all pus to a greater or less extent, as this last may be the less or the more "laudable."

3. *The Cancer-Cell.*—The appearance of carcinoma in the system takes place under circumstances more obscure than those of any other morbid product with which we are acquainted. This is due to its high morphological character; for, although a heteromorphous formation, it stands far above those we have just been considering.

It will be seen that I have included it under the head of inflammatory products. I admit that, in so doing, I am stepping a little beyond the boundaries of demonstrated facts. But the position appears to me tenable. As a heteromorphous product, all my experience has led me to regard it as the result of an *abnormal nutrition*, and on a preceding page I have expressed my views as to the relations of this abnormal nutrition to the inflammatory process. Then again, its appearance often holds the relation, with inflammation, of cause and effect. Such is true as occurring in or after wounds of a part, or in the female breast, immediately sequent upon its inflammation. But in these instances we must not put too much stress upon the local lesion; for, as cancer, like tubercle, is always dependent upon a constitutional dyscrasia, its local appearance has other than simply local relations. This statement is broad, and expressive of many of the characteristics of the product; but, certainly, the tenor of its whole history, as we now understand it, is to this effect: its earliest appearance, in a distinct and unmistakable form, is as a few cells, as I have sometimes observed. We cannot trace it any farther back; but all analogy is in our favour for supposing that, like tubercle, these cells are formed in an unhealthy plasma effused from the blood. Moreover, upon the inner surface of the testicular tubes, where I have observed these cells just appearing, they seem to replace the normal epithelial cells there situated, and to be formed like them. As the first appearance of the disease is imperceptible in the majority of cases, there cannot be traced any coexistent hyperæmia of the part, or any congestion of the bloodvessels leading to an effusion of the morbid plasma; but in all probability, a condition of this kind does exist; and the fact that a mechanical injury, such as a blow or a compression, will sometimes give rise to the local appearance of this product, supports

* See, for some farther details on this point, an article of mine, The Microscope, and Renal Affections, Amer. Journ. Med. Sc. n. s. vol. xxii. p. 378.

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this hypothesis; for, in those cases, there is an effusion of the blood-plasma from the local congestion; and we can justly infer that the reason of its appearance, in that particular locality, is because the mechanical injury was *there* received.

As I have just remarked, the appearance of an adventitious cell or cells, constitutes the first indubitable evidence we have of the cancerous product. This cell, the first, is also the last expression of this product, it being the *terminal* and not the *transitional* form. It is nucleated, and oftentimes has several distinct nuclei; but as to size, form, and other external characteristics, there are none, it is my present opinion, which are constant and peculiar, and by which it may *always* be distinguished from other cell-forms. Formerly, my view was, like that of most other microscopical pathologists then and now, that the cancer-cell *as such* has physical peculiarities, by which it can always be distinguished from other cells. But, from a careful review, of late, of the subject, based not only upon observations of the cells as physical objects, but upon a consideration of them as constituting an expression of pathological action, I am free to confess that I do not consider my former opinion tenable.

All or nearly all the microscopical relations of the cancer-cell which have any claim of being peculiar to it alone, may, I think, be stated in the following brief manner: The cancer-cell, when fully formed, is simply a nucleated cell; it may, however, have several nuclei, or none at all; it often is of an irregular shape, fusiform, caudate, &c., &c.; and its size is frequently equally variable; but the evidence of its being not only pathological, but of what is called a cancerous nature, cannot and does not depend upon any one of these points alone, but rather upon a combination of them all; and that, too, united with the fact of these cells occurring in tissues, and under circumstances where they cannot be regarded as a healthy product. From this general statement, it is plain that the cancer-cell has no claim of having a type either of structure or general appearance. As a cell, or as cells, it forms the highest heteromorphous product with which we are acquainted, for it is structurally and morphologically quite above both tubercle, and pus, and pyoid forms. Its differential diagnosis, which I think can now be made out in nearly every case, depends upon these considerations, which, when fully examined, will be found to be amply sufficient.

Suppose, for an illustration, that a small portion of a cancerous tissue has been sent to a microscopist for microscopic examination. He decides, in the first place, that the tissue in question is not a normal one,

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from the fact that its cell-constituents possess no *type* peculiarities. As a heteromorphous product, then, the diagnosis lies between its being

Fig. 41.

Examples of cancer-cells, showing some variety of shape and general irregular character. *a.* Very regular nucleolated cells, from cancer of the arm. *b.* Irregular cancer-cells, and such as are more commonly met with. *c.* Cells very large, and in which daughter-cells have been formed. (Last two from cancer of the breast.) I give here a few figures only, because a correct idea of the cancer-cell, as an aberrant form, can be learned only from direct study by means of the microscope.

tubercle, a pyoid form, or cancer; and, from what has just been said, one can easily see that the decision that it was the last, or cancer, would not be difficult with a person who has long practised upon these products. But I may here remark that its differential diagnosis from certain homologous morbid products, although in my opinion quite clear, cannot always be as distinctly and positively made out. This point, however, belongs to a future page. All homœomorphous cell-products possess, to a considerable extent, as I shall soon attempt to show, the type-characteristics of the tissue to which they are homologous. All heteromorphous cell-products are characterized, as I have before said, by an absence of these type-forms. From these data, I think the experienced eye can always, or nearly always, decide correctly upon cancer-cells, when they exist in numbers sufficient to form a distinct product. On the whole, the high degree of organization is that which characterizes the carcinomatous product from that of all others of a heteromorphous nature; and, according to the scheme laid down on a preceding page, it may be justly inferred that the line between cancerous and non-cancerous products is always clearly defined, and also that they never pass insensibly into those of a healthy character.

The question may be fairly asked, In what consists the so-called "malignity of cancer-cells?" Space does not allow me here to criticize or even examine the different definitions given by various writers to the term *malignant*;* but if we are to understand by it the power

* It is quite remarkable to observe the obscurity of this point, not to mention the incompatibility of the notions entertained. Some writers define malignity to

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of a morbid product to replace a healthy tissue, to reappear, after removal, in the same or in a distant locality, and finally to compromise the life of the individual—if this, I say, is the definition of *malignity*, I think a tolerably satisfactory answer can be given to the question. It lies, in the first place, with a heteromorphous product, and its appearance here or there is but a local expression of a constitutional dyscrasia. So far, the peculiarities of cancer are parallel with those of tubercle. But it differs from tubercle in possessing a much higher organization. The constituents of tubercle are only cell-like forms; they have not risen to the dignity of nucleated cells. Their life, therefore, is finished with their formation, and the product increases by addition rather than by growth. Not so, however, is it with cancer. Its constituents are nucleated cells, whose existence is not completed by their simple formation; for they grow and increase, and new cells are formed within them, from their nuclei. They may be said to constitute potential germs, capable of an indefinite growth and reproduction, requiring only a nutritive material in which they may be formed.

The so-called malignity of the cancerous product may, then, be said to be due to its constitutional origin, united with a power of individual reproduction by the endogenous and exogenous formation of cells. In fact, it may be said to be due to this last alone; for, I may add, did tubercle-cells possess the high character and individual power of those of cancer, they would be equally as malignant.* Its capability of self-propagation and perpetuation, after being once formed, by the successive multiplication of cells, gives it a power of almost limitless expansion. Its liability to a plural appearance is, of course, due to its constitutional origin, for this is equally true of tubercle. But its liability to a reappearance in the same locality after removal, is dependent, probably, upon both the constitutional infection and the circumstance of a small portion being almost necessarily left behind, which serves as a basis for the building up of

be the sum of the properties possessed by cancer. Others define the word as that of incurable. Others base a definition of the term upon anatomical appearance, as, for instance, Dr. Hodgkin, who regards growths with a cystiform structure as malignant. But with a majority of writers, the term appears to have a very vague signification. The notion of a malignant disease exists in their minds as *an entity*, possessing irresistible destructive agencies and tendencies, manifested by a reappearance in a worse form as often as apparently entirely removed.

* The independent vitality of the cancerous tissue has been well shown by some curious experiments by Dr. Leidy, who introduced portions of it beneath the integuments of frogs. See Proceed. of the Acad. Nat. Sc. Philadelphia, vol. v. p. 212.

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a new product. For, both theory and experience teach us, that a cancerous product being *entirely* removed from a locality, that locality or tissue is not any more liable to its reappearance than it was previous to the first invasion. We may, therefore, conclude that the malignant character of the cancerous epigenesis, in contradistinction to that of other morbid products, is, after all, due to its high power of individual cell-*reproduction*. This is reducing the whole to morphological peculiarities, which are based upon our best appreciation of the nature of morbid cell-products.

Hitherto, we have considered only the true histological product of cancer—*the cell*. But the cancerous tissue, from the very fact of its being a pathological one, includes other elements, which may well be mentioned here. In a very few instances, I have met with the cancerous tissue as consisting *entirely* of cells; but this is quite rare, and occurs only when this epigenesis exists in its highest state. Elements of an inferior character generally enter largely into its composition, and these are such as are found in most other morbid growths.

One of the most constant of these is an amorphous granular substance, produced by the condensation of the granular matter. This is often so tough as to resemble a fibrous tissue; and, in cancers of a slow growth, forms a large ingredient of their substance. It is the framework on which the more delicate parts rest, and, in the higher forms of cancer, the grades of its transition into a fibrillated tissue are imperceptible.

The second element, almost equally as constant, is granules of various sizes, scattered everywhere. These are often expanded into cell-sacs, which are crowded with fat-globules. These fat-globules exist in a free state also, and often in such abundance as to give to the whole mass a decidedly fatty character. Crystallized fat, or cholesterin, is often found.

And thus it is that fat and albumen, in these imperfect conditions, form a large portion of the non-solid constituents of cancer. In many instances of this disease as occurring in the breast, it was evident to me that its rapid increase was due to a simple effusion of these elements, rather than to the increase of cells. In fact, my own experience is that the secondary forms of cancer, or the primary ones, after the cachexia is fully established, are for the most part made up of these low amorphous constituents.

In those of larger size and more active character, *bloodvessels* are

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found. These have, as Berard* has shown, the most simple structure, being tubes whose walls are composed of a single membrane. But the other peculiarities of the bloodvessels insisted upon by Berard, I have never been able clearly to make out.

It does not belong to me in this place, to dwell at all upon those peculiarities of physical appearance which many authors have thought sufficiently well marked and constant to serve as the basis of a very minute classification;† for these are points of much less scientific importance than the histological relations we have just considered. In fact, it may be fairly questioned if such divisions are in any sense scientific; and in support of this, I need only refer one to the incongruity of opinion existing upon this subject.

There is, however, this much certain, that the growth and condition of a cancerous epigenesis is *dictated* very much by the activity of the nutrition of the part in which it appears; and its first appearance is undoubtedly the same wherever it may be, that is, consisting of a blastema in which appear cells. Thus, when occurring in highly vascular organs, such as the brain and the eye, its growth is rapid, and consists almost entirely of cells. These forms have been called *encephaloid.* But when occurring in tissues less vascular, and in which the nutrition is less active, its growth is much more slow, and the cancerous blastema secondarily effused, less organizable. So that cells are not so numerous, but the growth is, for the most part, made up of a coagulated, fibrinous, granular fluid, constituting a kind of fibrillated or fibrous tissue.

These forms have been called *scirrhous*, and when a pigment forms a constituent of either the *encephaloid* or *scirrhous* forms it is then called *melanoid.* Such are the distinctions which have been made, and which are convenient for use. But even these, few as they are, pass into each other on every side. Some call certain forms scirrhous which others think scirrho-encephaloid, &c. &c.

It is enough for me that I should look at the subject of cancer as an histological product—as the expression of a constitutional dyscrasia, under the form of the highest pathological cell with which we are acquainted.

A discussion of the exciting causes of cancer, its development, distribution in this or that organ, its decay and consequence, belongs to

* P. H. Berard; Dict. de Médecine, in vol. xxx. art. Cancer.

† For a good illustration of this subdivision of forms, see Walshe's work, The Nature and Treatment of Cancer, London, 1846, p. 10.

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its clinical history. It would of itself be sufficient for a volume, as it already has been in several instances (Walshe and Lebert); and, following the plan pursued in the consideration of the other heterogeneous products, I purposely omit here anything of this kind.*

CHAPTER V.

HOMŒOMORPHOUS PATHOLOGICAL PRODUCTS.

In the pathological portion of this work, our attention has hitherto been occupied with a consideration of those morbid products which have no histological affinity with healthy tissues. They were, therefore, properly termed *heteromorphous;* and in the scheme of the origin of all morbid products which I advanced on a preceding page, I regarded them as the results of perversion of the plasma, which not being at all acceptable to the tissue into which it is effused, its low organizing power gives rise to products of an inferior character, which are at the same time quite dissimilar from any found in the healthy economy. But in discussing pathological conditions, one always perceives, on account of the anomalous nature of the subject, that the landmarks and exact definitions of natural science cannot be used. Thus, although I think that heteromorphous products are due to a perversion of the plasma or that condition known by the general term *inflammation*, yet, in some instances, this does not seem to express enough. For in the case of tubercle and cancer, another condition is coexistent (I do not say causative). This condition is what is called a constitutional dyscrasia, or an assemblage of depraved conditions leading to local expression of disease.

* Owing to the attention which the subject of cancer has excited with all pathologists from the earliest times, its bibliography is very full. I cannot pretend to give it here; but it will be found carefully and excellently drawn up in Dr. Walshe's work on Cancer (The Nature and Treatment of Cancer, p. 246, London, 1846). Since the work of Dr. Walshe, however, several excellent monographs have appeared, among which the following may well be mentioned here: Lebert; Physiologie Pathologique, Paris, 1845, tom. ii. p. 241. Vogel; Zeitsch. für Natur. Med. Bd. iii. 1 heft. Bruch; Die Diagnose der boesartigen Geschwulse, Mainz, 1847. Virchow; Archiv fur Pathol. Anat. und Physiol. Bd. iii. Bennett; On Cancer and Cancroid Growths, Edinburgh, 1849. Lebert; Traité pratique des Maladies Cancereuses et des affections curables confondues avec le cancer, Paris, 1851.

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We can get only approximatively at the intimate relations of disease; this may as well be stated first as last. Both microscopy and organic chemistry have quite failed to teach us anything respecting the ultimate data of pathological phenomena.

I have thought these remarks somewhat called for, before I proceed any farther, for, in regarding all pathological products as due either to a perversion of the plasma, or the type-power, I would not wish to be understood as thereby expressing the view that, by such scheme, the nature of all pathological phenomena can be comprehended.

The remaining morbid products which we have to consider belong to another class. In every instance they simulate the physical appearance of the healthy tissues. They are therefore properly called *homœomorphous*.

As I have stated on a preceding page, I do not regard these products as simple hypertrophies of the healthy tissues. For hypertrophy is simply the increase of the normal form, without the addition of a morbid element.

Then, again, homœomorphous products, although they simulate, yet they do not exactly resemble, the healthy structure. They always want their beautiful regularity and uniformity of physical contour, not to mention the entire want of function. I admit that, judging from mere physical aspect, it is often quite difficult, if not impossible, to draw the line between a true morbid epigenesis and a simple hypertrophy of the normal tissue. But, in my opinion, the line of demarcation is always distinct histologically, and in no instance is there a transition of the one into the other. These remarks, although applicable to all homœomorphous morbid tissues, are especially true, as we shall soon see, of the so-called hypertrophies of glands.

I regard all homœomorphous morbid products as simple *epigeneses*, or as always analogous new formations *upon* the healthy tissues. And when I use the word *always*, I make a statement of more physiological importance than would at first be supposed, for I thereby deny that new analogous tissues are ever produced, except in conjunction with the normal ones, through the influence of which alone they are developed. In fact, it appears to me that to admit the opposite view, that tissues, like the normal ones, may be formed anywhere adventitiously, is tantamount to the admission of the doctrine of *equivocal generation;* because, morphologically speaking, an animal is only a collection of individual tissues, and each of these tissues

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may be said to have in one sense an individuality as distinct as that of the whole animal itself.

The pathological relations of this doctrine are of very great importance, and I am inclined to look with doubt upon those descriptions of the appearance of new tissues in localities quite distant from those in which the same occur normally.

Aside from considerations of this kind, doubts might be properly raised from the fact that, in most instances, the evidence that the tissue in question is like the normal one, is based upon gross appearances alone. Every-day experience is showing us that, in important cases, we should not, and generally cannot, judge of the nature of a tissue by its mere physical aspects to the naked eye; and what is more, the liabilities for deception are such that no familiarity can remove them. I shall have occasion on a future page to refer to instances of this kind, and also to show that the microscopical structure is, and should be, the only reliable evidence. I may add that, upon a point of this importance, I do not think the *ipse dixit* of every observer should be taken; for aside from the comparatively few who well understand microscopy, the number is much fewer of those who know well the characteristic microscopic appearances of tissues.

I shall therefore take up homœomorphous morbid products in the light only of *epigeneses*.

I. Epithelial Epigeneses.

From the fact of the wide, indeed almost universal distribution of the epithelial tissue, its pathological epigeneses are more frequent and more important than any other belonging to this class of products.

There is one other circumstance which may be considered as favouring their frequent occurrence. This tissue is composed of simple cells, often existing to be sure under various combined forms; but its nutrition is necessarily delicate, and quite liable to be perverted, or rendered abnormal, thus giving rise to these products.

As a normal tissue, it necessarily possesses an individual type of structure and appearance; but this has a variety according to the locality. The pathological formations follow somewhat in the routine of this variety, so that, in the same way, there may be said to be different kinds of epithelial epigeneses. I am not, however, disposed to insist upon these refinements.

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Both for the sake of convenience, and as a somewhat natural division, epithelial epigeneses may be separated into three groups :—

1. *Those occurring upon mucous and serous membranes.*
2. *Those occurring in or upon the skin.*
3. *Those occurring in the ovarian tissue.*

1. THOSE OCCURRING UPON MUCOUS AND SEROUS MEMBRANES.

This group contains by far the largest number of these products; for they are liable to occur anywhere, where these membranes are distributed. But, according to recorded experience, they are rarely met with except in a few localities.

These are, in the uterus,* in the bladder,† in the mouth, antrum, and lip, on the pia mater,‡ pleura, and peritoneum. But the instances which do not belong to the mucous membrane of the mouth are few and isolated. The mucous membrane of the lower lip, where it passes into the epidermic tissue, is a very favourite locality, and the growth formed here is the so-called *cancer of the lip*.

These epithelial tumours have the same structural peculiarities wherever found. They are composed of epithelial cells, or the remains of such, combined with low cell-like or vesicular products, all lying in a coagulated, greasy stroma. At the earliest period of their growth, these cells cannot be well distinguished from those of the normal tissue with which they lie in contact; but they soon show their pathological character by their want of uniformity of size and shape, and their short lives. Thus, they seem to experience the variety of form and shape which is just compatible with their preserving their epithelial-cell type, and, being without function, their membranes burst, after which they are inclosed as flattened scale-like forms, in the cell-liquid and unorganizable plasma which has coagulated. This unorganizable plasma contains fat both combined and free; and crystallized fat or cholesterin is often found. Very rarely do they contain much or even any fibrous tissue. The whole consists of an abortive cell-product, and therefore without object and without limitation. The physical peculiarities of these tumours due to their particular locality need not be mentioned here; they belong rather to the practical than to the histological study of the subject. But

* Lebert; Physiologie Pathol. &c. tom. ii. p. 16.

† For those of this kind I refer to the cases of *trichiasis*, recently so thoroughly studied by M. Rayer. *Vide* Memoirs de la Soc. de Biologie, à Paris, 1850, p. 167.

‡ Lebert; *loc. citat.* tom. ii. p. 67.

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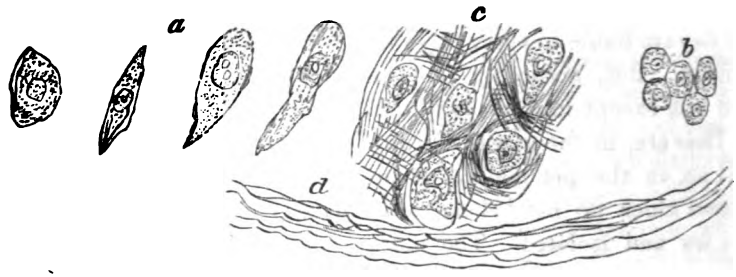
that occurring on the lip, or the so-called *labial cancer*, needs, however, from its frequency and notoriety, to be separately spoken of. Until within a few years past, it has been regarded as a true cancerous product; and even now it is regarded by some pathological microscopists as the transitionary form of the non-malignant into the malignant tissue.

Fig. 43.



Example of the cell-constituents of epithelial epigeneses. From "cancer" of lip. *a*. Abnormal and irregular epithelial cells. *b*. Normal epithelial cells from point where epigenesis arose. *c*. Cells and fibrillated tissue, the latter forming the framework of the epigenesis. *d*. Fibrillated tissue isolated.

But the most careful and thorough histological studies of this tumour do not indicate that it is malignant under any circumstances. In fact, such is my own view, based upon an experience not very limited, and that, too, with this question in view. Moreover, its whole history and economy is of the same import, and, as far as my own observation goes, there is no proof that it is even connected with a constitutional dyscrasia; and I will add that its liability to return always depends upon local instead of general causes.

A question of considerable both histological and practical importance has arisen of late. It is whether, by the aid of the microscope, these simple epithelial tumours can always be discriminated from cancerous products occurring in the same locality.

In giving an affirmative answer to this question, I know that I am expressing more than many pathological microscopists would be willing to allow. But falling back upon my own experience, which I may be allowed to say has not been inconsiderable, this is the opinion which I maintain, and, were it not out of place, I might cite here several prominent instances, illustrating quite well the basis of this view. If it were asked in what consisted the distinction, I should reply, in the absence of all individual cell-type in the one (cancer); and the presence of it in the other (epithelial epigenesis); and the appreciation of these differences in every, or almost every,

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instance, is something not to be described or figured, but to be learned only by long-continued study of morbid products.

Meckel* laid down the law that morbid tissues take upon themselves the morphological characteristics of the normal tissues with which they lie in contact. It might be argued that, this law being true, cancer occurring in an epithelial tissue would be so epitheloid as not to be distinguished from a simple epithelial product.

But it is my opinion that this law of Meckel holds true only of homœomorphous products. In fact, like the "law of analogous formation," of Vogel,† it seems expressive only of the same condition of nutrition that, on a preceding page, I have intended to convey in the terms *perversion* of the *type-power*. But it does not extend to that other condition, *perversion of the plasma*, which is the source of heteromorphous products. But as we are not yet thoroughly acquainted with all the phenomenal conditions of either physiological or pathological products, this is a point upon which, from due respect to science, a *positive* opinion might well be deferred. At all events, the course of study I have pursued has led me involuntarily to believe that the line of demarcation of these various products is much more clear and distinct than would be admitted by other observers.‡

2. THOSE OCCURRING UPON THE SKIN.

These constitute the so-called epidemic tumours, which include not only formations upon the skin, such as nails, horn, hair, and external tumours; but also formations *in* the skin, or beneath its surface, such as encysted growths, the former being only an *eversion*, while the latter is an *inversion* of the skin. The contents of these external tumours

* Meckel; Manuel d'Anat. gen. descrip. et Path. &c. trad. de l'allemand, Paris, 1825, tom. ii. pt. ii. p. 218.

† Vogel; Traité d'Anat. Path. gen., French ed. Paris, 1847; or, transl. by Dr. Day, Amer. ed. p. 114.

‡ The bibliography of epithelial epigeneses is confined almost entirely to that of the so-called *labial cancer*. The following is a reference to some of the cases, and writers who have devoted particular attention to the subject:—

Warren; Surgical Observations on Tumours, &c. Boston, 1839, p. 342.

Valentin; Repertorium, 2 ter Abth. p. 311, 1838.

Gluge; Anat. Mikros. Untersuch. II. Heft, p. 186, 1838.

Velpeau; Lec. Oral. de Clin. Chirurg. Paris, 1841, tom. iii. p. 138.

Walsh; *loc. cit.* p. 255.

Lebert; Physiol. Path. &c. Paris, 1845, tom. ii. p. 876; alo, Traité prat. des Mal. Cancer, &c. Paris, 1851, p. 611.

Bennett; On Cancer and Cancroid Growths, Edinburgh, 1849.

I have observed, microscopically, upwards of 25 of these epigeneses.

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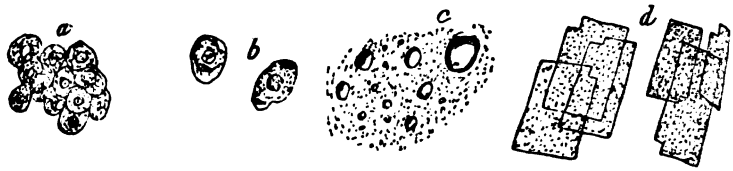
(such as *condylomata*, &c.) need not here be detailed, for, both generally and microscopically, they are the same as of the epithelial ones just described. In the instances of horn, hair, &c., these last follow so closely in the line of the normal tissue, that, were it not for their irregular occurrence, they could not well be discriminated therefrom; but their want of the normal tissue individuality is very apparent. In regard to cystic or follicular growths in the skin, they are, as I have just remarked, simple *inversions*, and are cavities (the external opening or origin of which may be closed up partially or completely) of greater or less size, lined with an epithelial membrane, and filled with epithelial products, mixed up with those forming in a very low plasma, or secreted by the contiguous glands. Such products consist of coagulated albumen, free and combined fat, cholesterin, and cerumenous matter. They often attain a large size by simple displacement, and good examples of them in their smallest form are not unfrequently seen on the end, or about the base, of the nose.*

Fig. 44.



Example of the cell-structure of epidermic epigeneses. When *upon* the skin, or eversions, their structure is exactly like that of epithelial tumours. The above are appearances of the inversions, or tumours beneath the surface. *a*. Layer of pavement epithelial cells lining the sac. *b*. Isolated cells. *c*. Free nuclei, in globules. *d*. Cholesterin. All from an encysted tumour of the breast.

* The literature of the epidemic tumours is very scattered, and owing to the singular classifications of morbid growths of some authors, they are not easily referred to. They are generally included under the head of *encysted tumours*, when they are situated *beneath* the skin. But when they are *eversions*, or above the skin, they have been known and described by a variety of names. But for more especial descriptions, based upon microscopical evidence, see—

Gluge; *loc. cit.* part iv.

Vogel; *loc. cit.* Amer. ed. p. 220.

Lebert; Phys. Pathol. tom. ii. chap. i.

Walshe; *loc. cit.* p. 544, *et seq.*

Bennett; On Cancer and Cancroid Growths, Edinburgh, 1849, *passim*.

In the follicles of the nose there lives a parasitic animal (*acarus folliculorum*), first described by G. Simon (Müller's Archiv, 1842, p. 212). It has since been thoroughly studied by E. Wilson (Lond. Philos. Mag. June 1844, and Phil. Trans. 1845). I have seldom found it, and think it quite rare. It belongs to this locality, just as the *Pediculus capitis* does to the head.

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3. THOSE OCCURRING IN THE OVARIAN TISSUE.

Under this head I include all cases of the so-called Cystic Disease of the Ovary. In other words, I regard this form of ovarian disease as simply an epithelial epigenesis developed in the ovarian stroma, and, more specially, this upon the Graafian vesicles.

This view may appear singular, but I base it upon a pretty extended investigation of the subject, recently made. I cannot here enter into the full details of this consideration. But the leading features of this doctrine may be briefly and concisely stated, as follows: On a preceding page, I remarked that, morphologically speaking, the ovum was only an epithelial cell, and an ovary may be considered as being only a local (and the only one in the body) collection of epithelial cells, destined for a definite object. But, until fecundated, these epithelial cells are subject to those morphological laws which appertain to all cells. The human ovary, then, is simply an epithelial tissue, and, therefore, we ought not to be surprised to find it subject to those morbid conditions which are constantly occurring in this same tissue elsewhere. Its whole structure consisting solely of epithelial cells lying in a fibrous stroma, the number of these morbid conditions must be quite few. In fact, they are almost necessarily limited to an epigenesis of the cells of the stroma. Experience shows that in almost every case this belongs to the cell-structure, and therefore an epithelial epigenesis may be truly said to embrace almost the whole list of ovarian diseases.

If we carefully refer to the subject, it will be seen that the human ovary is a tissue in which we might well expect disease would occur. Until the age of puberty it is passive, and exercises no function. But after this epoch it is the seat of a periodical disturbance. Each month there occurs in it a temporary congestion, leading to the evolution of a peculiar product, the ovum.*

Its nutrition as a tissue, therefore, is constantly disturbed, and with individuals whose general condition is not what is termed "healthy," this disturbance may lead to a permanent change, which, once established, is constantly fostered by each ensuing monthly period. This disturbance, known by the name of *ovaritis*, constitutes, in one sense, nearly the sum total of ovarian disease. This, modern

* I adopt unhesitatingly the view of Bischoff, that an ovum is discharged each month, in the human female.

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experience of some value and extent has quite clearly demonstrated, and upon data of a gross character alone.*

From this abnormal nutrition, an epigenesis arises, and the Graafian vesicles, instead of pursuing that line of normal development which would lead to the evolution of ova, deviate therefrom, and monstrous enlargements and growths ensue. As closed sacs, they are filled with liquid, and, this increasing, they rapidly dilate, and, having attained a certain size, are lined by an epithelial membrane, which arises according to the law of analogous formations. This last is truly indicative of the epithelial origin of the cysts, and we could have no better evidence of their histological nature. The gross appearance of an ovary filled with cystic disease is well known—a collection of sacs of all sizes, which may or may not communicate with each other; in fact, it is only and especially in those cases where the abnormal development has been general and pretty uniform, an enlarged condition of the normal structure of the ovary; for a thin slice of a healthy ovary has, under the microscope, very much the aspect of ordinary cystic disease; and I have often thought that, could the same be magnified with a solar microscope, we should have an appearance quite like that of the advanced cases of this disease. As it is of an epithelial epigenesis, every structure appertaining to it has an epithelial character; especially is this true of the pavement epithelial layer lining the sacs. In the liquid of these sacs may be found floating epithelial cells, single or in clusters. But in an epigenesis of this size and character, the abnormal nutrition is manifested otherwise than by its simple growth. Active inflammatory conditions are always met with, and these give rise to the various products here observed. Such are collections of pus and pyoid forms, effused blood, albuminous matter, fat, free and confined, and cholesterin. These, together with the serum, constitute the products and contents of the cysts. The uniformity of these results in an histological point of view, can scarcely be appreciated except by those who have studied these morbid products microscopically. And, notwithstanding the variety of forms under which ovarian disease appears and has been described as based upon gross appearances, yet, microscopically, they are (judg-

* On the Pathology of Chronic Forms of Ovarian Disease: by E. J. Tilt, London, Lancet, 1849, vol. ii.

Also, by the same; On Diseases of Menstruation and Ovarian Inflammation, London, 1850.

Dr. Tilt has collected 248 cases of this disease, traceable directly or indirectly to menstrual irregularities, or uterine disturbances.

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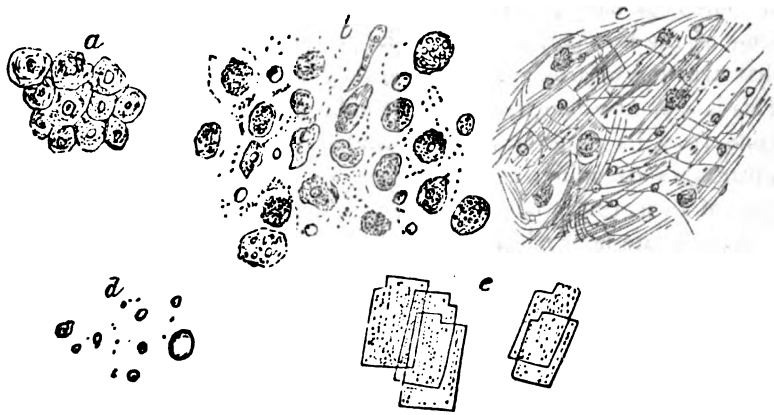
ing from my own experience), all resolvable to these epithelial elements, and others due to inflammation. Take an ordinary case: We find that the larger cysts have thickened walls composed of a fibrillated granular tissue, the result of inflammation. New vessels have formed on or in its substance, and its inner surface is lined with an epithelial layer, except perhaps in the most severe cases, where various inflammatory products of albuminous, fibrous, and granular forms take its place. The liquid contents of the sac are of a corresponding nature; for when the sac has a true epithelial lining, the liquid is mostly serous and clear, although there are, floating in it, flakes of epithelium peeled off from the inner surface. But when the cyst is large and its inner surface is covered with inflammatory products, in place of the epithelial membrane, the contained liquid is more dark, and its contents more heteromorphous in character; such are pus and inflammatory corpuscles, the latter of which are crowded with dark granules, and often attain a size so as to be distinctly seen by the unaided eye.

Fig. 45.



Examples of microscopical forms met with in ovarian cysts. *a.* Pavement epithelial cells, forming the lining membrane of all the cysts. *b.* The pus, pyoid, inflammatory, and irregular epithelial cells formed in the liquids of the sacs, the dark or brown colour of the liquid being in a ratio corresponding with the amount of inflammatory products. *c.* Structure of the *encephaloid-looking matter*, often found on inner surface of cysts, being composed of fibrillated tissue inclosing granular corpuscles, fat, and albuminous matter. *d.* Free fat in globules. *e.* Cholesterin.

In the excess of the inflammation, the capillaries may have burst, and then, blood forms one of the ingredients. These various products, in their different combinations, give rise to the peculiarities of the contained liquids of the sacs, which vary as to colour, as is well known, from that of straw to that of dark chocolate. All these

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various appearances can be generally noticed in an ordinary case of this disease, and, in quite an extended series of cases which I have had the good fortune to study, I have never failed to trace clearly the epithelial epigenetic relations of the affection.

It is not for me to say that cysts in this organ may not, and do not, sometimes, have another origin, being, as Rokitansky* observes, "new formations from the beginning." But cystic disease of this nature I have never had the good fortune to observe, and I cannot easily conceive of its occurring in an organ such as the ovary, composed of only vesicles and stroma. Real carcinomatous disease of the ovary, attended with an enlargement of the Graafian vesicles, sometimes occurs, and we have an additional evidence of its real nature, in the fact that the same disease existed in other portions of the body. Such cases have been recorded by Baillie, Cruveilhier,† and Rokitansky. But, without doubt, such cases are quite rare; and on those recorded cases, in which the presence of malignant disease was not met with in other parts, I cannot place much reliance; for, in the more advanced forms of common cystic disease, appearances are observed having all the gross aspects of real carcinoma; and some of the best pathologists of the present day have acknowledged that, trusting to gross appearances, they have been deceived thereby. This is a point which well might be enlarged upon with interest, for, aside from its importance, there could be well illustrated in it the advantages attending, in fact, the necessity, of the microscopic analysis of all morbid products.

At this late hour of our labours it would not be proper to pause to vindicate, or even to urge, a course of inquiry, the truth of which we have necessarily taken for granted all the while. From its not uncommon occurrence, and its almost necessary fatality, epithelial epigenesis occurring in this tissue has an importance quite exceeding that of its occurrence elsewhere. These relations, however, are more of a practical nature, and I have desired to look at the subject only in an histological point of view. Even in this light, I should not have discussed it thus fully, had I not felt the justness of asserting my claim for the priority of the view of *the single nature of the cystic disease of the ovary, it always being an epithelial epigenesis of the Graafian vesicle.*‡

* Rokitansky; Pathol. Anat. Syd. ed. vol. ii. p. 332.

† Cruveilhier; Anat. Path. du Corps Humain, &c. Paris, lib. v.

‡ For a farther discussion of this interesting subject I may refer to an article of mine, illustrated by cases, Cystic Disease of the Ovary, as elucidated by Microscopical Inquiry, Charleston Med. Journ. vol. vii. Jan. 1852, p. 27.

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It now remains for me to notice, by way of conclusion, another form of cystic disease of the ovary, which is so allied with the preceding that it may well be alluded to in this connection. I refer to those ovarian cysts in which are found hair, teeth, and sometimes bones. I have had a few opportunities to examine these piliferous cysts, and in every instance they had all the characteristics of enlarged Graafian vesicles. They were lined with an epithelial layer. When we consider that hair is, histologically, only metamorphosed epithelium, it is not difficult to comprehend that it should sometimes arise as well in these localities as elsewhere. The facts as stated by Cruveilhier, that it is found in males, and also in other than sexual organs of the body, would lead us to conclude that it is to be regarded only as one epithelial epigenesis occurring within another, and having no essential dependence upon the ovary. Cysts of this kind have undoubtedly a close relation with those constituting encysted dropsy of this organ; and, from all I have seen, may be referable to the same causes.

Of that other class of cases, coming, however, in the same category, in which teeth and pieces of jaw-bone are found in ovarian cysts, many curious examples have been recorded. It has not been my good fortune to see any of these cases, and concerning them, therefore, I am entitled to no scientific opinion. But this much I will say: that the time has now arrived when our knowledge of the relation of the sexes, based upon careful experience, enables us to justly take a decided view of the matter. With regard to the human female, the hypothesis that a new individual being, or parts of such (thus showing that a new individuality had at least commenced), can be produced without the conjunction with the ovum of the fertilizing fluid of the male, cannot, in the present state of science, be for a moment entertained. I well know that, in many of the remarkable cases recorded, the virgin aspect of the sexual organs is strongly insisted upon. But in all these matters, we must consider that it is unphilosophical to place an hypothesis founded upon statements of which we are not absolutely positive, against a view founded upon common experience and supported by all analogy.*

* For cases of this kind deserving notice, see the following: Anderson, Ed. Med. and Surg. Journ. vol. ii. p. 180; Cruveilhier, Anat. Path. livr. xviii. and xxxvi.; Abernethy, Med. Chirurg. Trans. vol. i. p. 35; Clapp, Lond. Med. Gaz. vol. xliv. p. 282. Also, Nouv. Dict. de Méd. et de Chirurg. Prat. art. Ovaire.

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II. Vascular Epigeneses.

Under this head, I do not mean the new vessels formed in tumours of either a heteromorphous or a homœomorphous nature, for such are direct prolongations of the normal vessels, and can only be regarded as morbid from the relations under which they are found. They exist in virtue of the law of congruity of tissues; that is, there must be a nutritive supply, where the nutritive function is to be performed. Strictly considered, therefore, they are physiological and not pathological formations. Not so is it, however, with true vascular epigeneses, which are found holding no relations of function. Such are the erectile tumours, collections, or little knots of bloodvessels, either venous or arterial, existing usually directly beneath the skin. They generally include in their midst a small quantity of areolar tissue, and are capable of a temporary erection like the ordinary erectile tissue. In almost every instance they are located somewhere on the upper portion of the body. In by far the majority of cases they are obviously congenital, and I think there is reason to believe that, in those few instances where they seem to arise anew after birth, this is really not so, but is due rather to a sudden increase of that which has been formed congenitally, but so small as not to be perceived. We are, however, quite imperfectly acquainted with the morbid conditions on which these epigeneses depend. Like all morbid products, they appear to be, as far as I have examined them, infra-formations; and, although there are vessels, yet these are much less perfect than the healthy ones on which they rest.

In an histological point of view they possess but little interest. When examined microscopically, after the tumour has been removed from the body, they will be found composed of vessels with quite tenuous walls, all bound together by a delicate areolar tissue, which shoots up between the meshes. This areolar tissue is an embryonic formation; that is, the common areolar tissue persistent on a low stage of development. The cells, therefore, are fusiform, and have not passed into true fibres.*

* These tumours have been known by several names—thus: Telangiectasis, Hæmatoma, Hæmatoneus, Nævus vasculosus, Aneurysma per anastomosin, &c. &c. Its literature is very scattered, and I cannot here notice it.

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III. Muscular Epigeneses.

The muscular tissue, as we have seen on a preceding page, is a true embryonic formation, and on this account we should not expect to find it subject to the morbid phenomena of epigenesis.

This I believe is really the case. For, although possessing a well-marked nutritive energy, there is no evidence, as far as I am aware, that it is ever repaired after it has been removed by accidents or wounds. In all these instances, and especially when analyzed by the microscope, the tissue of repair has been found to be of a fibroid character, the true muscular element being absent.

In regard to its real epigenesis in a distinct form, there is yet no evidence based upon direct observation that it ever occurs.* That it is thus produced, has been inferred from the consequences only. But for my own part, I am quite disinclined to admit it on such premises alone.

In the instances where the voluntary muscles of the extremities or the muscular tissue of the heart become greatly increased in size, there is no evidence, based upon microscopical analysis, that this increased size is due to the formation of new fibres. On the other hand, as far as I have examined the matter, it seems referable to the fact that the fibres, already existing, gain in size, exactly as we have seen to be the case with the increase of this tissue from the young animal to the adult.

The enlarged volume of muscular tissue due to increased function, as in the instances of that of the heart and those of the extremities, is therefore properly not a pathological, but a physiological phenomenon. It is an *hypertrophy*, rather than an *epigenesis*.

Those cases in which it has been asserted that this tissue is formed in exudations, and where the evidence that it is muscular tissue is not based upon a microscopic examination, I cannot for a moment admit as being worthy of consideration. They are probably founded on error, and, on an histological point of this importance, I should be quite unwilling to admit any but the very best evidence.†

In regard to the epigenesis of the non-striated muscular fibre,

* Since the above was written, I have met with two notices of the alleged appearance of true muscular fibres; one by Rokitansky (Weiner Zeitschrift, 1849), in a tumour of the testicle, the other by Virchow (quoted in Gluge's Atlas of Path. Histol. transl. by Leidy, p. 84), in a tumour of the ovary.

† See Müller's Archiv, 1834, p. 451.

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Vogel* thinks it sometimes occurs, and has given figures representing it. These appearances I do not remember to have met with in any tumours, and on that account I would not wish to appear to deny the fact of its occurrence. Still, if *ever* really an epigenesis, it is at least rarely so; and much care is necessary in discriminating between it and certain fibroid tissues which are not uncommon, and which simulate very closely the appearance of organic muscle.

IV. Nervous Epigeneses.

What has just been said of the muscular, is quite applicable to the nervous tissue. It is an embryonic one, and one, too, having the same or even a higher physiological dignity. Its occurrence, therefore, under abnormal conditions, would not be looked for. I am quite inclined to doubt if it ever is renewed after injuries.† It is true that the continuity is made up by a tissue seemingly capable of performing, to a certain extent, the nervous function, but there is no evidence that this tissue exactly resembles the true nervous tissue which has passed away. I know that this view is not acceded to by Steinrück,‡ Nasse,§ and others. Still, more careful and extended inquiries are needed; and I feel pretty confident that, unless the cases be in very young individuals, the view of the non-reparation of true nervous substance will be found to be the correct one. In regard to real epigeneses of nervous tissue, we have as yet no good evidence that they ever exist. In the cases of hypertrophy of nerves, and the so-called nervous tumours (*neuromata*), the new tissue is not that of nerve, but one of a delicate fibroid character. Such, at least, has been true of some few cases I have had the good fortune to analyze microscopically.

There is an additional reason why we should not expect the occurrence of the true nerve-tube under these abnormal relations. It is this: We have seen that it is formed from nerve-cells, and these are originally formed from the vitelline globules; and we should little look for the formation of these cells in the track of a nerve, and especially when the nutrition was abnormal.

I do not wish to limit my views by my own experience; still, I am not at all inclined to the opinion that new formations of true nerv-

* Vogel, *loc. cit.* Amer. ed. p. 176, pl. iv. fig. 4, and pl. vii. fig. 2.

† I refer here only to the human subject; for, as is well known, and as I have myself had the good fortune to observe, this tissue in common with the others is renewed in the restored parts of tritons, salamanders, &c.

‡ Steinrück; De nervorum regeneratione, Berlin, 1838.

§ Nasse; Müller's Archiv, 1839, p. 405. See also the same, 1840, p. 270.

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ous tissue ever take place, excepting, perhaps, in very young individuals, where the *type-power* of the tissue still persists to a sufficient extent.*

V. Cartilaginous and Osseous Epigeneses.

Here we touch upon the abnormal relations of tissues of less rank and physiological dignity, and we shall find them much more clearly marked and unequivocal in their nature. And although from their intimate connection I have included them under a single head, yet we shall best look at them separately.

1. *The Cartilage Epigenesis.*—For the first description of this product in an intelligible manner we are indebted to Müller.† From both its chemical and microscopical peculiarities he gave it the very proper name of *enchondroma*. It is a true epigenesis, and consists of a product exactly resembling normal cartilage of the transient form. By both Müller and subsequent writers, it has been described as taking its origin both upon bone and in soft parts quite distinctly separated from it.

Regarding it, as I do, as a true epigenesis, I cannot consider that it ever originates except in contiguity with its analogous tissues. For this reason, I am inclined to distrust the statements of those authors who allege to have met with it in the interior of tumours and other soft tissues. The fact that, in almost every case of this kind, the evidence that the tissue in question was cartilaginous, was based upon a gross, and not upon a microscopic examination; this, I say, would be sufficient to lead me to distrust their conclusions, even was there no histological point at stake. Much less, therefore, should I be willing to accept any such assertions to be true, when, in so doing, I should be introducing a new feature into our views of the relations and powers of tissues; for, as I have remarked on a preceding page, the declaration that a new individual tissue can arise *per se* in the midst of tissues of a dissimilar nature, is pretty much the same as asserting the doctrine of *spontaneous generation*.

* At the end of these remarks upon the epigenetic relations of the muscular and nervous tissues, we might properly take up the consideration of the subject of the regeneration of lost parts in the lower animals. The relations of this process and that leading to epigeneses in general are somewhat similar; but then again they are different, the one (epigeneses) being due to an *asthenic*, while the other (regeneration) to a *sthenic* condition of the type-power of tissues.

† Müller; Ueber den feineren Bau der Geschwulste, Berlin, 1838, pp. 31–49; or West's translation.

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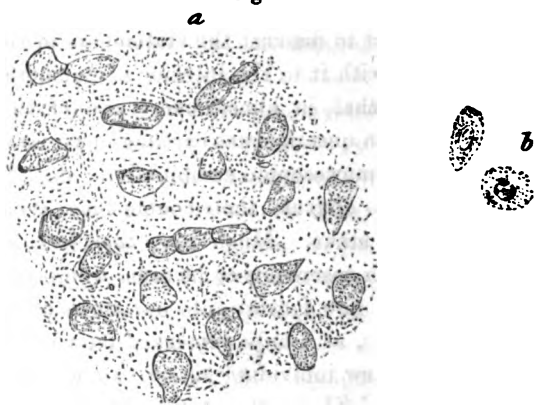
The number of even all the alleged cases of this kind is quite small, and a very small proportion of these were examined by the microscope.* In these few cases, I am inclined to the opinion that, as is often true of encysted tumours, they had, originally, a connection with the analogous tissue; but which growth, and change of form and size, had finally quite obscured. As usually occurring, its favourite locality is about the joints of the hands, and it may be said without exaggeration that nine out of ten of all the cases recorded have been upon these parts. Vogel† makes a distinction between *cartilaginous exostosis* and *true enchondroma*, and thinks that true enchondromata do not ossify, they being the fibrous form of cartilage. My own experience does not warrant this distinction, and as yet we seem quite unacquainted with the conditions leading to ossification in these products. Removed from the body, the incised surface presents a smooth, semitranslucent, turnip-like aspect. Under the microscope, appearances usually quite like those of normal cartilage are seen; a granular or semi-fibrous stroma, in which lie cells which are nucleated and irregular in aspect.

Fig. 46.



Microscopical appearances of enchondroma. *a*. Cartilage-cells in a finely granular stroma. *b*. Cells treated with acid, showing nucleated character. From specimen taken from the hand.

Ossification here takes place, as in normal cartilage, by the deposition of calcareous matter in the stroma or cells. Very rarely,

* As far as I am aware, this remark is true of those of Müller and Bennett only. See references at the end of this section. The pamphlet of Mr. Gamjee, ("On a Case of Ossifying Enchondroma in the Testicle of a Horse,") quoted by Mr. Paget (Lect. in London Med. Gaz. Aug. 1851), I have not seen.

† Vogel; *loc. citat.* Amer. edit. p. 211.

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however, does it proceed either very regularly or extensively, and in some few of these growths, simple osseous spiculæ are only found.

In nearly every instance, the cause of this form of epigenesis is traumatic, being due to an external injury. But in a case I saw some time ago, and in another, since reported (see that of Cobbald), the disease seemed referable to constitutional causes.

The passage of these tumours into those of a fibroid nature sometimes occurs, by the disappearance of the true cartilage element—the cell. The tumour is then simply a collection of fibrous tissue; but its practical relations are not thereby changed in the least. The opposite change, that of fibrous tumours into enchondromata, I cannot believe ever occurs; because that presupposes that the cartilage element is added spontaneously.

However, all these points have not been well studied; for the microscope has not been made to bear upon them, and we must look to future inquiries to make them more clear.*

2. *The Osseous Epigenesis.*—Pursuant to the doctrines I have inculcated on a preceding page—the invariable origin of bone in cartilage—it is evident that true osseous tumours are only a farther development of those we have just considered. And if cartilaginous tumours are rare, those in which ossification has taken place are rarer still.

It is quite important to correctly distinguish between false and true osseous growths; for the former are generally confounded with the latter; they are not uncommon, and are only calcareous deposits occurring almost anywhere, being due to the interstitial deposition of earthy salts in a fibrous tumour, or some inflammatory product. Such are many forms met with in fibrous membranes

* For farther details, together with some cases, see an article of mine, The Microscopical and Histological Relations of Enchondroma, Amer. Journ. Med. Sci. n. s. vol. xxiii. p. 397. The following is the bibliography of cartilaginous epigenesis, or the so-called *enchondroma*. I have here included *none* of those cases which might well be regarded doubtful: Müller: Ueber den feineren Bau der Geschwulste, Berlin, 1838, pp. 31–49. Gluge; Anat. Physiol. Untersuch. zur Pathol. Minden et Jena, 1838–41, ii. 153–87. Cruveilhier; Anat. Path. du Corps Humain, Paris, 1842, livr. 34, pl. 4–5. Frogley; Med.-Chir. Trans. 1843, vol. xxvi. p. 133. Goodsir; Lond. and Edinb. Month. Journ. Feb. 1843. Hertz; De Enchondromate, Erlangen, 1843. Lebert; Physiol. pathol. &c. Paris, 1845, tom. ii. p. 213. Ancell; Med.-Chir. Trans. vol. xxv. art. xv. Adams; London Lancet, 1847, vol. ii. p. 463. Bennett; On Cancer and Cancroid Growths, Edinb. 1849, Obser. 48–49. Fergusson; London Lancet, 1850, vol. i. p. 455. Lloyd; London Lancet, Dec. 1850. Kingdon; Lond. Med. Gaz. Sept. 5, 1851; also, Cobbald (the case apparently hereditary), Ed. Month. Journ. Aug. 1851.

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everywhere, in tendons in the coats of bloodvessels, and in the interior of various tumours.

But the true osseous substance is of course found only in connection with normal bone, although it possesses all the elements of normal bone; yet, generally, their structure wants that regularity and definiteness of arrangement found in normal bone. More than this, it has no histological peculiarities, and its clinical and practical relations are the same as those of enchondromata, from which they always arise.*

VI. Fibroid Epigeneses.

We have come upon the lowest class of epigeneses occurring in the economy. Under the term *fibroid*, I include not only *fibrous*, but *fibrinous*, *fibro-plastic*, and all other allied forms which have received many and various names.

For convenience sake, a division into several varieties, as authors have done, may be necessary; but histologically, the case is different, for they are composed of a single and the same element, although existing under some variety of form.

The frequency of their occurrence, and the wide range of their localities, is in perfect keeping with the commonness and universal distribution of this tissue.

If our subject was the fibrous growths only, we might pass it by with a very few remarks, and yet do it justice; for, both in their histological composition and in their clinical and practical relations, they have the most indifferent peculiarities. It is true that they have favourite localities, such as the uterus, the nasal fossæ, &c. and also that they sometimes seem to possess a kind of individuality; but generally they have none other than the most simple vegetative relations. To this point, however, I shall revert hereafter.

By *fibrinous* products, I mean those due to the extravasation of

* The literature of osseous tumours is much less extensive and full than that of those of cartilage. Many remarkable cases may be found scattered through journals and periodicals. Something, however, may be learned of them in the following works: Gluge; Atlas der Path. Anat. pt. ii. Müller; Ueber ossificirende Schwämme oder osteoid Geschwulste, &c. Müller's Woch. heft v. pp. 396–442. Ducrest; Mém de la Soc. Méd. d'Obser. de Paris, t. ii. pp. 318–452. Lebert; Phys. Pathologique, &c. Paris, 1845, tom. ii. pp. 219–241. Vogel; *loc. citat.* Amer. edit. pp. 185 and 212. I do not here refer to the literature of the older and well-known authors, both because they are well known, and because their cases, although seemingly true ones, were not proved to be positively such by microscopic analysis.

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fibrine from the bloodvessels, which afterwards becomes firm and undergoes changes, so that it is nourished and perhaps increased in the system as an analogous product. Being therefore due to traumatic causes, an extravasation rather than an exudation, it is scarcely an epigenesis in the true sense of that term. Velpeau,* in his brilliant exposition of the phenomena of contusion, called the attention of surgeons to this subject, upon which he placed the highest importance, regarding, as he did, this sanguineous effusion as the matrix of all, or nearly all, the fibroid growths in the body. I cannot here enter into an analysis of the points of this somewhat celebrated theory. It holds true in surgery much more than medicine; that is, with tumours due to direct external causes, rather than with those of a latent and constitutional origin.

The plasma of the blood, together with its colouring matter, is extravasated, and instead of being absorbed, remains as such. The colouring matter disappears, and a kind of semi-organization takes place in the product. A fibrillated tissue is the result, inclosing imperfect globules together with granules. Histologically speaking, therefore, they are of the lowest character, and I have thus briefly alluded to them, to avoid the appearance of omission in the line of my discourse.

The remaining form of fibroid epigenesis, the fibro-plastic or sarcomatous tumours, is of more importance, both histologically and practically.

Until the work of Lebert,† these tumours received a variety of names, according to the different gross aspects they presented.‡ But Lebert gave them this definite and appropriate name from a microscopical study of their elements.

Their elements are uniformly the same, and in a general classification may well serve as the basis of a distinct name. Nevertheless, in a true histological point of view, the elements are the same as those of fibrous tumours, the peculiarity of the tissue being due to the stage of development they have attained. On this account, I have included these epigeneses under the term of *fibroid* products. The histological peculiarities of these tumours may be briefly stated to be those of the fibrous form, resting on their embryonic stage of development. Therefore, instead of long slender fibres, matted to-

* Velpeau; De la contusion dans tous les organs, Thèse de concours, 1838.

† Lebert; Phys. pathol. &c. Paris, 1845, tom. ii. pp. 120–160.

‡ Among these names may be mentioned Sarcoma, Steatoma, Osteo-sarcoma, Osteo-steatoma, besides, Fibrous Tumour, &c.

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gether, forming the fibrous tumour, there is found a tissue made up of fibres and cells in all stages of transition, into fibres which are elongated, fusiform, &c. In the younger forms, the cells greatly predominate, but in the older ones, the fibrous tissue constitutes a large ingredient.

These cells are well formed, nucleated, and closely resemble those of epithelium. And, in many cases, where this epigenesis occurs in glandular organs, it is so combined with epithelial products, as to leave a doubt in the mind which was the original epigenesis.

Fig. 47.

Example of the microscopical structure of fibro-plastic epigeneses, from a "sarcomatous" tumour of the submaxillary gland. *a*. Cells changing in part to fibres, but not losing their cell character. *b*. The same bound together in a regular manner, as sometimes found. *c*. Fibrillated tissue accompanying the cell-growths.

The microscopic elements, then, of these fibro-plastic tumours may be stated to be cells, fibres, and granules; the fibres being the product of the cells, and the cells being of an epitheloid type. My own experience with these growths has been upon those springing from some glandular organ; and in all these cases, there was involved, to a variable extent, a morbid epithelial product. And although there is reason to believe that the primitive epigenesis in these cases is in the fibrous tissue of the gland, yet it afterward necessarily includes the epithelial structure. Judging, therefore, from the *result* we have at last, I should say that the definition of fibro-plastic tumours, as we usually meet with them, is *a fibrous and epithelial epigenesis combined*. This also will convey a good idea of the microscopic elements usually met with. However, in some that do not involve any epithelial element, the whole tissue is composed of fusiform and fibre-like cells. As to the localities of appearance, these tumours occur

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throughout the economy; but, as I have before said, they are generally connected with glandular tissues—such are the lymphatic, parotid, maxillary, and mammary glands. In almost every instance I have examined, the locality was one of these parts. According to the experience of Lebert, however, they are not unfrequently connected with the skin, and with mucous and serous membranes. The practical relations of these tumours are the same as those we have just spoken of. They are, of course, perfectly innocent in their nature and tendency, and the aid of the surgeon is required to check their rapid growth and consequent mechanical inconvenience; when once *entirely* removed, there is no liability to a return. In fact, I may say that their rapid increase is nearly the only reason for their being objects of especial regard, for they rise and increase generally without pain, and do not involve constitutional disturbances.

In closing these remarks upon *fibroid epigenesis*, it may be proper for me to allude briefly to a collateral point. This is, whether these fibroid tumours ever degenerate into cancer. With many of the elder surgeons and surgical writers it has been a favourite opinion that a tumour may be innocent at first, but afterwards degenerate and become malignant or cancerous, and they think that the fibroid tumours are those in which such changes usually take place. If we are to judge of tumours and their peculiar nature by their clinical history alone, such an opinion might be considered correct. But I need scarcely add that the opinion now is, that the soundest and most complete experience with these growths does not justify any view except that of the singleness of their nature throughout the whole period of their existence; that is, a cancer is a cancer from the beginning, and the same is true of other growths. I will add that, since the microscope has been made to bear upon these points, I am not aware that there is any evidence that such metamorphoses take place. If a tumour, originally considered innocent, afterwards proves itself cancerous, it is far more probable that it was cancerous from the beginning than that it afterwards became so. From the scheme of the origin of pathological products I have laid down on a preceding page, it will be remembered that cancer was regarded as a heteromorphous, and fibrous tumours as homœomorphous products, due in fact to quite different abnormal phases of the nutrition. On this account, it would, *à priori*, be difficult to conceive of their occurring together; from this reason alone, aside from the sufficiently auxiliary ones we have in experience, I should say that the hypothesis of the degeneration of fibrous growths into cancer is contradicted

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by the best and clearest views we possess of these abnormal tissues.*

VII. Adipose Epigeneses.

Under this head, I do not refer to the abnormal (?) increase and development of the adipose tissue generally, constituting obesity or corpulency; for, in most respects, this is to be regarded as merely a hypertrophy. On the other hand, I refer to the local appearance of the fatty tissue under circumstances and forms justly intimating that it is due to a perverted nutrition. Such are the so-called fatty tumours, and other adventitious fatty growths in various parts. A careful distinction must also be made between these true histological products and those due to a simple deposit of fat in the shape of globules in various tissues. The adipose tissue is as truly an individual one as any we have considered, and although of a common nature, and low physiological dignity, we are to look for the same rigid histological relations as in the others. True fatty tumours, therefore, are always epigenetic, and, when containing the true fat-cell, they invariably take their origin from the normal tissue. Their microscopical structure† so closely resembles that from which they spring, that they appear to be only a hypertrophy of the healthy tissue. In these cases I invariably found fat-cells imbedded in a fibrous stroma. The relative quantity of each of these elements varies, and in this fact may be found the reason of the variety of forms given by writers. Thus Müller‡ speaks of these products under two species, *Lipoma* and *Cholesteatoma*, and under each of these gives several varieties. But, I repeat, you cannot apply the scientific term *species* to pathological formations.

Common fatty tumours, taking their origin beneath the integu-

* Owing to the commonness of their occurrence, the literature of fibroid epigeneses is quite full. I cannot here even think of referring to the elder surgical writers who have written upon this subject. Below is a considerable portion of the literature of the subject, since it has been studied histologically and microscopically.

Gluge, *loc. citat.* part iv. Fasergeschwulste; Valentin, Repertorium, 1837, p. 270; Müller, Uber den feineren Bau und die Formen da Krankhaften Geschwulste, or West's transl. pl. iii. figs. 14, 15; Amussat, Mém. sur. l'Anat. Path. des Tumeurs fibreuses de Uterus, Paris, 1842; Berard, Diagnostic différential des Tumeurs du sein, Paris, 1842; Lebert, Physiol. path. &c. Paris, 1845, tom. ii. pp. 83, 120; Vogel, *loc. citat.* Amer. ed. p. 199; Bennett, On Cancer and Cancroid Growths, Edinb. 149.

† I give no example of the microscopical structure of the adipose epigenesis, for, in nearly all respects, it is the same as the normal tissue.

‡ Müller; *loc. cit.* West's transl. p. 152.

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ment, not unfrequently lobulated, and known generally by the name of *lipomata*, present a pretty uniform aspect. But then, again, they may vary according to their locality, and to the relative amount of the different elements their formation may involve. In some instances they are so much fibrous that Abernethy* was led to conclude that oftentimes they were only fibrous tumours that had absorbed fatty elements. This opinion is of course incorrect, although from clinical studies and gross examinations it might seem to be borne out.

The direct causes of these growths are equally as obscure as those of any other morbid products we have considered. They are sometimes congenital, and may occur at any age, or in any part of the body. A very common locality is the subcutaneous tissue of the back and shoulders, and, according to statistics, they are more common in females than in males. The fact that they not unfrequently arise simultaneously in different parts of the body, and this, too, without apparent cause, seems to indicate, not only that their ulterior causes may be constitutional, but also that we are far from being acquainted with their general etiology. They are, of course, non-malignant in themselves, but life may be endangered from the enormous size they sometimes attain. Like all homœomorphous products, they are liable to return if care is not taken that their matrix is entirely removed. Fatty degeneration of, and fatty deposition in organs, both internal and external, have nothing to do with the subject in question. They are not only histologically different, but are due to other causes.†

With these remarks, I close the subject of special organized pathological products. It is true that, in so doing, I omit a notice of diseased forms, which have been described under distinct names in various works. Such, for instance, are the melanotic and colloid products. I have not spoken of the first of these, because they seem to be only other growths of both an heteromorphous and homœomorphous character, of which pigment-matter forms an ingredient. Judging from my own studies they have no other peculiarity, and do not deserve a distinct name except for convenience sake. As for the

* Abernethy; Surgical Observations, London, 1814, p. 9.

† The literature of this form of epigenesis is equally as loose as that of the others we have considered. As histological examinations of tumours are of recent origin, it is difficult to ascertain the true nature of many growths spoken of by the older writers. The following are some of those who have studied the subject microscopically: Müller, *loc. cit.* p. 152, *et seq.*; Gluge; Abhandlungen zur Physiol. und Path. 1839, p. 130, 1841, p. 185; Heyfelder, De lipomate et steatomate, Leyden, 1842; Lebert, *loc. cit.* tom. ii. p. 100, *et seq.*; Bennett, On Cancer and Cancroid Growths, Edinburgh, 1849.

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colloid products, they may be regarded in a somewhat analogous light; for the most part they seem to be cancerous formations, containing an unusual quantity of albuminous and gelatinous material. My own experience of several cases is to this effect alone, and I know of no reason for the opinion that the *colloid* should constitute a distinct characteristic rather than an accidental one.

The domain of pathology is so extended, that it may well be prudent for me that I do not enter upon the discussion of points not directly relative to the subject in question. The part which cells play in pathological products is certainly very extensive; they may be said to constitute the chief, but not the sole actors; and, therefore, in treating of the subject of pathological products, as far only as appertains to their peculiarities and parts, I necessarily treat it quite imperfectly; but, at the same time, not necessarily indistinctly or obscurely, for these studies lie at the very foundation of the whole structure.

The distinct subject of the pathological relations of the blood, I have not touched upon, although they are of the most important and fundamental character. Its imperfect condition at the present time would necessarily preclude any special detail of scientific value. There are very many difficulties in the way, which must be removed before microscopic examinations of the blood will furnish, of themselves alone, positive data in the determination of disease. It is evident that by the microscope we can appreciate only two abnormal modifications of this liquid; the first, is imperfections in the structure of the red and white corpuscles, and the second, the absence of their normal mutual numerical relations; in other words, variations in the structure and healthy proportions of its solid component parts. But restricted as the field thus is, I am confident that it will yield much upon careful study and inquiry. My own observations have shown me that the microscope may indicate blood changes long before they would be suspected from the general aspect of the individual. It might, indeed, seem a fair question, how far blood extracted from the end of the finger would be a fair sample of that of the system generally; but it is much more so than would at first be supposed, as I have had reason to believe from continued experiments. However, in such examinations, the blood should be taken at different times in the day, and always observed without the use of water. It is to be hoped that this subject will receive the careful attention it deserves.

The department of *unorganized epigeneses* must likewise be

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omitted. It contains many interesting chapters, and one of them, not the least so, is that relating to the *urine*; and this is so, not only because this excretion is a good exponent of the chemico-vital condition of the system generally, but because its peculiarities can be most beautifully made out by the aid of the microscope. But I will not dwell farther upon these inviting topics.

Pathology, as manifested in cell-studies, we have seen to be not an entirely distinct, but only an erring, fallen condition of physiology. A thorough appreciation of this conclusion is of more value than might be supposed. It is true that these pathological cells often have the exact physical semblance of those of health, but if their imperfection is not traceable in their physical organization, it is manifested in their meaningless destiny. More than this we cannot well insist upon if we remain in the domain of strict science.

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PART III.

PHILOSOPHY.

THE PHILOSOPHICAL RELATIONS OF CELL-STUDIES.

Most of the philosophical relations of cell-studies may, I think, be comprised under two heads, viz: 1. The relations of cells to the *physical* or the *teleological* view of organization; 2. The direct agency of cells in the production and manifestation of nervous power, the intellectual processes, &c. I propose to consider each of these separately, and at some length.

I. The Relations of Cells to the Physical and to the Teleological View of Organization.

The cell is the fundamental unity of organized forms. It is the chosen material form through which organization gains its first expression. To use a metaphor, it is the only altar on which life and matter are married, to pursue together a common and definite end.

These premises being true, as all modern inquiry clearly proves, we can justly seek, from a study of this cell, a manifestation of all the conditions of organization, as far as they are capable of being made evident through material forms. We can also expect, by a consideration of these conditions, to be able to comprehend the higher and ulterior relations of organization as occurring in permanent individual forms.

Two dissimilar views of the real nature of organization have been urged in modern times.

That the bearings of the subject in question may be fully understood, a brief description of both of these views is here required. The first is, that every individual organism exists in virtue of a predominant idea; this idea, therefore, preceded the organism, of which this last is the true material manifestation. Organisms are, therefore, if I may so express myself, the incarnations of pre-existent

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ideas in nature. An organized form is simply a colligation of molecules of plastic matter occurring under the direction of a determinate idea or force. This force must, it is evident, be peculiar, and differing from any belonging to matter generally. It is an immaterial one, and can be likened to no other in nature with which we are acquainted. In its conception we should not confound it with our idea of life; for between them a broad distinction can be drawn.

Thus our idea of life is necessarily connected with, and dependent upon, that of organization; because, from the limitations of experience, we have not and can not conceive of it as a distinct and separate entity. On the other hand, our conception of this determinate idea, or that of the *individuality* of organization, although involving some of the same data as that of life, occurs from relations sufficiently dissimilar to show well its distinction. Thus, while our idea of life is only *coexistent* with that of organization, our idea of this individuality is not only *coexistent*, but necessarily pre-existent to it. We do not comprehend life except in conjunction with matter under peculiar forms; on the other hand, we conceive of this determinate idea or individuality as existing before a trace of organization appears.

This view of organization, which widely separates the organic from the inorganic world, as we can comprehend each through the expression of its forces, which gives an inherent dignity to individual existence, because there is always an end in view—this view of organization, I repeat, is called the *teleological* one.

The other and second view is fundamentally different. Adopting it, one does not recognize that the forces of organized are more in number or different in character from those of unorganized matter, the fact of organization being due to a certain combination of powers possessed by all material forms. And when this combination has once taken place, there necessarily results in virtue of it, and the forces impressed on matter in its beginning, a certain end, which is called individual existence. The common phenomena of organization, therefore, are due to the blind working of the laws of necessity, and which are irrespective of any purpose; they also involve no conditions excepting those properly belonging to chemistry and physics.

According to this view also, the so-called "individual adaptation" does not result from a determinate idea seeking an expression, but rather ensues from a combination of blind forces which the Deity impressed upon matter in the beginning. Design in nature, therefore, would be only another set of terms for necessity of physical condition.

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Again, when we see the habits and conformation of an animal *suited* to the circumstances under which it is found, we are not to attribute this to an idea pre-existing before all, and seeking its material expression, but are to regard the whole in the light of a necessity; for the very fact of the existing state of conditions implies that, were they different, the animal would not exist, and therefore the very data we reason from determine the idea of necessity, as far as justly applicable to works of an Almighty power.

This view of nature, which ranges all its phenomena under the domain of physical forces, thus giving, in one sense, the same dignity to inorganic as to organic forms—this view of organization, I repeat, is called the *physical* one.

In this brief description of these two widely dissimilar views of organisation, I may have failed to express clearly the grounds of their distinction; for nothing is more difficult than the successful expression of the exact definitions of mere modalities of matter. In a laconic and perhaps comprehensive form, I would say that the *teleological* view is the full definition of *Development*, while the *physical* view is the equally full definition of *Combination*.

We will now turn to the arguments for and against each of these views, at least as far as dependent upon cell-studies.

The first question is, does the adaptation perceived in the organic differ in essential character from that perceived in the inorganic world? An affirmative answer must, I think, be given to this question. The adaptation seen in the inorganic world has reference only to existing circumstances, and the only surety of its continuation is in the persistence of the forces on which it depends. Such, for instance, is that of the planetary system. It is properly called an adaptation only in virtue of the existing harmony found. But this harmony, however perfect, indicates no definite end, which is the final cause of its existence.

On the other hand, with organic nature, other and different relations are seen. For, besides the adaptation just spoken of, and which conduces to its general harmony with matter, there is superadded an *individual* adaptation of the whole to an end seemingly in view from the beginning. Indeed, we can truly say, with Kant, that this end constitutes the very definition of an organized form.

In all our investigations into the structure of animals and plants, we involuntarily (in one sense) proceed upon the principle that *nothing was made in vain*. If, at any time, we have even a misgiving

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of the whole truth of this principle, our scientific ardor, as hopeful of real results, is immediately relaxed, not to say harmed. This involuntary admission of a doctrine, shows how secretly yet how firmly is our mind, in its study of natural phenomena, linked with the *teleological* view of organization.* We cannot free ourselves from it, any more than we can free ourselves from the silent yet constant admission of the truth of the great physical doctrine that *nothing comes by chance.* This question, therefore, which I have put, might well seem not only superfluous, but highly unphilosophical. But I was led to commence with it from the fact that a very sagacious mind† thinks it correctly answered in the negative. We will, however, pursue this subject a little farther. I have said that the adaptation of the inorganic world has reference to existing circumstances only, while that of the organic world, beside being individual, has always reference to events or contingencies of the future. This point is well illustrated in the phenomena of reproduction.

When animals were first created, the footing on which they commenced life was the same in each. But why, accepting the *physical* view of organization, were their powers of reproduction formed exactly in accordance with their probabilities and liabilities of existence, as all zoological experience constantly shows is true? It is the characteristic of physical forces that they act upon the conditions of the time, and no physical force can act on the probability of future contingencies. The amount of animal and vegetable life on the earth's surface, in localities not disturbed by human or other agencies of an accidental nature, has not changed for an indefinite period of time. In those places the mutual relations of animals and vegetables exist now as we have reason to believe they did in the beginning.

There is no evidence that there has hitherto been a constant oscillation, and that the present rest and harmony is one ultimately and necessarily gained. Now in all these animals and plants, we find their reproductive powers, the capacity of their organs of generation, exactly in accordance with their liabilities to destruction. Were this

* I think it may be truly said that the strength and ever-constant presence of this principle in the minds of great naturalists, has been the grand secret of their success. To prove that this is so, the instances of many notable men might be adduced. In the case of Cuvier this was pre-eminently true, and it served as an ever-faithful intuition to open to him domains of knowledge hitherto untrodden. There is another naturalist now living, whose splendid labours, as I know personally, have been based upon this same principle.

† Schwann; Microskop. Untersuch. &c., or Sydenham Soc. ed. p. 187.

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the place, the truth of this remark might be illustrated by many special instances. But it is evident that, by this almost prescient adaptation, the numerical relations of species are preserved unchanged; and those who are familiar with the leading results of zoological research at the present day, can scarcely entertain this subject in the light of a question.

The next point of discussion is, whether, admitting organization to exist always in virtue of *determinate ideas*, it is not unphysical and unphilosophical to assert, as we must, that these ideas or peculiar forces exist previous to the material forms in which they are ultimately expressed? This question is so specious that at first an affirmative answer appears to be the only correct one. But a little consideration will, I think, show its error. Our knowledge of physical phenomena is derivable from observation only; and when points are urged upon us which we have not observed, and which also do not seem compatible with hitherto existing observation, they are liable to be pronounced unphysical. But the history of any of the inductive sciences shows that the singularity of any hypothesis or opinion concerning physical phenomena is no *à priori* argument against its truth. This remark is aptly true of the present case. The hypothesis that matter should have determinate ideas, not expressed by physical phenomena at the time, but which are latent, and to be developed under special forms in future, this hypothesis, I repeat, should not be rejected on the ground alone of its singularity; and much more will be admissible if its singularity can be disproved. This disproval may be found, I think, in the numerous analogous phenomena of generation. Here we are driven to the admission of the existence of forces, long before the formation of the material organs in which they ultimately find their expression.

Take, for instance, the *cicatricula* of a robin, and that also of a hawk; physically, they resemble each other exactly, and no microscopical analysis can detect between them any material difference. Yet no one will pretend to deny that in the one lies concealed all that belonging to a robin, and in the other all that which belongs to a hawk. This will bear no other interpretation. Here you have very dissimilar powers and forces contained in apparently identical material forms; and unless we declare that these different powers and forces existed before and aside from the material form, instead of gradually accruing to it as a particular character was assumed, we shall be at a loss to account for the certainty and uniformity of result in every case. Again, the primordial material condition of every animal is a nucle-

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ated cell, which in every case has exactly the same physical aspect. Now in its growth to the perfect form, the new material constantly added is extraneous, and there would appear no reason why it should always suit the conditions of the case, were there not an underlying pre-existing idea. Indeed, we might expect an occasional blending of dissimilar forms. But this never occurs, and few points connected with organization have been more positively determined than the distinct, unchanging individuality of animal and vegetable types.

If this point needs farther illustration, or even that which is better, we can find such in the male phenomena of generation. It may be well to discuss briefly a few of these points.

Within a few years, three prominent facts have been satisfactorily established concerning the function of the spermatic particles. These are, 1st. That they are the only fecundating particles of the semen. 2d. That a few of these particles are sufficient to fecundate a single ovum. 3d. That the physical phenomena of fecundation are those of simple contact alone, of the spermatic particles with the ovum; there being no material loss on the part of the former.

We know very well how important and active a part the male plays in the production of the new being, and I scarcely need add that the offspring partakes equally of the physical peculiarities of both parents. But in no way can it receive those of the father except through the medium of the spermatic particles. And so, however strange it may appear at first, yet the conclusion is irresistible, that a single spermatic particle must contain, concealed within it, not only the general physical peculiarities of the father, but mental dispositions also, and, as is too often true of our own species, morbid taints superadded to all.

The third and last point in the discussion of this subject, is the objection to the teleological view of organization urged by Schwann,* and that, too, concerning the general phenomena of the subject we have just left—generation. I will quote the entire passage, that there may be no misapprehension of the idea intended to be conveyed. He says: "If we assume each organism to be formed by a power which acts according to a certain predominant idea, a portion of this power may certainly reside in the ovum during generation; but then we must ascribe to this subdivision of the original power, at the separation of the ovum from the body of the mother, the capability of producing an organism similar to that which the power, of which

* *Loc. citat.* Syd. Soc. 2d, p. 189.

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it is but a portion, produced; that is, we must assume that this power is infinitely divisible, and yet that each part may perform the same actions as the whole power. If, on the other hand, the power of organised bodies resides, like the physical powers, in matter as such, and be set free only by a certain combination of molecules, as, for instance, electricity is set free by the combination of a zinc and copper plate, then also by the conjunction of molecules to form an ovum, the power may be set free by which the ovum is capable of appropriating to itself fresh molecules, and these newly-conjoined molecules again by this mode of combination acquire the same power to assimilate fresh molecules." Schwann is here evidently urging the doctrine of catalysis in organic forms. This same doctrine has lately been pushed to its limit, for similar reasons, by Mulder.*

And thus it would appear that some, in the study of organic phenomena, are willing, rather than appear what they call "unphysical," to summon to their aid the blindest physical forces, instead of admitting the existence of those which are special and distinct, and constantly urged upon them. The prominent objection Schwann urges, is the physical absurdity that the division of a power or force does not decrease it.

But this is falling back upon the properties of physical forms only, in which the mathematical axiom, that *a part is less than the whole*, is true. But it is the peculiarity of immaterial agents that they cannot be subject to the same definitions as those of a material nature. We have yet to escape from a strange confusion of ideas on these points, and, when considering these higher forces, we must not tacitly deny their higher and immaterial character by subjecting them to material definitions. For on these grounds, the very existence of that of which we feel most certain, namely, *thought*, might well be denied. In fact, I think many of us have yet to learn that the mathematical axiom above quoted holds true of the immaterial agents.

Perhaps this remark is true even of electricity, for in the phenomena of *catalysis*, we witness the transference of force or quality without an apparent loss from the body, from which the transference takes place.† To urge that only that which is substantial can be

* The Chemistry of Vegetable and Animal Physiology, Eng. trans., Edinb. 1849, pp. 28, *et seq.*

† In regard to this point, Mulder (*Loc. cit.*) takes a different view. He thinks that in the case of a magnet magnetizing a piece of iron, there is no *transmission* of force, but simply an awakening of slumbering forces in the iron. It might well be asked if

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transferred, is to reason on the ground that a phenomenon in nature is impossible because it eludes our observation and analysis.

In the phenomena of generation, we certainly have proved to us that there may be a division of power or quality without a real loss; in other words, that there may be an indefinite number of divisions, and yet each of these possess the capability of the whole from which the division was taken or made.

Thus, as I have before said, the spermatic particle is the potential representative of the whole male being. It contains within it the totality of the peculiarities of the individual in which it has been formed. These spermatic particles are constantly formed, and, in the case of any of the higher animals, literally millions are yearly evolved; yet each possesses the powers of the other, and of the whole animal; and the animal certainly loses none of its individuality by such a constant division.

The objections of Schwann to the teleological view of organization, are, therefore, more specious than valid, because the reality of the very conditions objected to has an expression in quite common phenomena.

These same teleological views might be shown to be almost imperatively forced upon us by a general review of developments imperfectly attained—the subject of monstrosities. Here you not unfrequently have conditions of organization, in which imperfections of physical form reveal distinctly the perfection of the underlying determinate idea of organization.

In special cell studies connected with the subject of generation, however, as we have seen, these doctrines of teleology find their most unequivocal support.*

this makes the matter any more clear? You are obliged, on this supposition, to admit the existence of *latent forces* in the iron, and this is "unphysical," according to his own admission. Mulder says: "A weak force is strengthened; it is impossible to imagine the transference of a force from one material mass to another." But, is it not truly "unphysical" to suppose that a weak force can be strengthened except by the *addition* of new force?

* It may be urged that in all cases it conduces much more to the object of science to strive, at least, to adopt the *physical* instead of the *teleological* explanation of the phenomena of organic life. This is so because it leads us in our investigations rigorously to exclude the admission of those various hypothetical forces and conditions, which ultimately serve as a universal refuge in the instances of inexplicable phenomena. On the other hand, if we seek to base all phenomena on physical grounds, we tend to exclude the admission of conditions not having their corresponding expression in physical forms. This objection, therefore, rests rather upon our liability to error in interpreting natural phenomena, than upon real grounds. It must be regarded in the

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In conclusion, I think it may be truly said that one of the tendencies of the most intimate physiological, microscopical studies of the present day, is to show the inadequacy of microscopic and chemical investigations to detect physical differences in structures essentially dissimilar. Once appreciating this fully, we are prepared for the ulterior doctrine that, in organization, matter is but the vehicle for the phenomenal manifestation of determinate ideas, which last only, therefore, have an objective reality.

II. The Direct Agency of Cells in the Production and Manifestation of Nervous Power, the Intellectual Processes, &c.

The subject which we have just discussed—that of a determinate teleological idea running through each organized form—is certainly one of a very high and important character, for, upon its admission, depends not only that of the doctrine of Final causes, but also many of our highest and grandest conceptions of nature as the work of an Almighty hand.

However this may be, it does not exceed in importance or magnitude the one which is now before us; for this last involves the question of the relations of the Thinking principle itself.

On a preceding page, when discussing in a general manner the subject of nerve-cell function, I briefly alluded to the present point, but I purposely deferred taking it up in a more complete manner, until the present time.

The point in question has nothing to do with the nature of the immortal Psyche—the immaterial mind; these are, by a wise Providence, beyond the pale of all scientific discussion. Our business is a humble inquiry into the conditions of the evolution of nervous power, and the manifestation of intellect, as referable to cell life and action.

In the first place, it cannot be denied that all nervous and mental phenomena are, in some way, directly connected with vital changes in nervous matter; this nervous matter is the nerve-cells which, exclusively, compose the potential part of the nervous substance. I would repeat, therefore, that whenever there is a manifestation of nervous power or intellect, its physical source lies with nerve-cells or their analogues, whether it be high or low in the animal kingdom.

light of a necessary *caveat* connected with the fallibility of our intellect. There is, however, as we have seen, a danger that, in endeavouring to be strictly *physical*, we may become highly "unphysical," not to say unphilosophical.

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Another point, equally well sustained, is, that there is a direct correlation between the capacity for such manifestation, and the physical amount of these nervous cells. So far the subject is clear. It next remains for us to inquire if there are sufficient grounds for the supposition that there is a difference in the quality of the forces resulting *after* (I do not say *from*) the action of these cells; in other words, is all nervous manifestation the same in essential character, the apparent difference being due to mere form of its expression or conditions of its mode of action? As a purely physiological question, I should say that the tendency of all our best studies is to answer it in the negative. It is true that nerve-cells, wherever found, have exactly the same physical appearance, but then the wide difference of their capacities cannot be well referable to merely different forms of the same essential function. I would, therefore, make a distinction between that portion of the nervous substance which is connected with the individual, voluntary life of the being, and the remaining portion, which is subservient to the mere economical preservation of the life of the animal. The action of the former ends in what is called intellectual manifestation; and that of the latter in the evolution of simple nervous force. Now, with the application of the usual physiological criteria, we may regard the nervous force as a power *sui generis* which belongs to the nervous cells in virtue of their constitution, exactly as belongs muscular force to muscle. This force is the resultant functional effect of these cells, as vital, ever-changing parts. Its evolution, therefore, must be attended with organic changes in these cells; which changes are included in that cycle of actions—waste and repair—which aggregately constitute their life. Whatever causes, under the form of stimuli, induce an increased functional activity of these cells, this increased action is followed by an evolution of a larger amount of nervous power.

The relations, therefore, of nerve-cells to nervous power or force seem to be, potentially, as much those of cause and effect as any connected with the manifestation of organic forces elsewhere; and, in one sense, the nervous power may be regarded as the *product* of the nervous cells—the function of these last being to eliminate this force in virtue of their organic constitution. But we cannot use the word *eliminate* here in the same sense as it is used when speaking of the mammary cells eliminating milk; for the latter is an elaborated, metamorphosed product, whereas, as to the former, physiology has not yet decided—nor can I see how it ever can decide—whether it

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is a distinct entity, or whether it is a manifestation of what may be termed mere conditions of matter. It is quite sufficient that our recognition and perception of it is *active*—that is, as a *force;* and, considered as such, we may, finally, regard it as indissolubly connected with matter under an organized condition. In conclusion, it may be added that the action of the nervous cells, for the elimination of simple nerve force, may be, in one sense, spontaneous (though it may be questioned if, even here, it is not the result of irritation), as in the splanchnic nervous centres; or it may be due to stimuli: these stimuli are either internal, the will; or external, through irritation.

But with that portion (cerebrum) of the nerve-cells concerned in the intellectual act, other, if not wholly dissimilar relations of action occur; and the capacity of these cells for those vital processes which terminate in intellectual manifestation, must belong to them *per se*, in contradistinction to those of the other nervous centres, such as the spinal cord or its analogue, &c., belonging to them in virtue of their organization. The primary intellectual act must always be referable to an external stimulus acting on the brain; this stimulus is an object, which is brought in connection with these cells by means of the senses. The primary intellectual act, therefore, is one and the same in character (exactly as in the mere nervous force) whenever occurring, and the mere perception of an idea must always be the same as to simple quality.

In this respect, then, the intellectual conditions of man would not differ in mere quality from those of any of the lower animals; and in the perception of an object, constituting the naked idea, man would have no advantage over the brutes in the lucidity of that act: thus, the idea of a tree, merely as an external object of a peculiar form, &c., is probably just as lucid and clear in the mind of a dog as in that of a man. This simple intellectual act is no more mysteriously connected with the agency of the cells of the cerebrum than is the manifestation of nervous power with the nerve-cells of other nervous centres. The nerve-cells which form the outer layer of the retina receive the optical image of all objects, and, as such, this image is perceived by the consciousness; but, truly, in itself, this act is no more incomprehensible than our perception that an object is round or square by feeling it with the hand.

This direct agency of the nerve-cells in the simple intellectual act, appears to be the only ground that can be taken, if we consider the character of many mental phenomena; and, thus adopted, it would seem to accord with, or, indeed, in one sense explain, some of the

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conditions of mental action, which, with the purely *spiritual* view of the mental relations, seem wholly irreconcilable with observed facts. All who have watched the physical conditions of their intellect, know that after severe and protracted mental occupation, there is just as sensible a feeling of weariness about the brain, as there is with the muscles of a limb after the latter has been severely used. Mental action, then, whether it be in the simple act of perception of external objects, or in the combination and colligation of previously acquired ideas—constituting abstraction—must invariably be attended with vital organic changes in the cells of the cerebrum.

This conclusion, thereby closely connecting the conditions of intellectuality with mere material forms, is one which we must accept according to the laws of physiology. It may be asked, how can such doctrines be reconciled with the never-failing wonders of Memory—that voluntary substitution of the past for the present, and that, too, with the truth and beauty it had when really the present—when the agents (cells) of the intellectual conceptions are ever changing and passing away. To this deeply interesting question, I think physiology has an adequate answer. Let me make some explanation. An intellectual act occurs by the perception of an external object, the material structure of a nerve-cell or cells has thereby been changed, and this change, whatever it may be, is one and indivisible, and, in my opinion, is never completely effaced. Like the cicatrix of a wound, it persists to indicate the occurrence of a past physical effect. It is not overgrown or removed because of the exactness of the nutrition of these cells; the physical impressions made are preserved, and when afterwards, by means of association or combination of ideas, they are visited, as it were, by the consciousness, they represent the pictures of the past. It is not necessary to continue this topic farther, or to point out the relations of this exquisite nutritive act to the phases of memory in youth, manhood, and old age; they all accord with the more or less organic plasticity of the brain at these different epochs.

But this simple intellectual act we have been considering, and which we have intimately linked with the physical condition of nerve-cells, is the lowest form of mental action. The higher forms of mental activity belong to the colligation and comparison of previously acquired ideas, and the deriving of conclusions from such method. This is accomplished under the superintendence of that incomprehensible power we ever feel—the Will, a kind of metaphysical Ego. These mental processes constitute what is called abstraction, which seems

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to be the distinguishing feature of man's intellectual condition from that of the brutes, as far as we are able to judge; for there is no reason to believe that animals ever have a true mental abstraction. It is true that they sometimes act as though they really possessed such a power, but these instances may all, perhaps, be referable to the laws of association and combination in the simple intellectual acts. Man's nobility, then, lies in his exclusive possession of a deeply mysterious power, inseparably connected with, yet quite different from, his consciousness. A power to do or not to do as may be; a power which may induce mental action in this or that direction without limit, and which, therefore, is the supreme of stimulants. This is the Will—the immortal Psyche—which the innermost depths of our consciousness tell us holds no relations with matter, for it never grows weary or becomes borne down with labor, but even when the intellectual acts have ceased to become distinct, it retains that freshness of action belonging to its unearthly nature. Physiology and Psychology, therefore, are not only two distinct sciences, but they even do not pass into each other. By Physiology, in this connection, we learn the necessary relations of cell life and action to the conditions of the simple intellectual act; whereas, by Psychology, we seek to learn the ever-varying changes of this intellectual act due to the agency of the immortal will. Physiology, therefore, as connected with cells, would, indeed, be sadly deficient, did it not indicate to the humble student, as he stands upon the confines of its domain, the necessity of conditions and springs of action connected with our mental life, which lie above and beyond any mere conditional relations of ever-changing, decaying matter.

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OF THE

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* By a resolution of the Association, adopted at the annual meeting in Baltimore, the Committee on Publication are "desired to append to the Proceedings of the Association, each year, a catalogue of its officers and permanent members."

In preparing the Catalogue, in accordance with the above resolution, the Committee have been guided,

1st. By the article of the Constitution which declares that "the permanent members shall consist of all those *who have served in the capacity of delegates*, and of such other members as may receive the appointment by unanimous vote." Farther: "Every member elect, prior to the permanent organization of the annual meeting, or before voting on any question after the meeting has been organized, must sign these regulations, inscribing his name and address in full, specifying in what capacity he attends, and, if a delegate, the title of the institution from which he has received his appointment."

2d. By the resolution adopted by the Association, at its meeting in Boston, "that any delegate who is in arrears for an annual assessment shall not be considered a permanent member."

To have served as a delegate, or received the appointment by unanimous vote, signed the Constitution, and paid the annual assessment, are thus essential to permanent membership. The neglect of some of the delegates to comply with *all* these conditions will explain the omission of their names from the catalogue. The difficulty of deciphering, in many instances, the signatures of the members, must be pleaded in extenuation of such errors of orthography as may be detected.

The names, and also the addresses, with a few exceptions, are given exactly as registered, the Committee not having the means of designating all the changes of residence, or of indicating all those whom the Association has lost by death. The latter, as far as known, are marked thus † For the correctness of future catalogues, it is requested that the secretaries may be informed of all errors and omissions in the present one.

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Clapp, Sylvanus	*Pawtucket.*
Cleaveland, H.	*New Providence.*
Clifford, Lewis W.	*Providence.*
Collins, George L.	*do.*
Dunn, Theophilus C.	*Newport.*
Ely, James W. C.	*Providence.*
Fowler, Ezekiel	*Woonsocket Falls.*
King, David	*Newport.*
Le Prohon, E. P.	*Providence.*
Mauran, Joseph	*do.*
Miller, Lewis L.	*do.*
Parsons, Charles W.	*do.*
Parsons, Usher	*do.*
Pratt, H. P.	*do.*
Rivers, H. W.	*do.*
Snow, Ed. M.	*do.*

CONNECTICUT.

Barrows, A. W.	*Hartford.*
†Beach, Samuel	*Bridgeport.*
Beardsley, Sheldon	*North Branford.*
Beardsley, L. N.	*Milford.*
Bennett, H. N.	*Bridgeport.*
Beresford, S. B.	*Hartford.*
Bishop, E. H.	*New Haven.*
†Bowen, Samuel	*Thompson.*
Bradford, Milton	*Woodstock.*
Brigham, Norman	*Mansfield.*
Bronson, Henry	*New Haven.*
Brownell, Clarence M.	*East Hartford.*
Campbell, Harvey	*Voluntown.*
Canfield, Joel	*Guilford.*
†Carrington, E. W.	*Farmington.*

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Casey, W. B.	*Middletown.*
Castle, Andrew	*New Haven Co.*
Catlin, S.	*West Meriden.*
Comings, B. N.	*New Britain.*
Dickinson, F. L.	*Willington, Tolland Co.*
Downing, Eleazer B.	*Preston.*
Fowler, R. M.	*Litchfield.*
Hall, Eli	*East Hartford.*
Hammond, J.	*Killingly.*
Hatch, Johnson C.	*Kent.*
Hawley, Roswell	*New Britain.*
Hooker, Charles	*New Haven.*
Hooker, Worthington	*do.*
Hubbard, S. G.	*do.*
Hubbard, Robert	*Bridgeport.*
Hutchins, Samuel	*East Brooklyn.*
Huxley, A. M.	*Goshen, Litchfield Co.*
Ives, Eli	*New Haven.*
Ives, Nathan B.	*do.*
Jarvis, George O.	*Tolland.*
Jewett, P. A.	*New Haven.*
Knight, Jonathan	*do.*
†Lyman, Norman	*Warren.*
Middlebrook, Elijah	*Trumbull.*
Peters, Manly	*Woodville.*
Pierson, William S.	*Windsor.*
Pike, N. S.	*Moosup P. O.*
Platt, Gideon L.	*Waterbury.*
Rogers, Benjamin	*Hartford.*
Russell, Gurdon W.	*do.*
St. John, G. H.	*New Milford.*
Sumner, George	*Hartford.*
Tallcot, Alvan	*Guilford.*
Warner, Richard	*Cromwell.*
Webb, Reynold	*Madison.*
†Welch, Archibald	*Hartford.*
Welch, Benjamin	*Lakeville.*
White, P. A.	*Simsbury, Hartford Co.*
†Witter, William	*Windham.*
Woodward, Ashbel	*Franklin.*
Wright, Albert A.	*Canaan.*

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NEW YORK.

Adams, John G.	*New York.*
Allen, Lucius H.	*Owego, Tioga Co.*
Anderson, James	*New York.*
Armsby, James H.	*Albany.*
Atwater, D. F.	*Brooklyn.*
Austin, James M.	*Waterford.*
Ayres, Daniel	*Brooklyn.*
Ayres, Daniel	*Amsterdam.*
Babcock, Charles	*New Hartford, Oneida Co.*
Babcock, M. N.	*Saratoga Springs.*
Baker, jr., A.	*Norwich.*
Baker, B. Fordyce	*New York.*
Batchelder, J. P.	*do.*
Bay, William	*Albany.*
Baxter, Hiram	*Stillwater, Saratoga Co.*
Beadle, E. L.	*New York.*
Beck, T. Romeyn	*Albany.*
Beebe, Seneca	*McDonough, Chenango Co.*
Bissell, H. P.	*Utica.*
Blakeman, William N.	*New York.*
Blatchford, Thomas W.	*Troy.*
Bolton, Jackson	*New York.*
Borrowe, J. H.	*do.*
Bradford, George W.	*Homer, Cortland Co.*
Brinsmade, Thomas C.	*Troy.*
Brooks, P. A.	*Binghamton, Broome Co.*
Buck, jr., Gurdon	*New York.*
Budd, A. E.	*Medford, Burlington Co.*
Buel, William P.	*New York.*
Bulkley, Henry D.	*do.*
Burton, C. V. W.	*Lansingburgh.*
Burwell, Bryant	*Buffalo.*
Burwell, George N.	*do.*
Calkins, Alonzo	*New York.*
Cammann, G. P.	*do.*
Carr, Edson	*Canandaigua, Ontario Co.*
Cary, Walter	*Buffalo.*
Carter, Galen,	*New York.*
Cash, M. H.	*Ridgebury, Orange Co.*

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Chubbuck, H. S.	*Elmira, Chenango Co.*
Church, W. Henry	*New York.*
Clark, Alonzo	*do.*
Clements, James W. G.	*do.*
Clymer, Meredith	*do.*
Cock, Thomas	*do.*
Cock, Thomas F.	*do.*
Cockcroft, James	*do.*
Coe, A. B.	*Oswego.*
Congar, Horace M.	*Buffalo.*
Cook, Simeon A.	*Buskerk's Bridge.*
Corliss, Hiram	*Greenwich, Washington Co.*
Corsin, J. W.	*New York.*
Coventry, C. B.	*Utica.*
Crandall, R. O.	*Waverley.*
Cuyler, J. M.	*West Point.*
Dalton, J. C.	*New York.*
Dering, Nicoll H.	*Utica.*
Donoughue, E. O'	*Bergen, Genesee Co.*
Doolittle, A. F.	*Herkimer.*
Downs, Henry S.	*New York.*
Draper, John W.	*do.*
Dubois, Abram	*do.*
Dwight, W. C.	*Moscow, Livingston Co.*
Earle, Pliny	*New York.*
Eastman, N. H.	*Owego.*
Flint, Austin	*New York.*
Ford, Corydon L.	*Lockport.*
Foster, Joel	*New York.*
Foster, S. Conant	*do.*
Francis, John W.	*do.*
Gardner, Augustus K.	*do.*
Gilman, Chandler R.	*do.*
Goodsell, Thomas	*Utica.*
Gray, Henry G.	*North White Creek.*
Greene, Caleb	*Homer, Cortland Co.*
Greene, Isaac	*New York.*
Green, Horace	*do.*
Goodyear, Miles	*Cortland.*
Griscom, John H.	*New York.*
Guthrie, C. B.	*do.*

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Hadley, George	*Buffalo.*
Halsted, T. M.	*New York.*
Hamilton, Frank H.	*Buffalo.*
Harvey, Charles W.	*do.*
Hard, P. H.	*Oswego.*
Hart, John	*New York.*
Hart, Samuel	*Oswego.*
Hawley, Joel E.	*Ithaca.*
Henschell, C.	*New York.*
Hubbard, Samuel T.	*do.*
Hubbard, Silas	*Buffalo.*
Hun, Thomas	*Albany.*
Hyde, Frederick	*Cortlandville.*
Hyde, Lucius	*Brooklyn,*
Jackson, William H.	*New York.*
Jacobs, Ferris,	*Delhi, Delaware Co.*
Jenkins, J. Foster	*New York.*
Jerome, James H.	*Trumansburg.*
Jewett, C.	*Buffalo.*
Jewett, Harvey	*Canandaigua.*
Jones, Daniel J.	*Baldwinsville.*
Kissam, R. S.	*New York.*
Lee, Charles A.	*do.*
Linsly, Jared	*do.*
Mac Farlan, Ebenezer	*Williamsburg, Long Island.*
†Manley, James R.	*New York.*
March, Alden	*Albany.*
Markoe, T. M.	*New York.*
McCall, John	*Utica.*
McIntyre, A.	*Wayne Co.*
McIntyre, A.	*Palmyra.*
McNaughton, James	*Albany.*
McNevin, William H.	*New York.*
McPhail, L. C.	*Brooklyn.*
Metcalfe, John T.	*New York.*
Miner, J. M.	*Brooklyn.*
Mitchell, Henry	*Norwich.*
Mixer, S. F.	*Buffalo.*
Morris, R. L.	*New York.*
Mott, Valentine	*do.*
Mott, Walter	*Schuylerville, Saratoga Co.*

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Moulton, Peter	*New Rochelle, Westchester Co.*
Ogden, Benjamin	*New York.*
Parker, Willard	*do.*
Parker, Edward H.	*do.*
Parmly, Eleazer	*do.*
†Pattison, Granville S.	*do.*
Peters, George A.	*do.*
Phelps, James L.	*do.*
Phillips, S. B.	*do.*
Pierce, J. B.	*Lyons.*
Pomeroy, Charles G.	*Newark, Wayne Co.*
Pond, James O.	*New York.*
Post, Alfred C.	*do.*
Punnett, John	*do.*
Purdy, J.	*Elmira, Chemung Co.*
Purdy, Samuel A.	*New York.*
Purple, S. S.	*do.*
Raymond, Charles H.	*do.*
Reese, D. Meredith	*do.*
Reynolds, Tabor B.	*Wilton, Saratoga Co.*
Reynolds, J. H.	*do.*
Rockwell, Wm.	*New York.*
†Rodgers, J. Kearny	*do.*
Russ, J. D.	*do.*
Saunders, A. L.	*Brookfield, Madison Co.*
Sayre, Lewis A.	*New York.*
Shipman, Azariah B.	*Syracuse.*
Skilton, Avery J.	*Troy.*
Smith, Albert,	*New Rochelle.*
Smith, Charles D.	*New York.*
Smith, Joseph M.	*do.*
Snow, Simeon	*Root, Montgomery Co.*
Snyder, Morgan	*Fort Plain.*
Sprague, J. S.	*Exeter, Otsego Co.*
†Stearns, John	*New York.*
Stephenson, Mark	*do.*
Stevens, Alexander H.	*do.*
Stewart, F. Campbell	*do.*
Stewart, James	*do.*
Stewart, Philanda	*Peekskill.*
Stout, Arthur B.	*New York.*

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Swett, John A.	*New York.*
Taft, Marcus L.	*do.*
Taylor, Isaac E.	*do.*
Taylor, Wm.	*Manlius.*
Tefft, Lake J.	*Onondaga.*
Thomas, Daniel	*Utica.*
Thompson, jr., A. G.	*New York.*
Thompson, Alexander	*Aurora, Cayuga Co.*
Thorn, James	*Troy.*
Trenor, Jno.	*New York.*
Van Buren, W. H.	*do.*
Van Buren, Peter	*Albany.*
Vanderpool, S. O.	*do.*
Van Kleek, John R.	*New York.*
Van Pelt, M. D.	*do.*
Van Rensselaer, Alexander	*do.*
Warren, James	*do.*
Watson, John	*do.*
Watts, jr., Robert	*do.*
Weeks, Cyrus	*do.*
West, H. S.	*Binghamton, Broome Co.*
Wheeler, Lewis C.	*Troy.*
White, Devillo	*Sherburne, Chenango Co.*
White, James P.	*Buffalo.*
White, Oliver	*New York.*
Willard, Augustus	*Green, Chenango Co.*
Wilcox, Charles H.	*Buffalo.*
Winton, Nelson	*Havana, Chemung Co.*
Wing, Joel A.	*Albany.*
Wood, Isaac	*New York.*
Wood, James R.	*do.*
Woodward, G. F.	*do.*
Worster, Joseph	*do.*
Wotkyns, Alfred	*Troy.*

NEW JERSEY.

Bateman, B. Rush	*Cedarville.*
Brakeley, P. F.	*Belvidere, Warren Co.*
Butcher, Charles	*Maurice Town, Cumberland Co.*
Butler, S. W.	*Burlington.*
Canfield, J. W.	*Morristown.*

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Chetwood, G. R.	*Elizabethtown.*
Clarke, J. Henry	*Newark.*
†Cole, N. W.	*Burlington.*
Condict, Lewis	*Morristown.*
Cooper, Richard M.	*Camden.*
Craig, J. W.	*Plainfield.*
Dougherty, Alexander N.	*Newark.*
Elmer, William	*Bridgetown.*
Fithian, Enoch	*Greenwich.*
Fithian, Joseph	*Woodbury.*
Garrison, Charles	*Swedesboro'.*
Garrison, J. F.	*do.*
Gibson, Quinton,	*Salem.*
Haines, Job	*Burlington.*
Hendry, Charles D.	*Camden Co.*
Hunt, T. Edgar	*Clarksville.*
Marsh, Elias J.	*Paterson.*
Munn, J. B.	*Chatham, Morris Co.*
Nichols, Whitfield	*Newark.*
Parrish, Joseph	*Burlington.*
Phillips, John H.	*Pennington.*
Porter, E. M.	*Bridgetown.*
Rogers, Alexander N.	*Paterson.*
Pennington, Samuel H.	*Newark.*
Pierson, W.	*Orange.*
Read, Zachariah	*Mount Holly.*
Schenck, F. S.	*Camden.*
Smith, Lyndon A.	*Newark.*
Stratton, B. H.	*Mount Holly.*
Taylor, Othniel H.	*Camden.*

PENNSYLVANIA.

Allen, J. M.	*Philadelphia.*
Andrews, James P.	*Lancaster.*
Ashmead, William	*Philadelphia.*
Atlee, Washington L.	*do.*
Atlee, John L.	*Lancaster.*
Bache, Franklin	*Philadelphia.*
Baker, Charles L.	*Lancaster.*
Bare, A. S.	*do.*
Baskin, George W.	*Mercer.*

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Bell, John	*Philadelphia.*
Betton, Thomas F.	*Germantown.*
Bibighaus, C. H.	*Philadelphia.*
Biddle, John B.	*do.*
Bond, Henry	*do.*
Breitenbach, J.	*Myerstown.*
Bridges, Robert	*Philadelphia.*
Brinton, John B.	*Westchester.*
Brown, H. J.	*Philadelphia.*
Bryan, Joseph R.	*do.*
Bryan, James	*do.*
Burden, Jesse R.	*do.*
Burrowes, Francis S.	*Lancaster.*
Carpenter, Henry	*do.*
Carpenter, James S.	*Pottsville.*
Carson, Joseph	*Philadelphia.*
Cassidy, P.	*Lancaster.*
†Chapman, Nathaniel	*Philadelphia.*
Coates, B. Horner	*do.*
Condie, D. Francis	*do.*
Corson, Hiram	*Plymouth Meeting, Mont. Co.*
Darlington, William	*Westchester.*
Dilworth, Samuel	*Pittsburg.*
Drayton, H. E.	*Philadelphia.*
Duffield, Samuel	*Lancaster.*
Ehler, J. Augustus	*do.*
Ellmaker, Thomas	*do.*
Ely, Edward	*New Hope, Bucks Co.*
Emerson, Gouverneur	*Philadelphia.*
Eshleman, J. K.	*Lancaster Co.*
Foulke, J. S.	*Gwynedd, Montgomery Co.*
Fox, George	*Philadelphia.*
Fronefield, Charles	*Montgomery Co.*
Gaston, A. K.	*Chester Co.*
Gazzam, Joseph P.	*Pittsburg.*
Gerhard, William W.	*Philadelphia.*
Gibson, William	*do.*
Gilbert, David	*do.*
†Grant, William R.	*do.*
Green, Triall	*Easton.*
Gryder, M. R.	*Chestnut Level, Lancaster Co.*

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Haines, William S.	*Philadelphia.*
Hallowell, Edward	*do.*
Handy, J. H.	*do.*
Harry, Samuel H.	*Doe Run, Chester Co.*
Hart, Alexander C.	*Philadelphia.*
Hartshorne, Henry	*do.*
Hatfield, N. L.	*do.*
Hays, Isaac	*do.*
Heger, Anthony	*Pottsville.*
Heister, Frank M.	*Reading.*
Heister, J. P.	*do.*
Hodge, Hugh L.	*Philadelphia.*
†Horner, William E.	*do.*
†Humes, Samuel	*Lancaster.*
Huston, Robert M.	*Philadelphia.*
Innes, Charles	*Easton.*
Irwin, John S.	*Pittsburg.*
Jackson, Samuel (Prof.)	*Philadelphia.*
Jackson, Samuel	*do.*
Janney, Benjamin S.	*do.*
Jewell, Wilson	*do.*
Keith, William	*do.*
Kennedy, A. L.	*do.*
Kerr, James W.	*York, York Co.*
Kerfoot, George B.	*Lancaster.*
King, Charles R.	*Philadelphia.*
Klapp, William H.	*do.*
Lajus, Paul D.	*do.*
Lang, Edmund	*do.*
Lamb, Jno. F.	*Frankford.*
La Roche, Réné	*Philadelphia.*
Leib, Henry F.	*do.*
Lewis, Samuel	*do.*
Lowman, Jno.	*Johnstown, Cambria Co.*
Matthews, Charles H.	*Doylestown.*
Mayburry, William	*Philadelphia.*
McClellan, J. H. B.	*do.*
McCorkle, W. S.	*Columbia.*
McCoy, John M.	*Bellefonte.*
McClintock, James	*Philadelphia.*
McIlvain, William	*York.*

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Meigs, Charles D.	*Philadelphia.*
Mitchell, John K.	*do.*
Mitchell, Thomas D.	*do.*
Moore, John Wilson	*do.*
Morris, Casper	*do.*
†Morton, Samuel George	*do.*
Mowry, Robert B.	*Alleghany City.*
Muhlenberg, F. H.	*Lancaster.*
Muhlenberg, Henry E.	*do.*
Murphey, Andrews	*Parkesburg, Chester Co.*
Mütter, Thomas D.	*Philadelphia.*
Naudain, Arnold	*do.*
Neill, John	*do.*
Norris, George W.	*do.*
Ogier, S. A.	*Frazier, Chester Co.*
Page, William Byrd	*Philadelphia.*
Pancoast, Joseph	*do.*
†Parrish, Isaac	*do.*
Parry, Ely	*Lancaster.*
Patterson, Henry S.	*Philadelphia.*
†Patterson, George W.	*do.*
Paul, J. Rodman	*do.*
Pepper, William	*do.*
Piper, W. A.	*do.*
Pollock, A. M.	*Pittsburg.*
†Randolph, Jacob	*Philadelphia.*
Ream, John	*Lancaster.*
Remington, Isaac	*Philadelphia.*
Richards, C. O.	*Lancaster.*
Richardson	*Brooklyn.*
Rivinus, Edward F.	*Westchester.*
Rodman, Lewis	*Philadelphia.*
†Rogers, James B.	*do.*
Ruschenberger, W. S. W.	*do.*
Sargeant, jr., Winthrop	*Manayunk.*
Smith, H. H.	*Philadelphia.*
Smith, R. K.	*Darby, Delaware Co.*
Smith, Moses B.	*Philadelphia.*
Smith, jr., Francis G.	*do.*
Stewart, J. D.	*do.*
Stillé, Alfred	*do.*

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Stocker, A. E.	*Philadelphia.*
Stubbs, J. B.	*Rock P. O., Lancaster Co.*
Thomas, George W.	*Norristown.*
Thomas, Isaac	*Westchester.*
Thompson, William S.	*Lancaster.*
Townsend, R. H.	*Philadelphia.*
Turnbull, Lawrence	*do.*
Uhler, John	*do.*
Vanbuskirk, Wm. A.	*Pottstown, Montgomery Co.*
Wadsworth, Henry	*Philadelphia.*
Walker, Isaac R.	*Spread Eagle, Chester Co.*
West, Francis	*Philadelphia.*
Wilcocks, Alexander	*do.*
Wilson, William J.	*Potter's Mill, Centre County.*
Wiltbank, John	*Philadelphia.*
Wimley, G. W.	*Limerick Township, Mont. Co.*
Wister, Caspar	*Philadelphia.*
Wood, George B.	*do.*
Wood, Thomas	*Muncy, Lycoming Co.*
Worthington, Wilmer	*Westchester.*
Yardley, Thomas H.	*Philadelphia.*
Zeigler, J. L.	*Mountjoy P. O., Lancaster Co.*
Zeigler, George J.	*Philadelphia.*
Zulick, S. M.	*Orwigsburg.*

DELAWARE.

Askew, Henry F.	*Wilmington.*
Baker, George R.	*do.*
Bush, Lewis P.	*do.*
Couper, James	*Newcastle.*
Cummins, William	*Smyrna.*
Grimshaw, A. H.	*Wilmington.*
Hamilton, William N.	*Cantwell's Bridge.*
Jump, Isaac	*Dover.*
Morris, William W.	*do.*
Perkins, John D.	*Smyrna.*
Porter, Robert R.	*Wilmington.*
†Smith, Thomas Mackie	*Brandywine.*
Thomson, James W.	*Wilmington.*
Wilson, James F.	*do.*

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MARYLAND.

Alexander, Ashton	*Baltimore.*
Armitage, James	*do.*
Atkinson, Thomas C.	*do.*
Backler, Thomas H.	*do.*
Baer, M. S.	*do.*
Baker, Alfred	*do.*
Baldwin, E. C.	*do.*
Baxley, Willis H.	*do.*
Bolton, James	*do.*
Bond, Thomas E.	*do.*
Bordley, James	*do.*
Boyd, Charles	*Frederick City.*
Briscoe, John H.	*Baltimore.*
Chew, Samuel	*do.*
Clarke, S. R.	*do.*
Cohen, Joshua J.	*do.*
Cox, C. C.	*Easton.*
Dallam, William M.	*Harford Co.*
Davis, William H.	*Baltimore.*
Dawson, James	*St. Michael's, Talbot Co.*
Donaldson, Francis	*Baltimore.*
Dorsey, Lloyd	*Frederick.*
Dulin, Alexander F.	*Baltimore.*
Dunbar, John R. W.	*do.*
Duvall, E. P.	*do.*
Duvall, W.	*Washington, Montgomery Co.*
Fonerden, John	*Baltimore.*
Gibson, George S.	*do.*
Gibson, J. Grigg	*Fredericktown.*
Goldsborough, H. T.	*Easton, Talbot Co.*
Gunn, John P.	*Baltimore.*
Handy, Samuel K.	*Somerset Co.*
Harris, Chapin A.	*Baltimore.*
Hemsley, W.	*Caroline Co.*
Hintze, F. E. B.	*Baltimore.*
Hopkins, Joel	*Elkridge Landing, Howard Co.*
Jameson, sen., Horatio G.	*Baltimore.*
†Jenkins, Solomon M.	*Easton.*
Kinnemann, P. S.	*Baltimore.*

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Kinney, jr., William	*Baltimore.*
Leonard, William T.	*do.*
Martin, Thomas W.	*Dorchester Co.*
Miltenberger, G. W.	*Baltimore.*
Monkur, John C. S.	*do.*
Murray, R., (U. S. A.)	*do.*
O'Donnel, D. O.	*do.*
Pinckney, Ninian, (U. S. N.)	*Annapolis.*
Power, William	*Baltimore.*
Riley, William	*do.*
Roberts, George C. M.	*do.*
Robinson, Alexander C.	*do.*
Roby, Joseph	*do.*
Sappington, Thomas	*Liberty, Frederick Co.*
Smith, Nathan R.	*Baltimore.*
Smith, Samuel P.	*Cumberland.*
Steiner, L. H.	*Frederick Co.*
Steuart, R. S.	*Baltimore.*
Stevenson, J. S.	*do.*
Stokes, William H.	*do.*
Teackle, John N.	*do.*
Theobald, Elisha W.	*do.*
Thomas, Richard H.	*do.*
Tyler, Samuel	*Frederick City.*
Warfield, Jesse L.	*Westminster, Carroll Co.*
Webster, John Lee	*Baltimore.*
Williams, W.	*Somerset Co.*
Wroth, Peregrine	*Chestertown.*
Wynne, James	*Baltimore.*
Yeates, John L.	*do.*

DISTRICT OF COLUMBIA.

Boyle, Cornelius	*Washington.*
Busey, S. C.	*do.*
Coolidge, R. H.	*do.*
Garnett, A. Y. P.	*do.*
Johnston, William P.	*do.*
Lindsly, Harvey	*do.*
May, J. F.	*do.*
Miller, Thomas	*do.*
Morgan, James E.	*do.*

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Riley, Joshua	*Georgetown.*
Semmes, A. J.	*Washington.*
Stone, Robert K.	*do.*
Thomas, John M.	*Georgetown.*
Tyler, Grafton	*do.*
Young, N.	*Washington.*

VIRGINIA.

Ashby, C. H.	*Culpepper C. H.*
Atkinson, Thomas P.	*Danville.*
Barr, S. B. F. C.	*Big Spring, Taylorsville.*
Bates, W. J.	*Wheeling.*
Belt, H. S.	*Pittsylvania.*
Bolton, James	*Richmond.*
Broocks, John N.	*do.*
Browne, O. A.	*Hicksford.*
Burruss, J. L.	*Louisa C. H.*
Cabell, James L.	*University of Virginia.*
Carmichael, George F.	*Fredericksburg.*
Cochran, William B.	*Loudon Co.*
Coleman, J. C.	*Richmond.*
Cooke, A. T. M.	*Norfolk.*
Corbin, G. Lane	*Half-way House, York Co.*
Cox, Thomas E.	*Richmond.*
Creigh, Thomas	*Louisburg.*
Craghead, W. G.	*Danville.*
Cunningham, Jno. A.	*Richmond.*
Dove, James	*do.*
Dove, Jno.	*do.*
Faunt Le Roy, John	*White Post, Clarke Co.*
Frissell, John	*Wheeling.*
Gibson, Charles Bell	*Richmond.*
Gooch, P. Claiborne	*do.*
Hancock, F. W.	*do.*
Harrison, E. J.	*Cartersville P. O., Cumberland.*
Haskin, U. D.	*Richmond.*
Haxall, Robert W.	*do.*
Hildreth, E. A.	*Wheeling.*
Houston, M. H.	*do.*
Howard, Henry	*University of Virginia.*
Johnson, Carter P.	*Richmond.*

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Johnson, James	*Hicksford.*
Joynes, Levin S.	*Accomack C. H.*
Kemper, Charles R.	*Woodville, Rappahannock Co.*
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Wortham, A. G.	*do.*
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McKee, W. L.	*Morgantown, Burke Co.*
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Bullock, W. Gaston	*Savannah.*
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Eve, Joseph A.	*do.*
Eve, Paul F.	*do.*
Ford, L. D.	*do.*
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Freeman, D. L.	*Floydsburg.*
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Lincoln, J.	*Louisville.*
Miller, Henry	*do.*
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Ray, L. G.	*Paris, Bourbon Co.*
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Smith, Samuel M.	*Columbus.*
Smith, S. Hanbury	*do.*
Steele, H. K.	*Dayton.*
Stevens, Joshua	*Lebanon.*
Strader, D. P.	*Cincinnati.*
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Taylor, James	*do.*
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